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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Estimating real driving emissions from MAX-DOAS measurements at the A60 motorway near Mainz, Germany" *by* Bianca Lauster et al.

**Anonymous Referee #1**

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The study by Lauster et al. describes a new method to quantify the NOx emissions from a motorway using two MAX-DOAS in parallel. This method is new and complementary to the existing ones, the analysis appears valid, and the presentation of the results in the paper is in general clear, although there is room for improvement in this respect. The experiment also addresses a hot topic regarding air quality. This work fits well in the scope of AMT. Therefore this work should be published, once the authors have taken into account the following remarks.

One limitation of this study is its small database. To my understanding, there was only one day of measurements (10 May 2019). This date appears in the main text only in



section 3 (I know it is in the caption of Fig. 1). It is fine to demonstrate a new technique with a small database, but this should be clear in the text. That means adding the date of experiment to the sentences of the abstract and conclusion which gives the factor 11+/-7. In case the authors performed more of such measurements but could only use those of May 10 for some reasons, it would be interesting to (briefly) explain what the problems were.

It s confusing that the legends indicate 'west side', 'east side' in Fig. 2 and Fig. A1, since they show measurements when both instruments were on the west side to record reference measurements. I suggest to label the instruments e.g. A and B across the text and figures instead (keeping the west side, east side where it makes sense).

l. 125 and below: Can the authors explain why they use the non filtered SCDtraffic estimate in the main text, if they have filtered the clouds in A2? Does the statement that the 'clouds have only a small impact' refer to the 16% of A2? If so, this is more important than the standard error of the mean (5%) and thus not 'a small impact'.

l.177: 'Our simulations with CAABA confirm...' -> The O3 concentration is indeed an important parameter is in the NO2/NO evaluation, one can imagine that the atmospheric mixing is as well. Could the authors add a figure with these simulations, e.g in the appendix? If the NO2/NO ratio is stable in the O3 conditions on 10 May 2019 in Mainz, it is interesting to know in which O3 conditions this ratio is not stable.

l. 188 The authors could refer to previous experiments which indicate that it is unlikely that the NO2/NO equilibrium would be reached so close to a source, e.g. the airborne measurements of NOx fluxes from power plants (see for instance the Phd of A. Meier, Uni. Bremen), or similar studies.

In appendix A1, the statement 'As for cloud free condition a constant CI is expected' is misleading since the CI varies, even without any clouds, with the sun position (see e.g. Gielen, 2014). In practice, this statement is only valid because of the short considered time period, please rephrase.

The last sentence of the appendix 'a constant wind is advantageous for the measurements' is an important take-home message and should be explicit in Sect 3.2 and in the conclusions.

I have several smaller suggestions to improve the presentation, see below.

Minor edits —

l.8: 'independent' -> independently ?

l.13: 'A large fraction of the global emissions' -> can the authors be quantitative on this fraction?

'Therefore'-> does not seem an appropriate adverb here since it s not linked by cause to the previous sentence, what about 'Moreover'?

l.30: 'need to convert NO into NO2 as they directly measure the exhaust plume'-> can the author briefly explain why? (the emissions are mainly NO?). It makes sense to detail also since the reference is in German.

l.55: 'the differential SCD yields the integrated tropo concentration of a specific trace gas'. This seems too short to be accurate. Please specify that the integration is along the photon path and that this quantity is relative (differential) to the column in the reference spectrum.

l.68: perpendicular -> almost perpendicular?

l. 71: 'Possible source of NOx' although that may seem obvious to the authors, I suggest to add that 'since no fire was detected in the area' for other readers

Spectral analysis: presenting the DOAS fit parameters (window, cross-sections, polynomial orders...) in a dedicated table would be more readable and synthetic.

l. 98: 'As can be seen in the grey area' -> actually not much can be seen in the grey area due to the y-axis scale of the lower subplot. I suggest to redo this figure 2, with



the second subplot zoomed in the time period of the grey area so that we really see that the delta is about 4e14. This would also make the subplots less redundant.

In the text, the authors should also explain what this delta is in practice (interpolated? one channel assumed constant?) since the measurements do not appear synchronized in time.

l. 104 'spectra are being integrated' -> '... averaged' ?

l. 285 'as shown in fig.2'-> 'fig. A2'?

l. 292 'in Fig. A3 where the dashed line indicates ... threshold' -> Fig A2 ?

l. 112 It s expected than the error trends follows the RMS, as it is expected that the RMS decreases with increasing integration times. Please add a few words on the physical explanation (shot noise ...)

l. 115-116 'Consequently ... to resolve specific traffic event' -> Please break this sentence in two for the sake of readability

l. 160 For the sake of readability, I suggest to be more explicit with the geometric approximation of the AMF at 20° i.e. to write 2.92.

Figure 4 is important and should be improved. The y axis of pannels A and B should be zoomed to better see the variations and mean values. Pannels E and F are redundant, the authors could only show one of them (leading to larger remaining subplots and a clearer figure).

l. 199 'These emission standards' -> 'the emission standards of trucks' (would be clearer for the reader)

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