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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical Note: Cloud and aerosol effects on rotational Raman scattering: Measurement comparisons and sensitivity studies" *by* A. Kylling et al.

Anonymous Referee #3

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This paper documents the addition of rotational Raman scattering to the DISORT radiative transfer code that is implemented into the freely available libRadtran package. Since there are not many codes with this capability and some are not freely available, this is an important addition that should be documented. A technical note is appropriate for this. This technical note may have been a better fit to the sister Atmospheric Measurement Techniques (AMT) journal.

The model reproduces various features shown by other codes. Comparisons are made with ground-based observations for clear, cloudy, and aerosol-loaded conditions. In addition, sensitivities to various assumptions and parameters are documented. However,

I have several major concerns about the manuscript that must be address before it can pass to the next stage. For the sake of brevity, my review will attempt to hit points not already covered by the other reviewers. I also did not carefully review certain portions of the manuscript (e.g., Appendix), since those sections were already covered in some detail in the previous reviews.

I agree with the other reviewer regarding the organization of the manuscript. The first part should discuss the details, the second part should cover validation (including one to one comparisons with other documented codes). Only then can a reader have confidence in the sensitivity studies that should be presented lastly.

p. 22516, L 15: Since the filling-in has also become known as the "Ring effect" (named after Grainger and Ring), this work should be referenced in the introduction.

p. 22516, L 20: Cloud-top pressure is not the quantity that the filling-in is actually sensitive to. One can only obtain cloud-top pressure if the vertical structure of clouds is known, which is usually not the case.

p. 22517: Please carefully review your descriptions of various codes referenced. There is at least one major error in these descriptions.

p. 22520, L2: If NO2 absorption is included, than so too should be oxygen dimer as well as ozone. Oxygen dimer absorption at the peaks at 360 and 380 nm are comparable to absorption from NO2. It might explain some discrepancies with the observations, though not all of course. In addition, it is not stated how much NO2 absorption is included.

Figure 1a: The curve showing the measurement in thunderstorm provides no value as it is shown. It should be scaled up with an appropriate scale factor. This is true again for Figure 2a.

In the discussion of Fig. 1b on top of p. 22524, it is stated that "the inclusion of rotational Raman scattering in the model calculations clearly improves the model/measurement ratio for both situations, and especially the thunderstorm simulation." This is certainly true for the thunderstorm situation, but the clear sky shows basically no improvement except at the peak absorption of the calcium lines.

The data quality here appears to be particularly poor for comparison with model calculations as the errors are much larger than the Raman scattering contribution. There are much better data sets available for comparison, both ground- and satellite-based.

In the next paragraph, comparison is made with DODs with significant differences seen. It is stated that "The cause of the differences is not known, but the data suggests that it is due to measurement uncertainties." The authors should be more precise with the wording here regarding "measurement uncertainties". Some of the high spectral frequency differences are repeatable in both clear and cloudy skies (Fig. 1b), suggesting that this is not measurement noise, while there is also a low frequency difference between clear and cloudy sky. More precisely, can these differences be attributed to error in the assumed surface albedo, calibration, or model calculations, etc.?

In the next paragraph, the data are not of high enough quality to tell which model provides a better fit to the data. This should be stated. The parameters for computations should be repeated here rather than referring to other papers (e.g., description in Sect. 2.1). How is it known that there are absorbing aerosols present in the cloud? If this was shown in another paper, please provide a more detailed explanation of how.

Again, in sect. 3.2, the measurements have a noise of 0.3-0.5%. Again, measurements with much higher signal-to-noise ratio are available and would be more appropriate for this work.

In figure 2b, the disagreement between measurement and model is of comparable magnitude to the filling-in itself. I would not agree with the statement "As for the thunderstorm case there is very good agreement between the measured and modelled DODs." Does the model correctly reproduce the observed difference in DOD (or filling-in) between the two measurements at different solar zenith angles? This is not shown.

In Sect. 4.1, a statement is made "that the TOA results have similar behaviour to those presented by Joiner et al. (1995, Figs. 8 and 9). However, the results shown here are for a moderately thick cloud, while Joiner et al. results were for Lambertian surfaces with different pressures and reflectivities. So results cannot be directly compared. Why not reproduce the conditions of the Joiner et al. paper for more direct comparison as well?

Again in Sect. 4.2,a statement is made that "radiance results are consistent with similar results from de Beek et al. (2001) and LER results from Joiner and Bhartia (1995)." I would not agree with this statement and then, why not try to reproduce the conditions of these other works that were clearly stated. First, the de Beek et al. example was at a different solar zenith angle. Second, Fig. 4 was done differently than their Fig.4. They plotted as a function of cloud top height for different cloud optical thicknesses (COT) (2, 5, 10,... 100). So they did not produce the peak that is seen at cloud optical depth of about 2. While this may be correct, it would be nice to give more explanation of why there is a peak, etc. Also, de Beek results show a saturation of Ring DOD starting at COT of about 10, whereas the results here show saturation starting at COT of 30, so I do not agree that results are consistent. In addition, Joiner and Bhartia 1995 showed results for different pressures of Lambertian surfaces, so results cannot be directly compared. Spurr et al. (2008) show results of filling-in at 5 km for a cloud between 2 and 3 km for different COT. They see no peak in filling-in at low COT and similar to de Beek saturate at lower COT (5-10).

For downwelling (BOA) results, these should be compared with Spurr et al. (2008). For the values shown at 1 km from Spurr et al, the results are quite similar. Spurr et al. did not show results for higher COT. It would lend more confidence to the results if the behaviours seen in these other papers are reproduced. If that involves obtaining the other codes or working with the producers of these codes, then the effort should be undertaken.

Sec. 4.3: The MLER model has been used by others, such as in the FRESCO algorithm described by Koelemeijer and Stammes et al. in several papers (see also Stammes et al., JGR 2008). The LER (and also MLER methods) compensates for photon transport within the cloud by placing the Lambertian surface somewhere in the middle of the cloud, not at the top (see also Vasilkov et al., JGR 2008). The MLER provides a model to account for scattering/absorption that occurs below a thin cloud. If the LER cloud pressure is correctly placed, it should account for absorption by trace gases correctly (also see Ziemke et al., ACP 2009 and Vasilkov et al., AMT 2010).

Sect. 4.3 and Fig. 5: I am surprised that BOA results are “minimally influenced by cloud top height”. This is not intuitive to me. Also, the slight peak in filling-in at about 4 km for COT 10 is not completely intuitive and should be discussed more and validated with other codes.

Sect. 4.5: It would be nice to see results for nadir TOA as well.

Fig. 6: I do not understand exactly what is being shown or compared with the Spurr et al. results; the figure needs a better description. The diffuse irradiance results seem to be what agrees best with Spurr et al, but that does not appear to be what they calculated. Why are global results shown for downward irradiance and not upward?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 22515, 2010.