Atmos. Meas. Tech. Discuss., referee comment RC2
https://doi.org/10.5194/amt-2021-303-RC2, 2021


# Comment on amt-2021-303

Anonymous Referee #1

Referee comment on "Mapping the spatial distribution of $NO_2$ with in situ and remote sensing instruments during the Munich $NO_2$ imaging campaign" by Gerrit Kuhlmann et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-303-RC2, 2021

General comments

This manuscript mainly analyzes data taken on the single day of 7 July 2016 during the MuNIC campaign to investigate the NO2 VCD spatial distribution field from an airborne imaging spectrometer (APEX) and evaluate its validity by comparisons with other observations such as mobile MAX-DOAS observations. Then, an attempt is made to estimate NOx emissions from combined heat and power plants. Since the accurate estimate of NOx emissions is highly demanded, the subject is appropriate for AMT. However, uncertainty in the critical parameters discussed (such as the APEX NO2 VCD values and NOx and CO2 emissions) is potentially too large to appropriately bring new findings and therefore conclusions. In addition, I identified some places that need much more clarification and discussions. After adequately addressing these and other concerns described below, I recommend that this manuscript will be published.

Specific comments
I am concerned that uncertainty in the critical parameters discussed (such as the APEX NO2 VCD and NOx and CO2 emissions) is potentially too large to appropriately bring new findings and therefore conclusions. I think that the authors need more work to quantify those uncertainties more precisely by covering as much sources of uncertainty and, if possible, should attempt to reduce the total uncertainty, in order to draw conclusions through convincing discussion. The details will be given below.

I think that the term "VCD" used throughout this manuscript should be the tropospheric VCD. If so, please correct.

It is convenient for the readers to go through the manuscript without looking at supplement figures. Please consider to re-arrange all figures, including supplement figures.

In the abstract and conclusions, the authors state that "... agree well .., (r=0.55)". Also, it
is stated in the text (L345-346) that "... agree quite well with a moderate correlation coefficient of 0.55." I think that the authors overstate there although a correlation coefficient was only 0.55. In addition to the correlation coefficient, the authors should discuss the difference to evaluate the agreement quantitatively.

The last sentence of the abstract about the epidemiological studies should be removed since almost no relevant discussion has been made in the text.

In the abstract and the "2 Data and method" section, please provide information of latitude and longitude where the MuNIC campaign was conducted.

L90-105: For the stationary MAX-DOAS, please state how the scaling factor of O4 has been treated in the aerosol extinction coefficient profile retrieval.

L103: What does the 1D-layer mean?

L109: Please add a description how to estimate the along-track resolution of 6 m. I guess that the authors have considered the aircraft cruise speed and the integration time of spectrometer but the clarification would be helpful.

L128: For the fitting window of 470-510 nm, H2O and O3 should be included in the DOAS analysis.

L132, Eq 1: Please add an explanation for readers to readily understand this equation.

L171-174: Please discuss the difference between CAPS and CE-DOAS data, in addition to the correlation coefficient.

L211: Please elaborate what the surface hemispheric-conical reflectance factor HCRF.

L230: Please add a description about the physical processes behind this dispersion category, which was estimated as very unstable from wind speed, cloud fraction, and hour of day.

L233-234: I do not think that Figure 2a shows a clear diurnal cycle with a morning peak and an increase in the evening.

L255-258: I could not understand what authors want to say in this paragraph.

L267: How much FWHM values were indicated by in-flight and laboratory calibrations? Please add these info in the text.

L354-355: It was hard to understand why it is essential as input for epidemiological studies.

L374-375 and Figures 9a, e, and h: To confirm that the high APEX $NO_2$ areas were plumes from CHP plants, please add figures of the wind field and its discussion here.

L403-404: How accurate is the determined wind direction? How is the consistency with the wind field?

L417: I cannot evaluate how reasonable the assumed heat emissions of 70MW are.

L426-428: Potentially critical sources of uncertainties, such as heat emission, $NO_2$:NO conversion factor, and residence time, have been missing in the uncertainty estimate here.

L433: "35.6 g $NO_2$ $s^{-1}$" or "55%" is not an uncertainty but just the variation of the average. Please provide the uncertainty estimated considering as much sources of uncertainties as possible.

L464: I wondered that the estimated emissions were higher than those computed from fuel consumption because this work picked up peak values only. Moreover, I am afraid that the estimated emission depends on how peak values picked up.

L466-469: I could not evaluate the sentences. This is because I was skeptical if the discrepancy the authors state was really a meaningful difference while 1) the estimated emissions has potentially a large uncertainty (as discussed at L470-487) and 2) conditions for estimates of reported and estimated emissions were unlikely identical. Moreover, I did not see any reason why the main reason is the unstable and highly turbulent atmospheric boundary layer, while there are other significant error sources (as discussed at L470-487).

L476-477: The authors should estimate the emissions by incorporating minor peaks and add its quantitative discussion. I guess that the authors can identify minor peaks (at least some of them) manually without the algorithm.

L483-486: If the estimation of NOx emission is sensitive, the authors must include sensitivity analyses to the NO2-to-NOx conversion factor and residence time and their quantitative discussion in the manuscript.

L498: To support this argument, please discuss by referring to Irie et al. (2011) , who showed the relationship between the partial VCD (at 0-1 km) and NSCs.

Technical corrections

L1: "NO2" should be defined first. So, "... the Munich NO2 imaging ..." should be "... the Munich nitrogen dioxide (NO2) imaging ..."

L49-50: I had an impression that only the issue that typical urban monitoring network is too small has posed the reliable retrieval of NSCs with airborne remote sensing observations. There should be other issues. Please discuss them also here.

Fig. 1: Please provide information of latitude, longitude, and direction.

L162: Please write what API is.

L191: Please write what KIT is.

L192: Please write what TUM is.

L206: "R.Richter" should be "Richter"

L219: Should "... Munich based ..." be "... Munich is based ..."?

Fig 2: Please re-write the figure caption for readers to readily understand that Fig. 2a is for monitoring stations and Fig. 2b is for LP-DOAS.

Figure 4: Please provide information of latitude, longitude, and direction. Also, please indicate where CHP plants are located.

Figure 6a: Please provide information of latitude, longitude, and direction.

L350: Better to write "inconsistent" instead of "incorrect"?

Figure 8b and L354-363: The unit for the NSC-to-VCD ratio is incorrect. It should have a dimension of $m^{-1}$.

Figures 9b-d, f, g, i-k: It is unclear which figure shows which gray box.

Figure 10b: Please provide information of latitude, longitude, and direction.

L451: Please write what VDI is.