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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol absorption and radiative forcing" *by* P. Stier et al.

Anonymous Referee #1

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This paper discusses the sensitivity of aerosol absorption to refractive index, refractive index mixing rules, and surface albedo. It compares results with AERONET data. The paper also examines the forcing due to aerosol inclusions within clouds. The paper has potential to be a good contribution. However, significant revisions are required at this point.

The main shortcoming of the present study is that other studies have examined all the individual sensitivities examined in the present study to different degrees (e.g., refractive indices, mixing rules, surface albedo, absorbing inclusions within clouds), but not in the same paper. Whereas the present paper offers a new evaluation of the issues (and possibly more sensitivity tests), it is not clear what new scientific insight is gained, aside from different estimates. The authors should motivate better the reason why it is important to provide additional estimates of these parameters.

P. 7178. “an extensive evaluation of this base model can be found inĚ” The model appears to have been evaluated only at coarse global resolution and not at high resolution against field data. If this is correct, the authors should state so and remove the term “extensive” since global-scale evaluations are useful only for evaluating mean properties, not so much instantaneous, location-specific properties.

P. 7178. “Water vapour, cloud liquid water, cloud ice, and trace components are transported in grid-point spaceĚ” What is the implication of transporting cloud liquid water and cloud ice across course global grid cells? This would seem to indicate that clouds are spread across entire coarse grid cells, which is not realistic, and that numerical diffusion (which will occur with any transport scheme) will spread such clouds further. The authors then state, “Cloud cover is predicted with a prognositic-statistical schemeĚtotal water.” Does this mean that, on top of the cloud water transported, more cloud water is produced? The authors need to clarify the following: (1) How exactly are convective clouds versus stratus clouds treated, (2) Are convective clouds subgrid scale or grid scale, and how many convective clouds can form in a grid cell during a time step, (3) Are clouds formed then dissipated each time step (e.g., are they equilibrium clouds or do they grow and evolve and travel each time step), (4) Are clouds bulk (modal) or size-resolved (discrete size bins), (5) How do the bulk or size-resolved clouds interact with solar radiation? (6) How is cloud fractio calculated?

For balance, the authors should point out possible sources of error/uncertainty if the clouds are not subgrid scale (e.g., if one convective cloud forms per grid cell or if the clouds are treated with bulk microphysics rather than size-segregated microphysics).

P. 7179. “The microphysical aerosol module HAM predicts the evolution of an ensemble of seven interacting internally- and externally-mixed log-normal aerosol modes.” Zhang et al. (Aer. Sci. Technol. 31, 487, 1999) found that“Ěwith appropriate numerical algorithms and size resolution, a sectional representation can predict more accurate chemical composition and size distribution than a modal representation.” The authors should mention that the lognormal assumption is a source of error in the ECHAM-HAM

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model.

P. 7179. “The microphysical core M7 calculates coagulation among the modes.” This needs to be explained, since modes do not coagulate, particles of individual size coagulate. Please explain the treatment and mention that it is a potential source of error.

P. 7179. “Ěusing pre-calculated monthy mean oxidant fields.” The use of precalculated fields would indicate that the model cannot calculate the climate response completely. This should be mentioned as another potential source of uncertainty.

P. 7179. How are lognormal mode radii determined? Do they vary in time or are they fixed? Can chemicals dissolve from the gas phase into different modes, changing radii? Do they do so competeitively (e.g., do several modes compete for the same gas). Same question for condensation. Does chemistry occur within modes?

P. 7180. How have clouds been evaluated against data (e.g., cloud fraction, cloud liquid/ice, cloud optical depth, precipitation). Wet deposition depends on precipitation, for example. What evaluation of precipitation rates has been performed?

P. 7181. The authors should mention specifically which previous studies have tested which mixing rules on a global scale.

Section 2.2.3. Parameterisation of the effect of aerosol inclusions on cloud radiative propertiesĚ. The paper Jacobson (J. Phys. Chem. 110, 6860, 2006) examines the effects of absorption by soot inclusions within clouds and precipitation on global climate and compares the dynamic effective medium approximation of Chylek with the core-shell approximation of Toon. The authors should put their study of the effects of absorbing inclusions in context relative to this other study and discuss differences in treatments (e.g., with regard to size resolution versus modal treatment, for example).

Does the model treat the effects of absorption on snow or sea ice reflectivity?

P. 7190. “For all simulations, the AERONET evaluation of AAOD shows generally good agreementĚ” The agreement should be quantified numerically with statistics.

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P. 7190. How is absorption irradiance changes (W/m2) separated out from total irradiance changes?

P. 7192. The minimum and maximum surface albedos in Figure 2 (0.18 and 0.36) seem to be extreme variations. Why not use realistic variations in surface albedo?

P. 7195. The discussion of cloud radiative properties predicted in the present study should be compared to some extent with results from the Jacobson paper mentioned above.

P. 7197. From this evaluation we concludeĚprovide the best representationĚ" The authors should qualify this result to say that it is specific to the model used and its assumptions. Other models or changes in treatments of physical processes in the present model may yield different results.

P. 7199. "As previous studies indicated, the simulated effect of absorption by aerosol inclusions embedded in cloud droplets and ice crystals on the global radiation budget is small." The authors have not referred to any previous studies examining the global effect of embedded inclusions. The authors should specify which studies are being referred to.

Figure 7. The figures are too small to see anything useful. I suggest reduce the number of figures significantly and increase their size to illustrate a specific point.

Figure 8. This figure seems not to be so useful. I would suggest removing it.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 7171, 2007.