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Atmospheric
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Discussions

# *Interactive comment on* "Multiple scattering in a dense aerosol atmosphere" *by* S. Mukai et al.

**Anonymous Referee #2**

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Review of the manuscript entitled

"Multiple scattering in a dense aerosol atmosphere"

by S. Mukai, T. Yokomae, I. Sano, M. Nakata, and A. Kokhanovsky

The authors describe an interesting topic related to the retrieval of aerosol properties based on radiative transfer simulations for an optically thick, cloud-free aerosol atmosphere.

The manuscript has some major scientific and technical shortcomings so that a publication in the present form is not recommended.

The authors are asked to follow the recommendations and comments listed next when preparing the revised version of their manuscript.

General:



i) The English language needs to be improved at various places. It is strongly suggested that the manuscript is given to a native English speaker to polish up the text.

ii) The typesetting of some of the formulas in Section 3.1 is not correct. For example, the inner integrals of the terms on the right-hand side of Eq. (17) appear in the upper boundary of the outer integrals. This appears at several other places on pages 889 - 891. Please correct.

iii) Section 4 has just three pages and is meant to contain the main scientific results of this manuscript. These three pages seem not to be in balance with the lengthy derivation (9 pages) of the formulas in section 3.1.

More material should be added to Section 4, for example

- intercomparison of the MSOS code for semi-infinite medium with other exact and well-tested methods - assessment of the errors of the proposed retrieval scheme - sensitivity studies - comparison with other retrieval studies in the scientific literature having similar focus (optically dense case)

Major issues:

1) Abstract: Since the successive order of scattering method in the context of a one-dimensional semi-infinite atmosphere is the major ingredient to this manuscript, the semi-finite approach to solve the radiative transfer equation should also be mentioned in the abstract.

2) It is strongly suggested that the authors provide an intercomparison of their semi-finite medium MSOS code results with other well-accepted and verified codes (such as DISORT or an adding-doubling scheme). To achieve this, some plausible scenarios should be chosen which are representative for the dense aerosol atmosphere situation (optical depth, wavelength, solar zenith angle, observation viewing angle and surface albedo), since this type of application is the main focus of the manuscript.

Please provide the following information with respect to the radiative transfer solver:

- Details on the inputs required for running MSOS

- How many orders of scattering need to be considered for reaching a particular accuracy/convergence?

- Specify how many Fourier expansion terms had to be considered. How does the number of Fourier terms change with optical depth and with the anisotropy of the scattering phase function?

- CPU time of MSOS versus CPU requirements for other well-established radiative transfer solvers.

- Specify the single scattering albedo \omega as used in Section 4.

- Specify the aerosol optical depths at 460 nm and 550 nm for the AERONET sites A, B. This should then give the physical motivation for the optically dense aerosol situation.

3) p. 898: The retrieval of the refractive indices is not acceptable, because in principle all four parameters n and k for 550 nm and for 460 nm have to be considered. Here the authors only perturb k(460 nm). The retrieval procedure is thus not convincing. The authors are asked to treat the perturbations of the other free parameters, too, together with a discussion of the associated results/errors.

4) Section 4 and the scientific results: Regarding the satellite remote sensing aspect for the retrieval of geophysical parameters for biomass burning aerosols it appears that with MODIS alone no such retrieval would be possible, since there is no constraint for (n, k) at the two wavelengths (460 nm, 550 nm). Thus, AERONET ground-based measurements play a vital role to find appropriate restrictions for (n, k) for the single biomass burning case considered here.

In fact, the authors make no attempt to assess the errors of their retrieval approach. The situation for the error assessment is rather complex since, for example, errors for the AERONET products for (n, k) need also to be considered. In addition to this, uncertainties related to the "fixed" volume size distribution parameters (\r_f, \sigma_f),



(\r_c, \sigma_c) need to be specified together with their impact on the "retrieved" value for k(460 nm). It is recommended that the authors add more material that could support accuracy and reliability of their retrieval method, together with possible uncertainties of the proposed approach.

5) p. 884-885: Please describe the steps involved when going from the integro-differential equation (1) to the functional equation (3) which involves the source function J. What is the operator \Gamma_r as opposed to the operator \Gamma ? Please give the definition for the occurring operator(s).

6) p. 886, line 24: "... the replacement ... becomes unsafe ...". Please describe more clearly what is meant here.

7) p. 887, line 8: Nothing is said about the variable A. How can it be obtained? What is meant by "independent on albedo" ? Does "albedo" relate to the single scattering albedo, not to be confused with the surface albedo?

8) p. 888, Eq. (13), page 889, Eq. (18): It should be mentioned that terms of second order in \Delta have been neglected. Correct?

9) p. 891, line 4: The naming "method of successive order of scattering" is somewhat imprecise. The SOS (successive order of scattering) method as understood in the scientific literature is not limited to the semi-infinite medium. Please be more precise here, i.e. mentioning that the approach developed by the authors specializes the more general method to the particular case of a semi-infinite medium.

10) p. 894, line 8: There is no defining equation for the term d(\Omega, \Omega_0).

11) p. 897, lines 5-12: Please give more information on how the parameters (\r_f, \sigma_f), (\r_c, \sigma_c) have been estimated. Clearly make reference to the data of Omar et al. (2005) for the fine and coarse modes; also describe the procedure for the averaging; was the latter just the arithmetic mean, or was it something else? Please give a physically/mathematically based explanation how \sigma_f = 1.86 and

\sigma_c = 2.34 are obtained from the values given for \sigma_f,i, \sigma_c,i (i = 1, ...,6) in the authors' Table 1.

Minor comments:

1) p. 883, line 9: Please explain in which context "the AFGL code" is meant here. Do the authors make use of LOWTRAN to describe standard atmospheric "grids" (layering, pressure and temperature grid, aerosol optical properties and aerosol vertical layering) for running their own radiative transfer implementation (radiative transfer solver for semi-infinite atmosphere)? Please elaborate.

2) p. 883, lines 25-26: Please give some typical numbers for the aerosol optical depths at 460 nm and 550 nm, which justify the notion of a thick/dense aerosol atmosphere. Apart from biomass burning plumes the outbreak of mineral dust (such outbreaks might even cover larger areas) might also add a further example to this type of optically dense situation.

3) p. 889, lines 14-15: The reference is to (Mukai and Mukai, 1979)

4) p. 894, line 5: "(Uesugi and Irvine, 1970; Mukai and Mukai, 1979)" are the correct references.

5) p. 895, line 8: Typo "calculationsugnyway". Please correct.

6) p. 896, line 1: "play the sufficient role"; better would be "play the decisive role".

7) p. 896, line 3: The reference is (Mukai and Mukai, 1979)

8) p. 896, line 13: "blue (S1) and yellow (S2)" - Please specify the size of the squares S1 and S2. How many MODIS pixels are located within S1, S2? During the observations, were all these pixels without any cloud influence? How has the latter been ensured/checked? Is there a particular reason to just select these two locations from the much larger image in Fig. 2? Are additional AERONET stations available?

9) p. 896, line 14: "concerne3d" - typo; more appropriate would be "considered".



10) p. 896, line 20: "the observational error is less than 0.01" - Which quantity is meant in this context?

11) p. 897, line 7: "we adopted a determinant set" - "we adopted a fixed set for these parameters, namely ..."

12) p. 897, line 23: Typo "0.55g$\mu$m"

13) p. 899, line 15: Reference is made to a manuscript under review (Mukai et al. 2011) to which the reviewer of the present manuscript has no access. Please remove this particular reference, since its status is unclear/unavailable.

14) p. 904, Figure 1: What does "ex)" in the lowermost box mean?

15) p. 906, Figure 3: Case C cannot be found in this figure. Why? Please also correct the typo [R(lambda p] in the caption of this figure.

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