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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.

Anonymous Referee #1

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The paper by Merlaud et al. describes measurements of tropospheric NO2 columns using a new airborne DOAS instrument which has been optimized in terms of power consumption, automation and size to ensure the operation on an ultralight aircraft. The paper describes some new ideas which have the potential to further improve this technique in particular in terms of low-cost deployment on small airplanes. Furthermore retrieved NO2 data have been compared to satellite observations, some of them for regions with very sparse knowledge so far. In general the paper is well written and an interesting piece of work and therefore merits publication in AMT. However, I have some points to be addressed by the authors.

As referee #2 I recommend checking the manuscript with the help of a native speaker.

Instrumental description: Here some information to better understand the performance of the instrument is missing. Since a comparable thick fibre is used (400μ): what about the sensitivity to polarization? It would be nice to see the slit function of the instrument in the NO2 fitting region. 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 (!)). The spectrometer is not temperature stabilized, correct? I would expect a lot of short-term changes of the temperature during a single flight with the ultralight. What is the typical wavelength shift of the spectrometer for a single flight? How this is going to affect the results?

Spectral analysis and NO2 column retrieval: Please shorten this paragraph. There is no need to introduce 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 flight. Again: what is the overall stability of the instrument. In table 2 the ozone cross section is missing.

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'influence 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 flight altitude is a bit below or above the main aerosol bulk. What is the reason that the authors did not use the O4 as an indicator for the visibility? I would expect in particular for regions with high pollution very inhomogenous viewing conditions (like e.g. Riyadh).

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 flights? One would expect a rapid change of the BLH in the first hours after sunrise.

Results Which satellite data product has been used for the comparison?

It would be really helpful for the interpretation of the data to add more details to table 3: Name, coordinates, flight distance, flight time, sza range.

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