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Atmospheric Measurement Techniques Discussions



# *Interactive comment on* "DOAS measurements of $NO_2$ from an ultralight aircraft during the Earth Challenge expedition" *by* A. Merlaud et al.

**A. Merlaud et al.**

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We thank the first referee for his positive review and useful comments which help to improve the paper. The manuscript has been English-corrected by a native English speaker who was added in the acknowledgements.

"Instrumental description: Here some information to better understand the performance of the instrument is missing. Since a comparable thick ïňĄbre is used (400$\mu$): what about the sensitivity to polarization? "

The sensibility to polarization in our fitting window was tested in the lab with a Wollaston prism and was found to be inferior to 4 per mil with our 5m fiber. This was added in the instrument description. We put in supplement the figure showing the differential structure of the polarization sensitivity.

“It would be nice to see the slit function of the instrument in the NO2 ïňĄtting region. “

We fit the slit function during the DOAS analysis but we also measured it in the lab with a HgCd lamp. A figure showing the two slit functions was added in the paper.

“Please give here already some information on the possible spatial resolution. The averaging time for the spectra is 5 s. How this translates to the spatial resolution of about 5 km (given in section 6, Conclusions (!)).”

Considering the speed of the aircraft and the measurement frequency, the spectra are recorded over 150m. But the horizontal resolution is much more limited by the limb geometry, which averages the column in the horizontal direction according to the AMF and the BLH. This was added in the Section presenting the radiative transfer.

“The spectrometer is not temperature stabilized, correct? I would expect a lot of short-term changes of the temperature during a single ïňĆight with the ultralight. What is the typical wavelength shift of the spectrometer for a single ïňĆight? How this is going to affect the results?”

The spectrometer is indeed not temperature stabilized, but the shift is also fitted in the DOAS analysis. The shift typical variation during a flight is 0.2nm. This was added in the text.

“Spectral analysis and NO2 column retrieval: Please shorten this paragraph. There is no need to introduce the Beer-Lambert law!”

We have reformulated the paragraph but it seems to us relevant for the sake of clarity to include the DOAS equation coming from the Beer-Lambert law.

“ But: it is not clear to me how the I_ref is usually chosen. It is not always possible to select a spectrum with very low absorption of the trace gas of interest from the same ïňĆight. Again: what is the overall stability of the instrument."

Contrary to local measurements in a polluted zone, our measurements present the advantage to cross very clean zones (see Arabian Desert in Fig 11). So we selected, as it is already described in section 3.2.1, spectra of reference in these zones based on preliminary DSCD series with more randomly chosen reference spectra. In areas where it is less obvious that not any tropospheric NO2 is present, we checked by comparing the DSCD relative to a spectrum of the flight with the DSCD relative to clean reference spectrum taken above desert.

"In table 2 the ozone cross section is missing." Added.

"Air mass factor calculation: I'm a bit sceptical about the assumption of a well-mixed layer for both NO2 and aerosol. This might be the case in rural or better sub-urban areas, but in regions with many local sources one would expect a NO2 peak close to the surface (e.g. Elsa Dieudonne, Analyse multi-instrumentale de l'inïňĆuence de la variabilité de la hauteur de couche limite sur la distribution verticale des oxydes d'azotes en région parisienne, 2012). For the aerosol the layer is never block-shape and in particular for the viewing-geometry like in this study it is quite crucial if the ïňĆight altitude is a bit below or above the main aerosol bulk. "

In the areas where we made the measurements, except for the Po Valley, very few measurements have been performed so far, especially about the vertical distributions. We prefer thus to use simple box profiles, knowing their limitations, than more sophisticated inputs from global models whose accuracy could be overestimated due to the lack of validation data. Considering the NO2 profiles, we refer in the paper to Heland et al., 2002 and Boersma et al 2009, which have used successfully our assumption for satellite validation studies. We also refer to Volten 2009, which has measured NO2 profile in Holland and found a constant concentration in the boundary layer. We added a reference to Dieudonné 2012 saying that in urban areas the assumption is not valid, but our measurements were not taken directly above urban areas because it is not al-



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5, C1445–C1450, 2012

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lowed for a small aircraft to fly directly above a city. Concerning the aerosol extinction shape, we already write in the paper that it is less step-like. We also give references (Landulfo et al.2003, Guibert et al. 2005) to measurements in polluted zones, which shows that even if the profile is not block shaped, most of the extinction occurs in the boundary layer. Our way to calculate the visibility, if obviously approximate, is used in NOAA-NESDIS ATBD, 2010. However, we agree that an accurate knowledge of the extinction profile would increase the accuracy of the instrument. Small lidar have been installed on lidar already (Chazette, 2007), both instruments could be used simultaneously. We added this suggestion in the conclusion.

"What is the reason that the authors did not use the O4 as an indicator for the visibility? I would expect in particularfor regions with high pollution very inhomogenous viewing conditions (like e.g. Riyadh)."

Contrary to NO2 for which it is possible to find zones with negligible tropospheric concentration, O4 is ubiquitous in the troposphere since it is the square of oxygen concentration. And contrary to a previous experiment (Merlaud et al, ACP, 2011) the small aircraft could not fly high enough to be insensitive to aerosol extinction in the lower troposphere, not to mention the large field of view. So in the DSCD of O4 the information content is not sufficient to be quantitative on the visibility, even if O4 DSCDs can be used qualitatively to isolate cloudy areas. We show that in the new figure 10. Sensitivity studies and error analysis Assumed uncertainties of 300m for the boundary layer height and 0.1 for the AOD seem to be quite optimistic (see also my comments above about the aerosol). Was the boundary layer always fully developed during the ïňĆights? One would expect a rapid change of the BLH in the ïňĄrst hours after sunrise. Considering the sza (see table 3, maximum is for the Po Valley at 67°) and time of flight we consider it rather safe to assume a well-developed boundary layer. Regarding the uncertainties on the ECMWF BLH, it originates from published studies which are already mentioned in our paper ((Kittaka et al., 2011, Palm et al., 2005).

"Results Which satellite data product has been used for the comparison?" We have added the two products with references.

"It would be really helpful for the interpretation of the data to add more details to table 3: Name, coordinates, ïňĆight distance, ïňĆight time, sza range." We have added the solar zenith and azimuth angles to the table. Now the main parameters to calculate the air mass factor, which is also given, are available on the same table. Unfortunately adding more fields leads to splitting the table in two which we don t think is worth, for the sake of clarity. But we added the requested information under the figures and in the text.

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Interactive comment on Atmos. Meas. Tech. Discuss., 5, 1947, 2012.



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5, C1445–C1450, 2012

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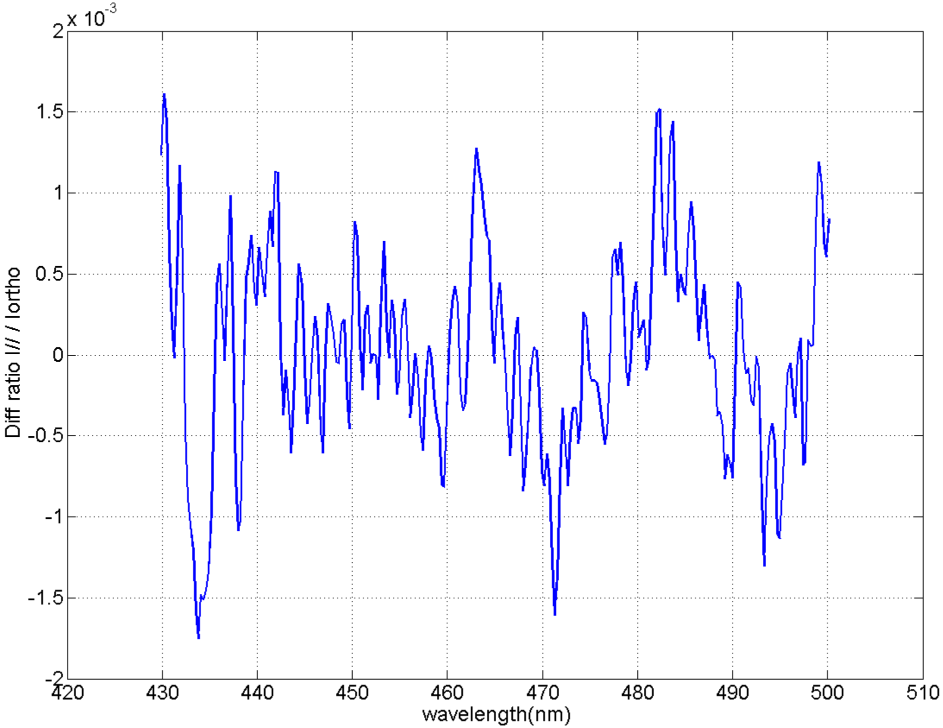


**Fig. 1.**



AMTD
5, C1445–C1450, 2012

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