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Atmospheric Measurement Techniques
Discussions
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Interactive comment on "Direct sun and airborne MAX-DOAS measurements of the collision induced oxygen complex, O₂O₂ absorption with significant pressure and temperature differences" *by* E. Spinei et al.

Anonymous Referee #1

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The paper " Direct sun and airborne MAX-DOAS measurements of the collision induced oxygen complex, O2O2, absorption with significant pressure and temperature differences " studies the influence of temperature and pressure dependence for several ground-based direct sun (DS) DOAS measurements and airborne MAX-DOAS observations. The publication is well structured and well-written. This study is necessary in order to clarify previous observations which showed the need for the introduction of

correction factors in order to explain measured O2O2 dSCDs by radiative transfer models. DS observations are useful since the air-mass factor can be calculated easily from geometric considerations. The MAX-DOAS data was recorded in an almost-Rayleigh atmosphere in an altitude of 9-13km, which simplifies radiative transfer modeling, even though observed spectra in this height contain a large contribution from upwelling photons which have scattered within the boundary layer. Large temperature differences showed the need to include the temperature dependent cross-section data from [Thalman 2013] in order to obtain small residuals of the DOAS fit. The overall absorption is buffered by the fact that the integral over the cross-section is stable with temperature. The correction factors obtained from the presented measurements differ from identity 5 times less than previous reported observations. Overall, this paper shows that for these types of applications the correction factor is close to 1, if it even exists. It furthermore shows that the size of the laboratory cross-sections of O2O2 is in agreement with radiative transfer modeling. It provides therefore important information for radiative transfer modeling in the atmosphere, e.g. for MAX-DOAS applications.

However, the previous correction factors were typically obtained from ground-based measurements which included small elevation angles and/or tropospheric air-masses. The limitation of the geometric calculation of the AMF for DS measurements to an AMF of 7 (to avoid using a more detailed model to calculate larger AMFs for larger SZA), results in a minimal elevation angle of ∼10°. Typical MAX-DOAS measurements have more than half of their elevation angles between 1-10°, thus information from these elevation angles, which can lead to the conclusion that a correction factor of the O2O2 XS is needed to explain observations, are excluded from this study. The same argumentation applies for the AMAX-DOAS measurements, which took place outside of the boundary layer. This publication does not discuss possible contributions within the boundary layer which could lead to a change in apparent optical depth of the O2O2 absorptions. Unknown absorbers (amongst others: water vapour absorption in the UV spectral range such as suggested in HITRAN2012 or [Polyansky2012]) or other influences could contribute to the discrepancy between modeled and observed O2O2 dSCDs. I must admit, however, that these are speculative, but possible consequences of the results of this publication could be discussed more in depth.

Minor points:

P 10026 l13: Herman et al 2009 (description of the MFDOAS instrument) cannot be found in the bibliography. Please recheck all your citations for completeness.

P 10032 l6: Instrumental stray light would cause spectral structures similar to 1/I0, and not as broad structures as shown in Figure 4. Additionally these residual structures are larger than typical peak-to-peak residual structures from MAX-DOAS measurements. For a tropospheric O2O2 dSCD at low elevation angles of 4e43 molecˆ2 cmˆ-5 the residual shown in figure 4 would result in a residual with at least 4e-3, which is typically not seen in MAX-DOAS measurements. Did the filters remove this (probably) systematic residual structure? Was it constant or did it change with the AMF?

P 10029 l2: The gap in the spectral evaluation from 366-374.5nm is explained by having problems with the correction of the Ring effect. This region does not show large Fraunhofer absorption lines. From my experience, this spectral region is one of the few regions <400nm, which does not show any systematic residual structures for long light paths. Has the size of the observed structure been correlated to the Ring signal, and if, how well was it correlated? How large is this residual structure compared to the Ring-signal?

P 10047 Table 3: H2O vapour absorption: Which of the cross-sections has been used, HITEMP or the cross-section measured in the laboratory? Has the HCHO cross-section from Meller and Moortgat been chosen for a specific reason instead of [Chance2011]?

P 10051 Figure 2: Since the correlations are almost perfect, the correlations plots do not add to the overall information content of the paper and the results could be reorganized in a table. However, the deviations from the linear fit might show systematic

limitations. I suggest changing this figure.

P 10054 Figure 5: The fact that the axis of Figure 4 and 5 are scaled similarly is appreciated. However, if the y-axis of the residual spectra would be scaled differently, the difference of using one and two temperature for the O2O2 cross-section could be seen more easily.

P 10026 l12: The baffling internal -> The internal

I recommend this article for publication. I apologize for the delay in reviewing this publication.

Bibliography:

Polyansky, O. L., Zobov, N. F., Mizus, I. I., Lodi, L., Yurchenko, S. N., Tennyson, J., Csaszar, A. G., and Boyarkin, O. V. (2012). Global spectroscopy of the water monomer. Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 370(1968):2728–2748.

Chance, K. and Orphal, J. (2011). Revised ultraviolet absorption cross sections of H2CO for the HITRAN database. Journal of Quantitative Spectroscopy and Radiative Transfer, 112(9):1509 – 1510.

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