Atmos. Meas. Tech. Discuss., 8, C1759–C1765, 2015
www.atmos-meas-tech-discuss.net/8/C1759/2015/

Atmospheric Measurement Techniques Discussions

Interactive comment on "High-resolution measurements from the airborne Atmospheric Nitrogen Dioxide Imager (ANDI)" *by* J. P. Lawrence et al.

Anonymous Referee #1

Received and published: 30 June 2015

1 general impression

The paper "High-resolution measurements from the airborne Atmospheric Nitrogen Dioxide Imager (ANDI)" by Lawrence et al. describes a first demonstrator application of an interesting instrument. The idea to map the trace gas distribution with a high resolution from an airborne instrument is currently fascinating several scientific groups, as can be seen by the references in the introduction. The approach is developing parallel to high resolution modeling of the trace gas distribution in cities.

The paper is well written and structured, but there is one point that unfortunately is the

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

basis of the manuscript and should be clarified:

On page 5692 the error caused by the DOAS fit is given as 7×10^{15}, this means the DOAS fit error is roughly three time higher or close to 2×10^{16} molec/cm^2, this seems quite high compared to similar applications (Schönhard et al.: 0.2e16 or Popp et al., 0.8e16). Eventhough for a differential measurement like this it is not such important but still it should be mentioned, that the retrieval error is of comparable magnitude as the observed SCD (Figure 7) (2×10^{16} molec/cm^2), averaged over most of the measurements. Does the DOAS fit improve if the authors partly sacrifice the spatial resolution? The current spatial resolution is very good, but even if it was 40 m, it would still be good enough to resolve a lot of details. It might be checked if the dSCD are similar for low resolution data and the error is reduced, if so the high resolution data have a better basis.

Concerning the DOAS fit: to omit the offset correction in the DOAS analysis (P 5686) can not be recommended, especially not when working with an imaging spectrograph in combination with a CCD. According to Whyte et al. the stray light within the spectrograph is not too big ($\leq$ 0.5 % at 430 nm) please add few comments about the influence of the fitted offset.

2 minor changes

- P 5678 L 9 and 5679 L 28 what is the resolution of the instrument (20 m or 5 m)? Both numbers are mentioned in the manuscript and dividing 600 m by 128 pixels results in 5 m. Here a binning of 4 CCD lines is already included.
- The general problem of all the airborne DOAS instruments is that one measures relative to an unknown background. There were some attempts to overcome this problem, comparison with satellite instruments or modeled data, but the general uncertainty remains. Therefore the authors decide not to correct for the unknown

AMTD
8, C1759–C1765, 2015

VCD in the reference region (page 5691 and 5692). This is of course a possible solution, but it has to be considered through out the paper, and direct comparisons and relations between VCDs should be avoided, because they will certainly change if an offset of $1 - 8 \times 10^{15}$ molec/cm^2 (satellite data from the respective region for February 2013) is added. So please do not set the results for the respective regions in relation, even if the error might be small. The total numbers speak for themselves, add a comment form time to time to remind the reader of the background problem.
(P 5694 L 20) just give the observed average VCD for the city
(P5696 Ls15-22)

- The spectral resolution of the instrument is 1.5 nm. However, in the paper by Whyte et al. (2009) it seems better spectral resolutions are possible with the instrument. Did the authors use a wider entrance slit to improve the light through put and hence spectral intensity?

- The authors made a very good error estimate in section 3.7, and the previous sections, but before coming to the results of the error analysis they discuss the results of the measurements. Therefore they have to repeat the main uncertainties later on, why not combing the sections 3.7 and 4.6 in either of these?

- The frame transfer camera worked with 1 Hz, this means the data are recorded for roughly 1 second, (e.g. 0.99 s) transferred to the readout chip within a few milliseconds and read out for about 1 second while the next image is taken. If this is correct this should be considered in the figures and the interpretation of the data, because it means the data are averages of 80 m and not point observations. In figure 4 it looks like point measurements, which is of course easier to plot. Add a comment in the caption that the centre is taken here. After the temporal shift correction and the interpolation this should be very similar. Does the time stamp of the measurements belong to the beginning or to the end of the measurements?

AMTD
8, C1759–C1765, 2015

This might explain parts of the temporal shift.

- P 5685 Ls 15-25 Was the temporal correction performed with the interpolated data or with the raw data? Here it seems the interpolated data were used (compare to the previous comment on the camera). Is a constant shift assumed for the complete flight or are some variations allowed, caused by changes in the pitch angle, uncertainties of the time stamps of both image and position data.
- P5687 L10 Plumes should not be included in the averages used for the destriping polynomial, therefore it's critical to include the measurements over Leicester city, however omitting the city data will reduce the statistical basis of the destriping.
- P5701 Ls 8-16 I am not sure the authors are right with the assumption that the enhanced NO_2 columns observed in this region are caused by a small farm machinery factory. Can transport be excluded? I suggest to skip the last half of this paragraph L13 " A possible reason" . . . to the end. To me this seems to be highly speculative.
- P 5704 L 1-3 Here the authors compare concentrations despite the facts that the background column is unknown and that the profile has a significant influence on the concentration, especially for elevated plumes like the one from the power station in Ratcliffe-on-Soar. (see P5698 Ls 1-4)

3 technical correction

- In the paper some numbers seem to be unrealistic, I have to apologize that I did not realize that during the quick review, please double check all the numbers:
  - P 5689 L 16: "mean surface height of 0.948 km" either the authors mean something different e.g. flight altitude above ground or this is just an over-

AMTD
8, C1759–C1765, 2015

sight, because the mean surface altitude around Leicester (UK) is 76 m (0.08 km). For comparison the highest mountain in England (Scafell Pike) is 0.97 km. I assume the correct number was considered in the AMF calculation, otherwise in figure 8 the detector would be at ground level.

  - P 5690 L1 “Lat: 58.7814° Long:-1.2844° ” There must be an error in the latitude. Is it 52.7814°? The station is mentioned to be just outside the city (p 5698 l 27), but the latitude is about 450 km north of Leicester 52.6° N)
  - P 5694 Ls 6/7: “an estimated error of approximately 0.02 . . . an uncertainty of 20%.” Relative to the albedo of 0.12 this correct, but it might be misleading in this context, especially as the albedo is defined as relative number.

- P 5681 L8: Lieght et al. is in preparation, please check the author guidelines whether a comment like “in prep.”, should be added in the text.
- P 5683 L 12 please add that the instrumental failure was close to the power station Ratcliffe-on-Soar (That was very bad luck)
- P 5684 Equation 2: In theory the pitch angle of the plane should be included here, but this is unknown (failure of the IMU) and probably constant through out the flight, but a varying pitch angle might cause some small difference in the temporal synchronization. A pitch angle of 2° causes 30 m difference on the ground.
- P 5687 L 3 Please add a marker in the map (figure 14) where about the reference was taken.
- P 5698 Ls 1-4: move these lines to the section about Ratcliffe-on-Soar (P5697 L14). Is the effect of the plume height, included in the AMF calculation?
- P 5697 L14 start a new section “regional summary” ?
- P 5698 L 27 see above latitude of the station

AMTD
8, C1759–C1765, 2015

4 tables

- Table 2 Typo O in H_2O
- Is Table 5 necessary or can it be combined with Table 4

5 figures

- Figure 1: Is it necessary? The main components and are described in section 2. Only that two computers are used to store the data of the IMU/GPS and the spectrometer is not mentioned explicitly in the text.
- Figures 4, 5 and 10 and 12 show city maps of Leicester with an overlay of different data. For a better comparability it would be nice to have “an approximate” scale
  Figures 4 and 5 can the authors use the same part of the city. Because in Figure 4 the intensity at the stadium (King Power) seems not to match, in Figure 5 (corrected) this can not be checked.
  Figure 5: There are some weak stripes in the intensity map, which of course can later be found in the AMF and the VCD. Also the intensity data were destriped but it seems some stripes remained e.g north of the “G” of “Google” . Does it matter whether the aircraft headed south (towards the sun) or north.
- Figure 9: The stratospheric NO_2 is not that relevant for the described application (of course it can not be omitted) but a more detailed figure about the troposphere is expected here, just add a reference to figure 11.
- Figure 11: Add the flight altitude (900 m) and a reference for black in the figure caption e.g. "default". May be the profile between 1000 and 2000 m can be omitted?

AMTD
8, C1759–C1765, 2015

- Figure 14: please mark the reference region and the regions 1 and 2 as well
- Figure 14 and 15: there are some single “scans” just downwind of the power station. What is the reason for these individual scans? It looks the instrument failed was restarted and failed again after one scan. The data look realistic or are they highly uncertain due to instrumental failure?

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 5677, 2015.