**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 #1

Received and published: 25 November 2010

The paper by Kylling et al. presents results of a new model for the simulation of the Ring effect and a comparison of the model results with selected measurements. New results compared to existing studies are a) the investigation of the height dependence of the filling in and b) the simulation of the FI for both radiances and irradiances. In principle the paper is well suited for publication in AMT. However, in its current form, I see two important shortcomings:

a) The comparison between measurements and model results is not convincing to me. My main concern is that spectra are compared, for which not only the cloud or aerosol properties changed, but also the SZA. Especially for the aerosol case, it is unclear,

which fraction of the observed difference is caused by the aerosol influence, and which fraction is caused by the change in SZA. I strongly suggest to repeat the comparison for spectra measured at the same SZA. There are additional concerns about the measurements, which are described in detail below. The part on the comparison between measurements should be improved as suggested or removed from the paper.

b) Many of the results are not new (e.g. the SZA dependence of the filling in). I suggest that for such cases, the authors should compare their results with those of existing studies, or should remove them. In addition, the authors should clearly state what the main findings of their study is. For example, in the conclusions, there is a list of detailed results, but it is not clear in which way these results could be used, e.g. for improved remote sensing applications. If the main purpose of the paper is just to introduce the new model (which in principle is fine), I suggest to include an explicit comparison of the new model results with those from existing models (e.g. Joiner et al., or Vountas et al.).

After addressing these points (and additional comments, see below), the paper should be published in ACP.

Specific comments:

1) Add references of existing studies (e.g. Joiner et al., Vountas et al.) at p. 22516, line 20

2) At p. 22519, line 23 it is not clear to me why a grid of 0.05nm was chosen? What is the original spectral resolution of the solar spectrum? How (strong) does the FI depend on the chosen wavelength grid?

3) p. 22520, line 1-3: why is NO2 included, but O3 excluded? For wavelengths >340nm the optical depth of the ozone absorption can reach relatively high values (e.g. optical depth >2% for an air mass factor of 2 (e.g. SZA of 60°). For larger SZA the optical depth becomes even much higher. I don't understand why NO2 was included in the

simulations, but O3 was not.

4) At p. 22520, line 20 it should be mentioned that the probability for Raman scattering depends not only on the path length, but also on the altitude

5) p. 22522, line 10: what is the justification of a varying albedo? What is the justification for the specific values selected at different wavelengths? Are the authors sure that the albedo is only 1% at 340nm (and changes rapidly to 3% within 20nm? The location of the measurement in Greece is close to the coast. What is the effect of 'reflection' at the water surface, for which the UV albedo is probably larger?

6) p. 22522, line 26: The authors might mention that the filling-in depends on the viewing angle of the satellite; even for 'quasi' nadir observations, these effects have to be considered (they are typically of the same order of magnitude compared to the aerosol effects).

7) p. 22523, line 12: according to the spectral resolution and the wavelength grid, the measured spectra are undersampled. I am concerned that undersampling effects could interfere with the Ring effect.

8) p. 22523, line 24: does this statement describe the ratio between the cloudless and cloudy spectra? Then part of the difference is also related to different SZA.

9) Fig. 1a: why are the model results for the cloudy case are not shown?

10) Fig. 1a: Maybe a logarithmic irradiance scale might be more useful. Then also the cloudy spectrum could be well displayed.

11) Fig. 1b: The legend seems to be wrong.

12) Fig. 1b: What is the reason for the relatively large differences between measurements and model results (up to +/- 10%) and for the systematic spectral structures? Could these structures be related to the spectral undersampling?

13) Fig. 1d: It seams that in the measurement ratio some spectral features are appar-

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ent (e.g. at 398nm), which are narrower than the spectral resolution of the instrument. What is the reason for these structures (undersampling?)?

14) What is the effect of the sequential scanning of the spectra? Especially for cloudy cases the atmospheric light paths might change rapidly.

15) p. 22525, line 17: What is meant with 'slit width' Is it the width of the entrance slit or the spectral resolution of the instrument? If it is the spectral resolution, the undersampling would be even worse compared to the cloud spectra.

16) p. 22526, line 3: I think the expression 'very good agreement' is not supported by the results.

17) p. 22527, line 5: I suggest to add that the filling in is also increased with increasing SZA due to the different phase functions for elastic and inelastic scattering. Especially for small SZA this is probably the dominating effect.

19) p. 22532, line 10: I am not sure that this can be concluded from the measurement/model comparison

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