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Interactive Comment

Interactive comment on "McSCIA: application of the Equivalence Theorem in a Monte Carlo radiative transfer model for spherical shell atmospheres" *by* F. Spada et al.

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General comments on Spada et al. (2006):

This paper addresses an area of research relevant to Atmospheric Chemistry and Physics Discussions, namely the simulation of radiative transfer in a spherical shell atmosphere. A new model is introduced and shown to produce results consistent with existing models that have been described in the literature. The testing is thorough enough to produce meaningful results, and is described carefully to permit the reader to understand and verify the results. The paper is structured well and clearly written. References, figures and tables are sufficient to support the text. The work is well conceived, and is refreshingly thorough and self-contained, almost reading like a dissertation in some sections. (The possibility of writing this way, including interesting details without particular regard for the page count, is one of my favorite aspects of online journals.)

Specific comments:

Line 130 - As the sentence is written, it is unclear whether (Marchuk et al., 1980) is cited as originating the idea of the biasing described in equation (A6), or as an example of past work that failed to use the biasing (and therefore produced poor statistics for the limb case).

Line 161 - What is meant by “conservative” in the context of ground reflection? My first thought is an analogy to a conservative (i.e., non-absorbing) scattering event, but that seems unlikely.

Line 168 - “differences less than the statistical error of McSCIA.” What was the statistical error of McSCIA (for this exercise)?

Line 170 - The work of Kattawar and Adams (1978) is mentioned here, and then never again until the Conclusions. Were any comparisons done between the McSCIA results and the Kattawar and Adams (1978) results (for a spherical shell atmosphere containing only haze particles that scatter according to the Henyey-Greenstein phase function)? If so, then I am interested to see the results; if not, then the reference to Kattawar and Adams (1978) should be removed.

Line 185 - I agree that the otherwise useful Adams and Kattawar (1978) reference contains no indication of the statistical error in their calculations (a most puzzling omission!). However, if I may shamelessly indulge myself and quote from my own dissertation (Loughman, 1998, pp. 92-93):

Adams and Kattawar (1978) do not state the number of photon histories used in their calculations, but the magnitude of the statistical fluctuations can be estimated indirectly. Adams and Kattawar (1978) note that reciprocity is violated by as much as $\pm 3\%$ in some of their flat atmosphere calculations (for which reciprocity should be observed exactly). Regarding an earlier test case, Adams and Kattawar (1978) state that “no differences were obtained between the (flat atmosphere results) and the (spherical-shell atmosphere results) other than those caused by statistical fluctuations. The largest errors encountered in the total radiance were of the order of 4 to 5% and in most cases substantially less" (p. 143). In addition, $I(\theta = 0°, \phi = 0°)$ in Table I of Adams and Kattawar (1978) differs from $I(\theta = 0°, \phi = 180°)$ by 5% for both $\theta_0 = 70.47°$ and $\theta_0 = 84.26°$. These values clearly should be equal, because I has a unique value at $\theta = 0°$ regardless of azimuth angle. These observations suggest that the statistical fluctuations in the Adams and Kattawar (1978) results are probably **at least** $\pm 3\%$, on average.

In that context, I am not surprised to hear about some cases with $3-5\%$ disagreement. I saw particularly large disagreements for the $\theta_0 = 84.26°$, optical thickness $\tau = 0.25$ case. For the following lines of sight, the total radiance difference exceeded 2%. A negative value indicates (Adams and Kattawar, 1978 radiance) < (Loughman, 1998 result):

$(\theta, \phi) = (85°, 180°) : -2.9\%$

$(\theta, \phi) = (70°, 180°) : -2.2\%$

$(\theta, \phi) = (30°, 180°) : -2.5\%$

$(\theta, \phi) = (30°, 0°) : -5.6\%$

$(\theta, \phi) = (50°, 0°) : -3.0\%$

Hopefully these results might be useful in providing a third opinion to detect which Adams and Kattawar (1978) numbers may be “outliers”. Disagreement in the single-scattered (SS) radiance was $< 0.7\%$ for all lines of sight in all cases simulated in

Loughman (1998), so nearly all of the observed disagreement can be attributed to the multiple-scattering (MS) calculation.

PS – The model described in the dissertation generally is no more effective than the much faster GSLS model devised by David Flittner (and improved further by Didier Rault with a little assistance from myself, which was ultimately described in Appendix A of Loughman et al., 2004), so this is probably as close as the material in the dissertation will ever get to publication!

Line 258 - The Equivalence Theorem (ET) method seems to have the potential to extend the traditional range of uses for Monte Carlo (MC) radiative transfer calculations. The MC method is often rejected as a practical method for operational radiative transfer calculations (such as those used for retrievals of atmospheric properties) due to its relatively low speed. But the ability to simulate the radiance field for many absorbing gas profiles (given the distribution of scatterers) off-line appears to change that cost-benefit calculation a bit (for the problem of retrieving absorbing gas profiles, at least!). How fast are the off-line calculations, compared to the on-line calculations? For me, this possibility is the most interesting aspect of this work, and I would like to hear the authors discuss the topic further.

Lines 315-316 - The atmospheric gridding is described as "similar to" the method used by Postylyakov (2004). Does that mean "the same as" or "nearly the same as"? Perhaps it would be best to describe the method used in this work precisely. I belabor this point because differences in the atmospheric layering were the dominant reason for the height-dependent differences in the SS radiances shown in Figs. 3, 5-7 of Loughman et al. (2004). When the same layering method was used, differences of $< 1\%$ were generally achieved for the SS radiance comparison. For the comparison of total (SS + MS) radiance, factors other than layering appeared to dominate the behavior of the differences. So, while I agree that differences between the models of $< 1\%$ are not cause for great alarm, I'm not sure I agree with the statement that they are "acceptable since the optical properties are derived in different ways." The atmospheric layering

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difference produced a systematic difference in the radiance calculations presented in Loughman et al. (2004), but it was not the dominant source of differences.

Lines 332-340 - These differences are quite small, so I wonder if some “shadow-chasing” is occurring here (appropriate for SCIAMACHY-related work, I guess?). But (continuing the previous thought), if the layering methods used by McSCIA and MCC++ are not truly identical, I wonder if that might be a more likely cause of the small observed agreement? The behavior of the SS radiance difference and the total scattering radiance difference are similar, particularly at 325 nm.

Line 349 - I think a symbol for the “standardized differences” might be in order.

Line 429 -Why are no quantitative results presented to indicate the performance of the model in plane-parallel mode?

Lines 430-432 - This sentence is unclear, since the spherical-shell atmosphere is homogenous (extinction coefficient does not vary with altitude) in the Adams and Kattawar (1978) and Kattawar and Adams (1978) cases, but not in the Postylyakov (2004) case (unless the atmosphere described in Loughman et al., 2004 was not actually used?). Also, as mentioned earlier, no comparisons to Kattawar and Adams (1978) were actually presented.

Line 443 - Could you define “extreme cases” in this context? I have some ideas (large solar zenith angles, optically thick atmospheres), but I’d rather hear what was actually done.

Line 510 - My copy of the paper says “In this case, Eq. refeq:fundamental becomes” (this looks like a LaTeX problem).

Fig. 5 - The data is given by Adams and Kattawar (1978) as discrete points, so I don’t understand the decision to present it as lines in this figure. Do the lines simply connect the dots, or is something more sophisticated done? It would be more useful to add a plot (or present a table) of the differences between the McSCIA and Adams and

Kattawar (1978) results, point-by-point.

Fig. 8 - This is just a tiny quibble, but if you can achieve accuracy on the order of 0.1% (for the SS radiance in Fig. 9, anyway), why is the Rayleigh scattering coefficient profile for 325 nm scaled by 0.776 to produce the Rayleigh scattering coefficient profile for 345 nm? According to Table 3, the ratio is 0.7757, a 0.034% difference.

Fig. 9 - I would call the colored region “shaded” or “colored”, rather than “grey.” And your results resemble Fig. 4 of Loughman et al. (2004) most closely, since that figure shows scalar calculated radiances (rather than the vector radiances shown in Fig. 3).

Technical corrections:

Line 218 - “bounded to as spherical” should be “bounded to a spherical”?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 1199, 2006.