Atmos. Chem. Phys. Discuss., 9, C7273–C7277, 2009
www.atmos-chem-phys-discuss.net/9/C7273/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of aerosols on polarized sky radiance: model development, validation, and applications" *by* C. Emde et al.

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Received and published: 19 November 2009

First we would like to thank Brian Cairns for his valuable comments and suggestions. We followed each of them in our revised version of the manuscript.

Below follows a detailed point-by-point reply.

The paper presents a new Monte Carlo model that includes polarization and benchmarks the model against existing results. This is a valuable capability and well worth being published in ACP to provide documentation of the performance of the model. To that end I would suggest that the authors also test their model against the inhomogeneous atmosphere results for a Haze L/Rayleigh atmosphere presented in de Haan et al. (1987). I have found this test case much more useful in benchmarking model performance than the standard Rayleigh test cases because

it is more stressing and includes internal fields which, if a model is to be used for simulating aircraft data is a necessary capability.

We have compared our model against Haan et al. (1987) for the inhomogeneous atmosphere and found an excellent agreement. Results are shown in Table 2. These results correspond to Table 9 of de Haan et al. We have also compared to Tables 10-12 of de Haan et al. and the agreement was always found to be excellent.

One aspect of this paper that I find a little off putting, given its intention, is the documentation of the size distributions and refractive indices used in the microphysics calculations. Referring back to OPAC is not helpful and it would be far preferable to tabulate size distribution and refractive index parameters used for all the various calculations in a table. One obvious reason for doing this is Figure 2 where the ripples on the large particle phase matrix elements may be real, or may be due to incorrect truncation of the size distribution, or an inadequately fine sampling of the size distribution. In particular for a 10μm drop the effective variance should also be provided so that any results can be replicated.

The size distributions and refractive indices are now provided (see Table 1 for aerosols and text for the cloud). The ripples on the phase matrix are most probably due to the not fine enough sampling of the size distribution. Since these ripples do not affect the results much we think that there is no need to redo all calculations. The comparison against measurements would not improve by using a finer sampling of the size distribution.

Given that the aerosol optical depth is often specified at a reference wavelength of 550 nm I would suggest that the authors make a statement at the beginning of the section in which they make their various calculations that the optical depth always refers to the optical depth at the wavelength of interest.

Such a statement has been included in the beginning of section 3.

I find the efficiency calculations a little surprising. Although the scaling of time as #streams^3 for *polradtran is correct the 22m for 100 aerosol+Rayleigh calculations for 16 streams compares with a doubling/adding (DA) code that I run on a 2.4 GHz Intel Core Duo chip that for 20 streams takes 14 seconds to make 100 calculations for a multi-layer atmosphere. The accuracy settings for a run of that speed allow the de Haan benchmark to be matched to within 0.1% worst case. Also a DA code gives all view and solar illumination angles in a single run of the code which is not true of MC - this should be noted. Lastly for a Rayleigh atmosphere there are only three terms in the azimuthal decomposition that contribute to the observed radiance so 22 m for a hundred Rayleigh atmospheres is frankly bizarre. I would suggest that the authors de-emphasize this comparison because I believe they are not running the polradtran code in an efficient manner. As I note a well designed DA code will run far faster than the MC code for both aerosols and Rayleigh scattering. The advantages of a MC code lie elsewhere and it would make sense to emphasize the accuracy of the code and its consequent availability for calculations for which standard plane parallel codes are entirely inadequate.*

We agree that we probably did not use polradtran in the most efficient way. Therefore we have removed the comparison against polradtran from section 2.7. Now we show only the MYSTIC CPU times.

Comparisons between the MC and delta-M approximations are interesting academically, but in point of fact the delta-M approximation is a crude way of dealing with forward scattering. Removing the diffraction peak when it is of 1-2 width and compensating by reducing the extinction for large particles (cf Hansen and Travis 1974) does not have nearly such a deleterious effect as the delta-M approximation particularly if it is combined with the removal of single scattering from the DA calculation which is subsequently added back in exactly (cf de Haan et al. 1987). Also for aerosols at least 20 quadrature points are needed to get good results while for clouds 70 quadrature points, or streams allow good results to be obtained.

In the meantime we became aware that using delta-M scaling alone is not the "common" method anymore. Many 1D-models use it in combination with a second order intensity correction and can handle the phase matrices quite accurately. We have not removed the comparison from the paper since we also find it "academically interesting".

Since this comparison is not very important we have changed the abstract and removed the part about advantages of the Monte Carlo methods compared to discrete ordinate methods regarding the accurate handling of the phase matrix. Instead we focus on model development and validation, and the comparison against UV measurements.

Lastly, the comparison of data with OPAC aerosol mixtures merely emphasizes how limited the utility of such aerosol climatologies is. Clearly the mineral dust size is not consistent with the data (forward scattering mismatch) and the water soluble particles are either the wrong size and/or the wrong refractive index. 5-10% errors between model and measurement are huge and it should be so stated. Much of the value of polarimetry lies in the fact the DoLP can be measured with 0.1% accuracy which is why these mismatches should be emphasized as ruling out OPAC mixtures as an acceptable model.

We have stated that there are larger descrepancies between model and measurement. We emphasized more clearly in section 4 and in the conclusions that these differences are due to OPAC, which does not include any aerosol type or mixture that matches the observation.

The suggestion to look at 3D effects for the APS Glory sensor is an interesting one, but I would suggest getting one's feet wet with the Research Scanning Polarimeter that make very similar measurements to APS and has been deployed in the MILAGRO, ARCTAS, CRYSTAL-FACE and RACORO field experiments and where there is a substantial amount of in situ and correlative data with which to test any model results/comparisons.

We find the suggestion to look at 3D effects for the Research Scanning Polarimeter (RSP) very interesting and hope that we will do such a study soon. The suggestion is now also included as outlook at the end of the paper. We have also included the RSP and appropriate references in the introduction.

Inspite of the equivocations above I found the paper well written and informative and would hope that the authors take the criticisms and comments and use them to focus the paper on what MC methods are good for and how well their particular model works.

We thank again for the constructive criticisms and the helpful comments!

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 17753, 2009.