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Atmospheric Measurement Techniques
Discussions

Interactive comment on "High resolution mapping of the NO_2 spatial distribution over Belgian urban areas based on airborne APEX remote sensing" *by* Frederik Tack et al.

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General comments

The paper describes the retrieval of high resolution NO2 maps from the airborne imaging spectrometer APEX over three cities in Belgium (Antwerp, Brussels and Liege). The authors develop a new retrieval algorithm for APEX similar to the algorithm described by Popp et al. (2012) but with several improvements such as better characterization of the influence of spatial binning and reference NO2 VCD on the retrieval as well as a correction term for in-flight changes of spectral resolution ("resolution cross section"). They also estimate VCD errors, which were not included in the paper by Popp et al.,

and compared their measurements with car mobile-DOAS measurements. The authors conclude that an airborne imager can be used for studying the spatial distribution of NO2 in urban areas.

The authors present new ideas, tools and data and their conclusions are mostly substantial. The topic is well within the scope of AMT. The scientific methods are valid and mostly clearly described. Some revisions are required, in particular, in Sections 3.1 and 3.2 which contain some small inaccuracies and/or not fully understandable results (see special comments). The paper is generally well written and the trail of ideas is understandable, but additional copy-editing is necessary.

Special comments

Section 1 (Introduction)

The authors state that one objective is to access APEX's capabilities for mapping NO2 in a "cost-effective way" (p. 2, l. 13ff). However, the cost effectiveness of APEX is not discussed in the paper. Thus, I suggest revising the objective or adding a section/paragraph on the cost effectiveness of APEX.

Section 3.1

The authors describe the data as "time series" (p. 7, l. 4f and Fig. 4), but since the spatial dimension should be more important than temporal dimensions, I think a better term would be "along-track profile" as used in Fig. 14.

The description of the smoothing effect and conclusion from this analysis are difficult to understand (p. 7, l. 6ff and Fig. 5). The authors should consider showing the raw data used for the fits in Fig. 5 and mark the location of the x-axis of Fig. 5 in Fig. 14.

Section 3.2

Paragraphs 3 and 4 need to be revised, because the conclusions are based on a wrong premise. APEX has no absolute pressure stabilization but is kept at 200 hPa above ambient pressure (Kuhlmann et al. 2016). Thus, CW positions depend on the ambient pressure at flight altitude during data acquisition and spectral shifts can occur between different campaigns. I think this is the most likely explanation for the differences found by the authors between the three datasets such as the shift between Antwerp and Brussels which were not flown on the same day.

Furthermore, the CW accuracy of 0.2 nm does not include the spectral smile, which is defined as the range of wavelength shifts in across-track direction. Schaepman et al. (2015) state the spectral smile is smaller than 0.35 px (px = nm smile per nm nominal FWHM) which is about 0.5 nm at 490 nm. This is in good agreement with the spectral smile (about 0.4 nm and not 0.8 nm) the authors show in Figure 6b.

APEX's in-flight spectral calibration of is described by D'Odorico et al. (2011) and Kuhlmann et al. (2016). The authors should consider citing these papers:

D'Odorico et al.: Performance assessment of onboard and scene-based methods for Airborne Prism Experiment spectral characterization, doi: https://doi.org/10.1364/AO.50.004755, 2011.

Kuhlmann et al.: An Algorithm for In-Flight Spectral Calibration of Imaging Spectrometers, doi: https://doi.org/10.3390/rs8121017, 2016.

Section 4

Since a "synthetic resolution cross section" (p. 9, l. 23) is not commonly used in DOAS fits, the authors should provide additional explanations or a reference.

While the differential approach reduces the impact of systematic instrumental stabilities and Fraunhofer lines, it does not necessarily neutralize them (p. 10, l. 10).

The authors state that spatial resolution of the NO2 map is 60x80m2. However, for visualization the maps are spatially smoothed by a Savitzky-Golay filter (p. 14, l. 9f). The authors should add sentence explaining how much this additional smoothing reduces the effective resolution of the NO2 map.

The DOAS fitting error is not a random error, because it also contains systematic errors, e.g. from the parametrization (p. 15, l. 5).

The authors should add the magnitude of the reference VCD used for the three campaigns.

Section 5/6

I think the discussion of the results need additional data for supporting the conclusions. The authors should consider providing a time series of NO2 concentrations for the three sites. These data would help to interpret the bias expected between adjacent flight lines (p. 17, l. 1f) and the difference between the cities (p. 18, l. 5ff). In particular, the lower NO2 levels in Brussels might be also explainable by synoptic differences and the diurnal NO2 cycle (Antwerp was flown 11 LT and Brussels at 15 LT). In addition, additional labels on maps would help to follow the explanations (Figs. 14, 16 and 17). An NO2 time series would also help interpreting the bias between APEX and mobile-DOAS measurements (p. 20, l. 12ff).

The authors might consider adding the route of the mobile measurements in Figures 14 and 16. The results for Liege are not shown and only briefly discussed in the paper with the reasoning that the results are similar to the other findings (p. 17, l. 19ff). However, since the Liege dataset is mentioned throughout the paper, I think the NO2 map for Liege should be added to the paper or the supplement for completeness.

Section 7

The authors state that APEX can expose fine-scale structures in plumes (p. 21, l. 7). I think this should be specified to “city plume” or “urban plume”, because fine-scale structures in stack plumes are not visible in the maps. The authors should add the information compared to what the geolocation accuracy is superior (p. 21, l. 10).

Table 5

The authors should consider splitting Table 5 in two tables, because the headings for the last three rows (NO2 profile) seem not to match with the table content. In result, the quite long caption would be shorter, as well.

Table 6

I think the last column is probably showing the mean or median error for all VCDs and not the error of the total VCD.

Technical corrections

p. 4, l. 18: “bulk” -> “majority”?

p. 7, l. 1f: What is the column co-located with?

p. 13, l. 3: “compensation factor” -> “enhancement factor” or “air mass factor”

p. 17, l. 20: 11u30 -> 11:30

p. 20, l. 24: It is not clear what is meant by: “averaged by 2 subsequent retrievals“.

p. 20, l. 29: “Gerrit Kuhlman” -> “Gerrit Kuhlmann”

Table 1: I would suggest using “pixels” instead of “detectors” in the second row.

Table 2: “Date” -> “Date (day of year)” and “0-180” -> “0, 180” in row flight pattern

Table 4: Is “slope” a polynomial of degree 1?

Figure 6: “performed at” -> “shown for”?

Figure 15/18: X-axis label: “Dec. time” -> “decimal time”?

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