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Atmospheric Chemistry and Physics Discussions

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.

F. Spada et al.

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We thank Dr. Loughman for his careful reading and constructive comments. Below we give our reply point-by-point.

Comment #1 *[...] 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.)*

Reply #1 Since the beginning of the development of McSCIA, we noticed that there were many good references for the development of a 3D MC RT model, but they were all written for experts. Therefore we decided to use the opportunity of this online journal to make the paper also a primer for people that would start to develop such a code.

Comment #2 *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).*

Reply #2 The idea originated by Marchuk. We will change the sentence into:
"As described by Marchuk et al. (1980), not using this biasing would result ..."

Comment #3 *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.*

Reply #3 The analogy is, actually, correct: the ground reflection is considered non-absorbing. The reasons are two:

1. We are evaluating only the ray-tracing part of the model. Absorption (both by molecules and the surface) will be considered later.

2. Absorption will be computed, anyhow, by reducing the weight of the photons reflected by the surface.

This will be clarified in the text.

Comment #4 *Line 168 - "differences less than the statistical error of McSCIA." What was the statistical error of McSCIA (for this exercise)?*

Reply #4 The statistical error was on average 0.1%. This will be added to the manuscript.

Comment #5 *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*

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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.

Reply #5 The results are plotted in Figure 5. Only some cases were chosen to give an impression of the quality of the results, because otherwise the paper would become too lengthy.

Comment #6 *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!). [...] 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.*

Reply #6 As pointed out in the previous comment, due to the length issue, we think that it would be better not to expand this part of the paper, but may be we could provide our and your results and considerations on a public web-page. These results can be very important when developing a new spherical RT model. In fact, in other intercomparisons in which McSCIA took part (e.g. Walter et al., 2005), these results from Adams and Kattawar (1978) and Kattawar and Adams (1978) were the first ones to be used for validation purposes.

Comment #7 *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.*

Reply #7 The gain of speed due to the use of the ET depends critically on how it is used. In fact, the gain of speed in McSCIA depends on the amount of statistical information retained after the ray-tracing part. With a small amount of information retained it would be possible to build a very fast model but without too much flexibility (e.g. as done by Partain et al. (2000)). Otherwise, if more information would be saved, in order to study more complex cases (e.g. 2D absorber fields (Spada et al., 2006, in preparation)), then the gain of speed would be less.

Partain et al. (2000) retained the statistical information as a PDF. This condensed form of information works efficiently, but is only an approximation. So, we decided to retain all ray-tracing information. At the moment the ray tracing part of the model is written in FORTRAN-90 and all statistical information is stored (scattering position, scattering angles, photon weight). A post-processing code is written in Interactive Data Language (IDL) which provides good visualization tools. From this choice it is clear that computing speed is not a consideration at the moment.

This consideration on the gain of speed using the ET in McSCIA is now added to the paper in Sect. 6 Discussion.

Comment #8 *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. [...]*

Reply #8 Agreed. We will do this in the revised version.

Comment #9 *Lines 315-316 [...] So, while I agree that differences between the models*

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of $<$ 1% are not cause for great alarm, I am not sure I agree with the statement that they are "acceptable since the optical properties are derived in different ways." The atmospheric layering difference produced a systematic difference in the radiance calculations presented in Loughman et al. (2004), but it was not the dominant source of differences.

Reply #9 We agree that this may not be the dominant source of differences, but during the intercomparison of McSCIA with the model of H. Walter (Walter et al, 2006), we have noticed that a small difference in the pointing of the satellite, along with slightly different optical properties of the atmospheres, could cause differences of more than 1%. We will mention this in the revised version in section 5.1 Validation of McSCIA in 3-D.

Comment #10 *Lines 332-340 - [...] 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.*

Reply #10 While the layering may be the same, we still think that differences in optical values and satellite pointing could cause these differences, given also the similarity between SS and TS, as you pointed out.

Comment #11 *Line 349 - I think a symbol for the "standardized differences" might be in order.*

Reply #11 We will add the symbol Δ_S in the revised version.

Comment #12 *Line 429 -Why are no quantitative results presented to indicate the performance of the model in plane-parallel mode?*

Reply #12 We checked of course the model for plane-parallel geometry, as mentioned in Sect. 3 second paragraph, but because of the length of the paper we did not give

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more details. Calculations in plane-parallel geometry typically take less than 5 minutes for 1.000.000 photons.

Comment #13 *Lines 430-432 - This sentence is unclear, since the spherical-shell atmosphere is homogeneous (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.*

Reply #13 Yes , the sentence is unclear, it will be corrected in the revised version. For the intercomparison with MCC++ the atmosphere of Loughman et al. (2004) was used, which is not homogeneous, while for the case of Adams and Kattawar (1978) and Kattawar and Adams (1978) a homogeneous atmosphere was used, as described by their papers. Partial results of the intercomparison with Adams and Kattawar (1978) and Kattawar and Adams (1978) are plotted in Figure 5.

Comment #14 *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.*

Reply #14 The cases studied are illustrated by Table 4 and in the caption of Figure 11 and are mainly studies that go from very thin to very thick atmospheres for tangent altitudes from 20 to 60 km. This will be clarified in the revised version.

Comment #15 *Line 510 - My copy of the paper says "In this case, Eq. refeq:fundamental becomes" (this looks like a LaTeX problem).*

Reply #15 This error will be corrected.

Comment #16 *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*

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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.

Reply #16 The lines only connect the points of the results by Adams and Kattawar (1978) while the dots represent the results of McSCIA. The lines were chosen instead of another symbol because it is more readable. While a plot/table with the differences between McSCIA and Adams and Kattawar (1978) would have better illustrated the errors, we considered more interesting to show radiances of the different scenarios (with the sun is in front or behind the satellite instrument).

Comment #17 *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.*

Reply #17 You are right. The profile was scaled with the correct factor, but we wrote an approximate value. Actually the factor used for the scaling was 0.77573347. This is now corrected in the manuscript.

Comment #18 *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).*

Reply #18 Both inaccuracies will be corrected in the revised version.

Comment #19 *Line 218 - "bounded to as spherical" should be "bounded to a spherical"?*

Reply #19 This will be corrected in the revised version.

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