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Atmospheric Measurement Techniques Discussions

Interactive comment on "Cloud detection and classification based on MAX-DOAS observations" *by* T. Wagner et al.

T. Wagner et al.

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Reply to Reviewer #2

The paper by Wagner et al. gives a very thorough overview on the possible impact of clouds on MAX-DOAS observations. The study also introduces a cloud detection and classification algorithm using typical quantities derived from MAX-DOAS observations like the column of the oxygen dimer O4 or the radiance ratio at two selected wavelengths (colour index). Some of the findings and also some concepts and ideas have been already published in recent studies, but never in such a comprehensive way like here. Since the presence of clouds is one of the major uncertainties for all studies using MAX-DOAS data the paper is well-suited for the publication in AMT. The authors

should address for some minor revisions/corrections as detailed below.

Detailed comments:

-p10301, 2.1: Please introduce CI before using.

Author comment: We replaced 'CI' with 'a so called colour index (CI)'

-p10302 2.2: Please explain why asymmetry parameters of 0.68 and 0.85 have been chosen or a give a reference to another study supporting these assumptions.

Author comment: We added the following references to the paper:

Dubovik, O., Holben, B. N., Eck, T. F., Smirnov, A., Kaufman, Y. J., King, M. D., Tanr′e, D., and Slutsker, I.: Variability of absorption and optical properties of key aerosol types observed in worldwide locations, J. Atmos. Sci., 59, 590–608, 2002.

Nakajima, T., and King, M. D.: Determination of the Optical-Thickness and Effective Particle Radius of Clouds from Reflected Solar-Radiation Measurements .1. Theory, J Atmos Sci, 47, 1878-1893, 1990.

We added the following text to section 2.2: These are typical values for clouds and pollution aerosols (see e.g. Nakajima, T., and King, 1990 and Dubovik et al., 2002); the actual choice is not critical for our study.

-p10303 2.3 Why the authors have not used backscatter lidar measurements which have been carried out during CINDI to further (and better) characterize the selected days? This information might be very useful to further prove some of the statements in section 3.1..

Author comment: Many thanks for this hint! We added LIDAR backscatter profiles to the paper (in Fig. 18). The comparison with the cloud properties from MAX-DOAS shows very good agreement in most cases. Remaining differences might be attributed to the following reasons:

-difference in location (LIDAR was about 150 m away from the MAX-DOAS)

-difference in viewing direction (MAX-DOAS looked 5° away from zenith)

-lack of sensitivity of Backscatter LIDAR for lowest atmospheric layer

-different temporal sampling

We added this information to the text (in section 4.7).

-we added a short description of the Backscatter LIDAR to the paper (new section 2.5)

-we added another co-author, Arnoud Apituley to the paper, who was responsible for the Backscatter LIDAR measurements during CINDI

-p10306,l5: "most probable reason for the discrepancy is the neglect of polarization in our radiative transfer model", please explain in more detail or again give a reference to a study supporting this hypothesis.

Author comment: We added the reference 'Mishchenko, M. I., Lacis, A. A., and Travis, L. D.: Errors induced by the neglect of polarization in radiance calculations for Rayleigh-scattering atmospheres, J. Quant. Spectrosc. Ra., 51, 491–510, doi:10.1016/0022-4073(94)90149-X, 1994.' to the text. In this study it is shown that errors induced by the neglect of polarisation are especially large in the UV spectral range (for non-cloudy sky).

-p10311 l3: 1:3 _ 1043 molec2/cm5 sounds like a very small number for the VCD for normal conditions. How this number was calculated? Do the authors have any idea why for this study no scaling factor is needed to get an agreement between modelled data and observations?

Author comment: Many thanks for this hint. Indeed we made a mistake (wrong surface elevation) in calculating the O4 VCD. The correct value is 1.41e43 molec2/cm5. We added the correct value to the text. We also updated the corresponding O4 dAMFs in all figures, and we corrected the threshold values in Table 2. Currently we have no

idea why in this study no scaling factor is needed. Interestingly this is also valid for the observations at low elevation angles. We added this information to the text.

-p10314 l25, "non-ambiguous", better: more apparent : : :

Author comment: We think that 'more apparent' is not exactly what we wanted to express. The effect of clouds on the CI is not only more apparent, but it always causes changes in just one direction. In contrast, the effect of clouds on the radiance can cause changes in both directions. Thus in specific cases both effects (increasing and decreasing) might exactly cancel. Such a cancellation can not happen for the CI.

We replaced 'non-ambiguous' with 'unambiguous'

-Table 1 and Figures 1 to 5, 15, 17: I would prefer to have the days in a temporal order instead of this arbitrary selection.

Author comment: The chosen days represent specific cases (e.g. 'continuous cloud cover' or 'fog', etc.). Our intention was to start with the description of simple cases (clear day, low aerosol load) and then address more complex scenarios (high aerosol load, continuous clouds, fog, broken clouds). We think this order is a good choice to successively introduce the different effects. Therefore we want to keep the order of days as it is. To make our intention more clear, we added the following text to the beginning of section 3: 'The first two days represent rather simple cases with either mainly clear or cloudy skies, the latter represent more complex situations with e.g. fog or broken clouds.'

-Figure 13: "indictor" indicator

corrected

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 10297, 2013.