This is an interesting paper about first results from the MAD-CAT intercomparison campaign taken place in Mainz, Germany, in summer 2013. Apart from (1) introducing the CU Boulder MAX-DOAS instrument basic points of this study are (2) the retrieval of boundary layer vertical profiles and PBL, (3) near-surface horizontal distributions of NO_2, (4) range resolved NO_2 horizontal distribution measurements using an "onion peeling" approach, and (5) ratios of HCHO-to-NO_2, CHOCHO-to-NO_2 and CHOCHO-to-HCHO in different azimuthal directions. In addition, (6) OMI pixels are validated using the introduced method.

In general, the paper suffers a bit from its many topics. The introduced analysis is applied only for a case study of one day within a whole campaign and not validated with independent measurements. Consequently, the OMI validation comprises only two pixels. Instead of a more comprehensive timeseries, trace gas ratios are presented but not using the introduced method but simply by taking the ratios of slant columns.

However, the demonstration of the onion peeling approach for range resolved trace gas distributions, the measurement strategy for azimuthal distributions of volume mixing ratios as well as the interpretation of trace gas ratios are interesting and a valuable contribution for ground-based DOAS measurements. I therefore recommend publication in AMT after consideration of following points.

General comments:

(1) In the abstract and introduction, the new instrument and 2D-MAX-DOAS are frequently praised as new, innovative technique implying somehow that the 2D-MAX-DOAS technique is invented here. Please go through the text and rephrase carefully. 2D-MAX-DOAS is a well-established technique used by many groups (NASA, Heidelberg, BIRA, Bremen, Washington and many others). Instead, the authors should make the point that they present a measurement strategy which exploits the time consuming 2D-MAX-DOAS very efficiently (i.e., performing profile retrievals in each azimuth direction would require complete elevation scans and thus much more time).

(2) There is no doubt that the authors built a very good instrument, but if the focus is so much on the new instrument (even the title states it) the authors have to point out what the new, innovative aspect is. In principle, no component of the instrument is new, but they are combined and arranged in a different way. For example, Heidelberg and Bremen are doing 2D-MAX-DOAS, Heidelberg even with a rotating prism; BIRA, NASA and Washington can perform direct sun measurements, Washington even with an integrating sphere. I see insufficient references to these instruments. My personal preference was to include a section describing other existing instruments and their capabilities (with references). Then, the authors could highlight advantages of their instrument (maybe stating that they combined the best aspects of existing technologies, which is true to my opinion).

(3) As a comment: There is a lack of independent ground-based measurements (e.g., in situ) to validate the results and the presented approach. This is a pity since Mainz (as any other larger

German city) is performing routinely in situ air quality measurements. There are 6 stations in Mainz which measure at least NO_2 (see link below, unfortunately only in German). Normally, there are some short-term results available online, but these people are often helpful if asked for data.

http://www.luft-rlp.de/aktuell/stationswerte/stickstoffdioxid/index.php

This data would have been well-suited to validate range-resolved NO_2 in different azimuth directions (as shown in Fig. 9).

(4) There is something strange with the fit settings (Tab. 3 and 4). For example, Tab. 4 states that CHOCHO and NO_2 fits in the range of 434-460 nm are using cross sections of BrO, HCHO (UV absorbers), but not CHOCHO itself which would be number 6 according to Tab. 3.

In addition, if the authors include two ozone cross-sections at different temperatures in the CHOCHO fit in the Visible, please shortly explain why this is necessary.

(5) As being based on Sinreich et al. (2013) the authors should include a brief section summarizing this study and the basic idea and findings of the parameterization approach.

In this context, please clarify which preconditions exist for the parameterization approach. Sinreich et al. (2013) state that collapsing of slant columns is a necessary condition. Was this the case for the MAD-CAT campaign? Please provide evidence. If yes, is your approach limited to conditions of very high aerosol scenarios? Please clarify and give recommendations, under which conditions your approach is applicable.

(6) The authors used a profile retrieval to obtain the PBL from measurements in different elevation angles (but only 1 azimuth direction) and then the PBL from this for the parameterization approach, i.e. for retrieving trace gas VMR in different azimuthal directions. This requires a constant PBL in all directions (and all ranges), which would require a flat terrain. Is this the case for the measurement site or are changes of the PBL or the terrain taken into account?

In addition, parameterization approaches are normally well-suited for box-like profiles. While this is likely at the (polluted) measurement location, on the other hand, the authors use a profile retrieval with an exponentially decreasing apriori profile (and the retrieved profiles in Fig. 6 are also not box-shaped) and a factor of 1/2e to determine the boundary layer height which is then used in the parameterization approach for other azimuth directions (and I guess the conversion from VMR to VC in Sect. 4.5 using the PBL is again based on a box-profile assumption?). This is a contradiction.

(7) Figure 8 shows that the main azimuthal viewing direction is very close to Frankfurt Airport which is only about 20 km away (i.e. within the effective radius) and one of Europe's main hubs. Thus, enhanced uplifted layers of NO_2 (and aerosols) are likely in this direction in which the profile retrieval is performed. The MAX-DOAS profile retrieval has normally high sensitivity only close to the ground,

while the retrieved profile in higher altitudes is determined mostly by the apriori profile (which is an exponential decrease in the present study). Thus, the authors should elaborate sensitivity tests for uplifted NO_2 and possible impacts on their retrieval.

(8) How does the retrieved profile (from optimal estimation) in the main azimuthal direction look in comparison to the VMR_{NO2} calculated from Eq. 5 with f_c from Eq. 6 (where the PBL from profiling is needed for) in the same azimuthal direction? Are the results consistent? Please give an example plot of this and a short comment (I guess one would expect that VMR_NO_2 from Eq. 5 and 6 is some average of the profile in the lowest layers?).

(9) The trace gas ratios (CHOCHO-to-HCHO etc., Sect. 3.4 and 4.4) are not calculated from the onion-peeling approach, but directly from slant columns. Differences in the path length in the UV (HCHO) and Visible (CHOCHO) are accounted for using a correction factor determined by O4 slant columns in the Visible and UV, which I think is valid (R_O4 in Eq. 9). However, parts of the CHOCHO information come from a distance from where no HCHO information comes from. For example, it can happen that a source of CHOCHO and HCHO is in a larger distance and can be seen only in the Visible (this is even more likely in an urban, inhomogeneous environment like the Frankfurt-Mainz area). Then, the CHOCHO slant column would be enhanced while the HCHO slant column is not enhanced and the simple conversion based on R_O4 does not take care of this. Thus, I think using range-resolved results from the onion-peeling is the much better way, especially since this technique is demonstrated in the previous section (of course the ratio of CHOCHO to HCHO is impossible then, but for the ratios with NO_2 this is possible).

In addition: Despite that the site of study is an urban area in Mainz, some biogenic contribution of VOC could be expected at least in some of the azimuth directions due to major growth of plants corresponding to the warm season and then major isoprene emissions. Could the authors comment on this?

(10) As MAD-CAT is an intercomparison campaign and many international groups gathered for several weeks, do you know if there will be further results/publications (if yes, the authors should announce this as being the first publication)?

Specific comments:

P. 11655, L. 1-3: 2D-MAX-DOAS is well-known (rephrase or delete the sentence)

P. 11656, L. 4-6 "The development … currently missing". Again one of the sentences that have to be rephrased. You may want to include for example Piters et al. (2012) for MAX-DOAS instruments already measuring in (more than 4) different azimuthal directions.

Piters et al.: The Cabauw Intercomparison campaign for Nitrogen Dioxide measuring Instruments (CINDI): design, execution, and early results, AMT, 2012.

P. 11658, L. 10 That means you are not using a fiber bundle? Does the mono-fiber preserve polarization?

P. 11658, L. 12: "was designed to exhibit a low residual error". Please explain what is the special thing that reduces the residual error (since this addresses the spectrometers and not the telescope).

P. 11658 L. 12 (and figure 1): The light from the integrating sphere and the scattered light view ports are using the same lens, correct (which is not shown in Fig. 1, but would be located at the right edge of the Fig.1)?

P. 11656, L. 21-23 and P. 11658, L. 20-22: The two modes are not special to the instrument which is able to point in any direction, I think. Moreover, these three modes have to do with the retrieval strategy that is presented in this study which should be pointed out more clearly.

P. 11659, Sect. 2.1.3 I find the alignment procedure really interesting. For the direct sun measurement (I know, this is the topic of another paper), do you use the same alignment procedure, so is this accurate enough for doing direct sun DOAS (without tracking)?

P. 11660, L. 18. Do you care for effects like atmospheric refraction when using a distant object for calibration of the EA, i.e. effects that make the object appear at another angle as the geometric angle? How large do you estimate such effect?

P. 11661, L. 10: I would put the information that you restrict to a case study already to the introduction. Furthermore, this raises again questions about possible upcoming publications/results.

P. 11661, L. 24 "1.7 mm optical mono-fiber" This is the outer diameter of the mono-fiber, correct? Otherwise, there is a discrepancy with the theoretical FOV = 2*arctan(d/2f) with d = 1.7 mm and f = 4*25.4 mm which would give a value of 0.95°.

P. 11662, Sect. 3.1 The authors should at least mention that the DOAS technique is based on the Lambert Beer law. In addition, one could show the DOAS equation, so that also readers who are not familiar with DOAS can conclude which "reference spectrum" is meant (P. 11663, L. 5)

P. 11663, L. 7 "… a Ring cross section is calculated from each reference spectrum…" This is performed as suggested by Wagner et al. (2009)? If yes, please give that reference here (or another reference describing the method you used).

Wagner et al.: "Three-dimensional simulation of the Ring effect…", AMT, 2009.

P. 11661, Sect. 2.3 In my opinion, the first paragraph of Sect. 2.3 belongs to the instrument description where I was missing information about the non-telescope-part of the instrument.

P. 11665, L. 16 How do you know if the minimum is local or global?

P. 11666, L. 9-11 This is somehow not a complete sentence…

P. 1166, Sect. 3.2.3 Please include a brief description/summary of Sinreich et al. (2013) and the correction factors, including pre-conditions and limitations, if existent.

P. 11670, L. 19 "… points out that the aerosol load around Mainz is homogeneous". This is an interesting result and it is also surprising. Looking at a map of Mainz and its surrounding (radius of 20 km, which is the effective radius), one finds large inhomogeneity in population (expecting less pollution to the North-West and more pollution to the North and North-East). Isn't this also in contradiction to the azimuthal NO_2 distribution derived later? Is there an explanation for the aerosol to be homogeneous?

P. 11671 L. 10-14 Please consider to show a picture of the averaging kernel from a measurement from 17 June 2013 to demonstrate your comment.

P. 11671, L. 20 Where does the factor of 1/2e for determining the PBL height come from? Is it a commonly used factor, just arbitrarily chosen or from any literature (then please give a reference).

P. 11674, Sect. 4.2.2 In the error discussion you should include changes of the PBL due to the terrain.

P. 11676, Sect. 4.4 In contrast to the onion-peeling approach which is performed only for the case study, the trace gas ratios were calculated for the whole campaign and your discussion is based on this? Maybe I missed that, but please make this clear.

P. 11678, L. 21-23: Please explain how you calculated the NO_2 VMR into VC using the PBL. If you perform this transformation assuming a box-profile, this is again in contradiction to the apriori-profile used in the profile retrieval (and to the retrieved profiles in Fig. 6).

P. 11681, L. 10-11 "We present the first fast 2D-MAX-DOAS measurements…" Again, please be more careful and rephrase (e.g., find other fast 2D-MAX-DOAS instruments in Piters et al. 2012).

Figure 1: The authors state that the fiber does not move, what is clear for the elevation due to the prism. But when changing the azimuth, does the fiber move? Is there an advantage of not moving the fiber (other than mechanical stress) because when taking a measurement the fiber does not move anyways?

Figure 2: Why is there shown an example from a previous campaign? I would prefer an example from the campaign the paper is about.

Figure 3: Why showing fit examples from 06 July 2013? All the study is about 17 June 2013, so please provide fit examples from this day.