

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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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.

One aspect of this paper that I find a little off putting, given its intention, is the doc-

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umentation 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 10 μm drop the effective variance should also be provided so that any results can be replicated.

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.

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.

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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 $1\text{--}2^\circ$ 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.

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.

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.

In spite 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.

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