

Interactive comment on “High resolution NO₂ remote sensing from the Airborne Prism EXperiment (APEX) imaging spectrometer” by C. Popp et al.

Anonymous Referee #1

Received and published: 23 May 2012

In this paper, the authors report on a new and exciting application of the airborne APEX instrument, namely imaging of tropospheric NO₂ columns at unprecedented spatial resolution over an extended area around Zurich, Switzerland. They briefly describe the instrument, measurements, data analysis, and uncertainties, and then compare measurements taken at two different times of the day over the Zurich area. The observations provide a highly structured spatial map of tropospheric NO₂, with good consistency between the two observation periods but lower values and less structures in the afternoon. The APEX data are then compared to a high resolution emission inventory, an annual model average and surface in-situ observations with overall good

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consistency.

The paper reports on a very interesting new measurement method and fits well into the scope of AMT. It is well written, provides all the information necessary and shows some interesting results. I therefore recommend publication in AMT after minor revisions and consideration of the points made below.

1. One aspect of the paper which is not convincing to me is the comparison with in situ data. I have two concerns about this comparison:

First, as the authors point out several times, the two data sets represent different quantities, the surface concentration and the column. While there often is a linear relation between the two, we should not expect this relation to be constant over time, in particular if the BL depth changes as much as the authors report for this case. In very first approximation, one would expect the ratio between column and surface concentration to increase by a factor of 2 – 4 if the BL height increases by a factor of 4. Therefore, it doesn't make much sense to include both the morning and afternoon measurements in the same graph (Figure 12) or linear fit.

Secondly, if we look at the two data sets independently, the correlation looks acceptable in the afternoon but in the morning, 8 out of 9 APEX measurements give the same value within uncertainties while the surface observations vary by a factor of 4. To me, this is not a strong confirmation of the ability of APEX to “observe spatial NO₂ gradients”.

I'm actually not too concerned about this mismatch as the spatial pattern of the measurements looks very reasonable and there are many uncertainties in the in-situ measurements and the comparison of these two different quantities. However, I think the authors should re-consider the discussion and interpretation of this comparison.

2. The comparison between morning and afternoon data is also affected by the

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interpretation of the in situ data. The authors use these as confirmation for strong NO₂ decrease between the two time periods. However, as in the same time the BL depth increases so much, the situation is not as straight forward. Also, while higher wind speeds and stronger vertical mixing will reduce spatial gradients, the reduced lifetime (more OH) will counteract this effect and in general I would expect larger spatial gradients at high sun.

3. There is one aspect which the authors only touch upon but which in my opinion is very relevant for their study and future applications of it. As these data represent a large step forward in terms of spatial resolution, spatial representativeness problems already discussed for satellite observations will become a significant issue. All the a priori data used (topography, surface spectral reflectance, NO₂ profiles, aerosol profiles) are needed at the resolution of the measurements as otherwise large errors can be introduced to individual data points. The authors did a good job on topography and surface reflectance but could not use equally well resolved profiles of NO₂ and aerosols. As all quantities, surface reflectance, NO₂ emission strength, NO₂ profile, and aerosol profile are expected to be highly correlated on small spatial scales, this is a relevant aspect which should be discussed in the paper. Further complication is added by the fact that at a flight altitude of 5 km and a spatial resolution of the order of 100 m, the independent pixel approximation used in the data analysis might no longer hold and three-dimensional effects of the radiative transfer might have to be considered.
4. Because the AMF is varying with location, it would be good to add a figure like Fig. 7 showing for either the morning or the afternoon overpasses the DSC, the AMF and the VC to show which of the variability is due to surface reflectance and which is in the raw data.
5. The fitting window selected is affected by O₃ and H₂O absorption features, but neither of the two interfering species appears to be included in the fit. While O₃

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might cancel as spatial gradients are weak and the tropospheric absorption is small, this is not true for H₂O. Please discuss the reasons for excluding these absorbers and why that's acceptable.

6. What is the rationale of using a polynomial in the destriping? I don't see any justification for this and would suggest using the assumption that NO₂ on average does not depend on scan line.
7. Another aspect related to destriping is the question related to the consistency between different overpasses of the same location. The authors state that the consistency is good and that differences can be explained by transport. While I agree that there are no large discontinuities, there seems to be a gradient in each of the overpasses and I think it would be very interesting to show a direct comparison of the NO₂ results for the overlapping parts of the measurements (which now are hidden in the figure), for example as a line plot.
8. The discussion of the effect of the reference spectra taken over a "clean" region is in part misleading. While it is true that the stratospheric contribution will cancel if time is short enough as suggested in eqs. 4 and 5, the same holds for the tropospheric part above the aircraft. Only *variations* in the free troposphere will contribute to the retrieved DSCDs, and considering the lifetime of NO₂ in these altitudes as well as average wind speeds, there should be relatively little variability. This has an impact on the interpretation of the data (they are more weighted to the BL) and on comparison of measurements taken at different times of the day.
9. In Fig. 8, it would have been nicer to have more similar colour scales – as the figures are now, the extreme values in the two panels are represented opposing ends of the colour scale.
10. It would be good if Fig. 11 could have better image quality – it is difficult to see

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the plume and other details in the RGB images

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 2449, 2012.

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