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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The evolution of the global aerosol system in a transient climate simulation from 1860 to 2100" *by* P. Stier et al.

Anonymous Referee #1

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Highlights:

1). Atmospheric aerosol system and oceanic biogeochemistry are coupled within the GCM to simulate the evolution of aerosol distribution from 1860-2100.

2). The residence time, aging time, and mixing state of aerosols are showed to be amendable to the climate change. Thus, estimation of aerosol forcing in long term can not rely on the emission projections alone.

Comments:

1) Precipitation is critical for the aerosol distribution and aging processes. It is not clear in the paper how the precipitation in a transient climate would affect the aerosol.

2) The description of aerosol optical properties (such as single scattering albedo) is not clear in the paper. Are they a function of aerosol mixing state? I doubt the significance of aerosol forcing computed in the paper, because aerosol forcing also depends on surface reflectance. How do you consider the land use in the paper? I would recommend authors not mention the aerosol forcing numbers in the abstract, because the uncertainties in these numbers are too huge to be quantified.

3) page 12786, near line 10. Why dust emission showed a significant increase while sea salt not? Are they both parameterized according to wind speed? If yes, is this difference because the wind speed decreases over the ocean while increases over the land in your simulation?

4) page 12792, line 25, "for technical reasons, areas with a change in surface albedo larger than 0.03 Ĕ is masked out"? What are the technical reasons? If your forcing calculation doesn't include these areas, you should mention that your forcing value is only a near-global averaged value. This again goes to the key point: how is the land use and surface processes modeled in your GCM?

5) If the aerosol tends to become more internally mixed in long term, then the overall aerosol single scattering albedo could increase also, because the internal mixing of soot (such as core/shell structure) has a larger single scattering albedo than external mixing (Ackerman and Toon, 1981, Applied Optics). Yet, your results are contradictory to this. You should have more explanation on how the aerosol forcing and aerosol properties are computed?

6) While this study primarily is a modeling study, I think there is still some validation that should be done easily. Otherwise, it is difficult to convince readers. I would like to see authors to present some comparisons between their AOD values with those from satellite (such as AVHRR or MODIS) or ground-based observations (such as AERONET). The comparison can be done either in monthly or yearly basis for 10 or 5 years, either in regional or global averages. Without a good simulation of current aerosol state, the

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number and the significance of this paper is very doubtful.

7) Given there are large uncertainties in the results of this paper (as authors acknowledged in the end of the manuscript), there should be a table that lists all the key components (processes) relevant to the aerosol simulation and describes the treatment of those components. For instance, aerosol first indirect effect (considered), precipitation effect on aerosol (?), evolution of surface reflectance on aerosol forcing (?), . . .

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 12775, 2005.