

Interactive
Comment

Interactive comment on “A wide field-of-view imaging DOAS instrument for continuous trace gas mapping from aircraft” by A. Schönhardt et al.

Anonymous Referee #2

Received and published: 23 April 2014

1 general impression

The authors describe a new airborne imaging DOAS instrument and two different test scenarios under high and low pollution levels. The presented images show no striping effect, and even in the low pollution case the NO₂ SC image shows no instrumental artefacts. The data observed downwind of a power station in Ibbenbüren are used to estimate the flux and the source strength. At 6 km distance from the stack an emission flux around 0.7×10^{24} molec/s was found, with a slight increase towards higher distances. However, even though overpass flights over the plume closer to the stack were performed no emissions are estimated here.

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A similar instrument was developed in parallel by General et al., 2014 (<http://www.atmos-meas-tech-discuss.net/7/2187/2014/amtd-7-2187-2014.html>) which shows the scientific interest in the described observation approach.

The paper is well structured and describes the instrument and the applications very well. After addressing three major points and several smaller ones, I recommend publication.

2 major points

- The plume emissions are performed for a distance of 6 km to the stack, which is probably good to estimate emission correctly. On the other hand this instruments offers the possibility to study the chemical processes on a small scale (30 × 30 m). For this purpose a more detailed discussion of the observation close to the stack would be of interest.
- In section 7.4 the conversion of NO to NO₂ is described as limitation of the flux estimate, is it possible that this "limitation" might be used to gain additional information about the mixing in of O₃ and the conversion mechanism as in Louban et al., 2009 (DOI 10.1007/s00445-008-0262-6) for BrO?
- Although the authors demonstrate very well the instruments ability to detect a low level NO₂ enhancement above a motorway the uncertainty seems quite high (section 9). Therefore I am not sure how useful the comparison to ground based observation really is, especially as these were not performed simultaneously at the same road. Hence the emission should only be compared with respect to the order of magnitude for a medium sized highway (55 000 cars/day). The authors might emphasis this difference more clearly.

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3 smaller changes and technical points

- p 3596 l 15-21: I am not sure if Wang et al., 2005 is a proper reference for the description of the spectrograph, maybe Wang et al., 2006 or Bruns 2004 (PhD-thesis) are better references. I was a bit confused about the small wavelength interval detected by the instrument. Is it caused by the smaller detector size compared to Wang et al., 2006? In Wang et al., 2006 a wavelength range of ≈ 140 nm is given while here the total range is only 41 nm.
- p 3596 f l 26 - 2: The instrument includes a 200 μm fibre to illuminate a 100 μm entrance slit. It might be an idea to use a 100 μm fibre and omit the entrance slit. The risk of illumination the slit with half of the fibre is quite high i.e. misalignment of the fibre by 100 μm and this might cause an unpleasant slit function.
- p 3597 l 15 -25: In this section the readout time and shift time are compared. Please include the typical exposure time here as well. In line 23 on the following page it is said to be 0.5 sec. How is it determined, by the intensity of the previous measurement or is it fixed? Is there any risk that the 0.1 sec for readout are too long for the illumination of next spectra, e.g. when flying over bright clouds or snow. Thereby the spectra would be oversaturated.
- p 3903 l 20: I guess the authors used their “own” SCIAMACHY data available at <http://www.iup.uni-bremen.de/sciamachy/> please add the respective reference or link to the data. Even though they were provided by one of the co-authors.
- p 3604 l 20: Thank you for following my suggestion to extend the time series in figure 6. Please update the start and end times in the text as well.
- p 3604 l 27: Please mention the time of the fit shown in figure 7 in the text as well.

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- p 3606 l 16: Just a comment: The power stations discussed in Heue et al., 2008 have about 5 times the electrical output (≈ 4100 MW) compared to Ibbenbüren (≈ 800 MW), this corresponds quite well with the ratios of the SCs.

Also for the power station in Monticello (Texas, USA) (≈ 2000 MW) studied by Melamed et al., (2003) vertical column densities up to 8×10^{16} molec/cm² were observed.

- p 3607 eq. 7: Is this approximation really faster than doing simulation for nine viewing directions? (not including the roll angle of the aircraft) The influence will be small so there is no need to redo all the calculations. Aerosols are not yet included in the calculations, do the O₄ images show any features comparable to the intensity or the NO₂ images?
- p 3608 l 20-27: This section is slightly confusing. If I understand it correct you have two different effects: On the one hand NO is converted to NO₂, thereby the NO₂ SC increase on the other hand, the plume broadens thereby the SC decrease if you are further away from the source. However what really matters is the the integrated VC along the flight, and according to figure 17 it increases with increasing distance to the stack.
- p 3610 l 11 f: If the mixing layer height was 1300 m and the flight altitude was 1100 m, the aircraft might have been flying through the plume. Was any in situ NO_x instruments aboard? I am not sure if the geometric height of the stack is sufficient here, often the plume rises vertically directly at the beginning. Is this considered in the Gaussian dispersion?

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4 references

- Pundt, I., ... Losch, J., ..., doi 10.1016/j.atmosenv.2004.07.035, 2005. 3617, 3618

change Losch to Lösch

5 figures

- please zoom in a bit more on figure 12? (comparable to figure 14) Because it is difficult to find any differences between LOS 9 and LOS 35. It is the improvement in the resolution, what the readers are interested in.
- Is figure 13 necessary?
- What is the resolution of figure 14 - LOS 35?
- figure 18 is it useful to change the colour scale for the NO₂ plot to a maximum close to 1×10^{15} molec/cm²? So the weak signals become more visible, but also the noise.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 3591, 2014.

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