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Interactive comment on "Intercomparison of aerosol extinction profiles retrieved from MAX-DOAS measurements" *by* U. Frieß et al.

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We thank the anonymous reviewer for the constructive comments, which are very helpful for an improvement of our manuscript. In the following, reviewer comments are cited in *italic*.

Frieß et al present inter-comparison of AOT and surface extinction coefficients retrieved during CINDI 2009 by five different groups (algorithms) from MAX-DOAS data, AERONET and humidity controlled nephelometer. They also compare aerosol extinction coefficient profiles from MAX-DOAS and ceilometer backscatter profiles smoothed on MAX-DOAS measurements sensitivity vertical and temporal grid below 4 km. Most algorithms use optimal estimation iterative fitting of the measured and modeled dSCD of the oxygen collision complex (O2O2). SSA, asymmetry parameter (Henyey- Green-

stein approximation), and surface reflectivity are input parameters that are derived from external sources. The main conclusions of the paper: 1) MAX-DOAS AEC profiles are relatively well captured while comparing with the smoothed ceilometer backscatter profiles that have no sensitivity below 150 m; 2) relatively good correlation (R ? 0.8) with the AERONET AOT but with the systematic underestimation of 10 - C1 30

Major concern:

In my opinion the paper should address in more detail the "correction" factor of 0.8 ± 0.1 used to decrease the observed dSCD(O2O2) to match the modeled dSCD(O2O2). This correction factor is mentioned in description of MCIP and AIOFM algorithms, but based on the previous publications, it is applied by all participating groups.

A scaling factor for the measured O_4 dSCDs is indeed applied by all groups. This correction factor is not a focus of the present study since the potential necessity to apply such a factor, its influence of the agreement of simulated and measured O_4 dSCDs and the accuracy of the resulting extinction profiles and AOT, as well as possible causes of the observed discrepancies when not applying this correction have been addressed by several other publications (e.g., Clèmer et al,. 2010, Spinei et al., 2015, and Ortega et al., 2016). We will add the following statement to the conclusions section: "A further source of uncertainty is the empirical correction factor for the O_4 dSCDs, for which a value of 1.2 - 1.3 has been applied by all participating groups. This correction factor has not been the focus of the present paper, but recent studies indicate that the disagreement between modelled and measured O_4 dSCDs is probably not caused by uncertainties in the temperature dependence of the O_4 cross section, but that elevated aerosol layers might be a potential cause."

Originally thought to be caused by the T-dependence of O2O2 absorption cross section, it is not supported by direct sun and airborne MAX-DOAS (Spinei et al., 2015) measurements. Recent study by Ortega et al., 2016 suggests that increase in dSCD(O2O2) is due to elevated aerosol layers. If this is the case dSCD(O2O2)

have larger sensitivity to the aerosol elevated layers than it is commonly assumed and the application of the correction factor is not acceptable for aerosol retrieval. Under-estimation of the MAXDOAS derived AOT relative to AERONET is of about the same magnitude as the dSCD correction factor.

In accordance with the findings from the recent studies mentioned by the reviewer, we agree that the disagreement between modelled and measured O_4 dSCDs is most probably not due to a temperature dependence of the cross section. However, the remaining disagreement of about 20% between MAX-DOAS and sun photometer AOD is certainly not caused by the correction factor since the retrieved AOD would be even smaller without correction (see Clèmer et al,. 2010), and the discrepancy between MAX-DOAS and sun photometer would further increase.

Minor comments: Section 2.4: What is the source of MPIC temperature, pressure and relative humidity profiles during CINDI? Using soundings launched over deBilt gives maximum VCD(O2O2) of 1.32 molecules2/cm5 vs 1.43 molecules2/cm5.

We agree with the reviewer that the O_4 VCD assumed by MPIC is too high. Since the retrieved O_4 DSCDs are converted into DAMFs by dividing them by the atmospheric O_4 VCD in the MPIC algorithm, a smaller O_4 VCD is equivalent with a larger O_4 correction factor. Therefore we will state an O_4 VCD of $1.32 \cdot 10^{43}$ molec^2/cm^5 and a conversion factor of 1.3 in the revised version of the manuscript.

Section 3.1 Averaging kernels are the result of the OE retrieval so different averaging kernels will be produced by different groups depending on their algorithm implemen-tation and input parameters. Since the a priori is not a true climatology, the same MAX-DOAS measurements will have seemingly different vertical sensitivities. Non-OE algorithms have no easy way to generate averaging kernels. I find it somewhat mis-leading to show ceilometer data convolved with the Heidelberg averaging kernels as to "what to expect" for MAX-DOAS retrievals from all groups.

We agree that the averaging kernels depend on the choice of the a priori, not only

because the a priori is not a true climatology. However, the limited vertical resolution represented by the averaging kernel is mainly determined by the limited information content of the measurements and not by the a priori (see e.g. Frieß et al., 2006). Therefore, we can expect that the resulting convoluted ceilometer profiles using averaging kernels from the different groups are very similar.

How do the authors define the PBL height from the MAX-DOAS aerosol profiles? Figures 4-7 show rather large variability between the groups in vertical distribution of AEC.

We do not provide a quantitative definition of the PBL height, and a common definition would be difficult due to the differences in the parametrisation of the vertical profiles by the different algorithms. It is obvious from Figures 4-7 that significant differences exist between the data sets, and we hope that the discussion of the discrepancies in the structure and height of the extinction profiles are discussed sufficiently in section 3.1.

In my opinion it will be useful to add "lessons learned" section to elaborate on the potential improvement of MAX-DOAS aerosol validation during CINDI-2016 campaign (e.g. needed in-situ and remote sensing instrumentation, observation geometries, etc.)

This point is quite difficult to answer since there was already a large and almost complete suite of aerosol instrumentation present during CINDI-I, and similar measurements will be performed during CINDI-II. Thus not the amount of data available for comparison is the limiting factor, but rather the amount of aerosol parameters retrieved from the MAX-DOAS measurements. Specifically, I think about the retrieval of aerosol optical and microphysical properties (single scattering albedo, phase function, size distribution and complex refractive index), which are preferably retrieved from azimuthal scans which will most probably be performed by several groups during CINDI-II. However, at the current state I am not aware of any algorithm available for the retrieval of such parameters (although the sensitivity to aerosol optical parameters has been demonstrated by Frieß et al., 2006), and I am therefore hesitant to give specific recommendations regarding these aspects at the current stage.

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Table 2 and 3: Please add "" to JAMSTEC data and an explanation below: * Only data points at UTC before 16:00 are reported*

An according footnote will be added to the revised manuscript.

Figure 2. SSA and asymmetry factors from AERONET CIMEL are shown only for parts of some days. How do the groups estimate these inputs when there are no AERONET retrievals?

Indeed, aerosol optical and microphysical properties from AERONET are only available from almucantar measurements every few hours during clear sky conditions. However, the variability of these parameters, in particular of the asymmetry parameter, are quite small. Therefore we expect that a temporal interpolation of these parameters that we have applied introduces only small errors compared to other error sources.

Figure 6. July 2 panels are not aligned with July 1 2009 panels.

This will be corrected in the revised version of the manuscript.

Figure 7. Why the gap in ceilometer data smoothed by Heidelberg averaging kernels during noon reference (zenith) measurements is narrower than in the Heidelberg retrieval data that produced the averaging kernels?

Thank you for pointing this out. There was an error in the script that performs the convolution of the ceilometer profiles which led to a wrong assignment of the averaging kernel to the respective time interval at the beginning of a data gap. We have recalculated the convoluted profiles and the figures in the revised version will contain correct data.

Figure 10. It is difficult to see individual group results. I suggest having a panel with the "reference" data and then plot the differences to the reference data (maybe in percent to the reference?)

We appreciate this suggestion. However, data from MAX-DOAS covers a larger time

range than Sun photometer data (which is only available during clear sky). Thus we would omit a large fraction of the data in the absence of Sun photometer data if we would plot the difference between MAX-DOAS and Sun photometer AOT.

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