Atmos. Chem. Phys. Discuss., 5, S4939–S4942, 2005
www.atmos-chem-phys.org/acpd/5/S4939/
European Geosciences Union

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 #2

Received and published: 10 January 2006

This study represents a significant step in aerosol-climate modeling because it includes a new state-of-the-art aerosol microphysical scheme. It has the potential to offer important information to the climate community, however I think in several respects it fails to provide the information necessary to do so. My recommended revisions of the article are given below, grouped by topic, with most essential points marked by a "*".

1. Emissions.

1a. * The emissions used here are poorly documented, in the case of the historical emissions and in the case of the future carbonaceous aerosol emissions, and poorly regarded in the case of the future SRES A1B. As a result, this study has primarily

illustrative value of some possible impacts of aerosol microphysics on aerosol load and forcing. Throughout the text the study should be treated as such.

1b. * 12785. “Transient emission fluxes from 1860 to 2100 Ĕ for the SRES A1B scenario”? Isn’t the A1B scenario for future, not the past century? How are the past emissions derived? Provide more detail! Show in either a table or figure, the fossil fuel, biofuel and biomass burning emissions for each component as a function of time.

1c. Compare carbonaceous aerosol emissions with those of Streets et al (2004) SRES A1b estimates. Compare historic carbonaceous emissions with Ito and Penner (2005). Compare past SO2 emissions with van Aardenne et al. (2001).

1d. * I find the future increase (from present to 2050) in biomass burning especially difficult to understand, and this is crucial to the predicted aerosol evolution. Streets et al (2004) projected decreases in biomass burning during this period from their interpretation of the A1B scenario. Since open burning emissions don’t depend on technology-emission factor effects, there is clearly some important difference in methodologies that needs to be explained.

1e. Section 3.1 on emissions should perhaps be moved to the model setup, since it is not a “result”. The fixed dust source area might be justified based on Tegen et al (2004) which suggests a small, 10% anthropogenic component. Mention that historic eruptive volcanoes are not included among the interactive aerosols, according to Figure 1. Presumably they are treated as external climate forcing factors, but this is confusing. Also mention here in the emissions section that biogenic POM source is assumed constant.

1f. How large is seasonal variability for biofuel? I am not aware of published estimates of biofuel emission seasonal variability, more discussion is needed.

2. Impacts of aerosol microphysics on lifetime and burden.

2a. * Aerosol lifetime and burden are some complex mixture of aerosol microphysics and geographical shifts in aerosol emission. The importance of changes in the micro-

ACPD

5, S4939–S4942, 2005

physical aging time is not apparent since in many cases the aging time changes in an opposite sense to the change in aerosol lifetime (e.g. while the aging time decreases, the lifetime increases). For species in which the aging time is much smaller than the lifetime (e.g. BC and POM), I suspect that the change in aging time is not so important. In other words, if the particle is not likely to encounter a removal process (rain) within 5-6 days then it doesn't matter whether it becomes soluble in 1 day or 2 days. The situation might be more interesting in some regions. Also the dust story may be more complex. But I remain unconvinced that microphysics has a large impact on aerosol lifetime. The only way I can think of to prove an effect would be to repeat the experiment without microphysics. Discussion should be added regarding timescale impacts.

2b. 12788 A shift in emissions to lower latitudes (e.g. BC in 20th century) does not always mean shift to drier emission regions. For example, Asian emissions are generally in relatively moist regions.

2c *. In some models the indirect effect impacts aerosol lifetime. Does this model? Are both 1st and 2nd indirect effects included?

3. Impact of aerosol lifetime on radiative forcing.

3a. * It would be interesting to see this model's prediction of the degree of non-linearity between aerosol emissions and load or optical depth or forcing. I suggest adding figures showing the ratio between the load (or tau or forcing) and the emission as a function of time in order to quantify the degree of non-linearity.

3b. * I am confused about the assumed optical and radiative properties. How are the internally mixed aerosol properties determined? How much is the increase in absorption due to increased BC emissions? Again, this could be demonstrated via the ratio between tau_absorption and emission as a function of time.

4. Miscellaneous.

4a. Abstract. In accordance with comments above, I would place less emphasis on

ACPD

5, S4939–S4942, 2005

specific changes in optical thickness and radiative forcing. Provide more detailed information on the (percent) changes in lifetime, aging rate, mixing state. How non-linear is the relation between emissions and forcing?

4b. Section 2.6, 12784. How much do results for the 2 20th century realizations differ in their aerosol predictions?

4c. Figure 1: In the BC figure, the dashing in the curves do not correspond to that shown in the key. Also, the yellow under SO2 is hard to see, I suggest another color, like the darker yellow-orange used in later figures.

4d. Figure 2. The 2 reds at the top of the color scale look the same. Maybe make the top one lighter or purpler?

4e. The large predicted natural contribution to fine aerosol tau is interesting and pertinent. This could be pursued: how does the geographical distribution of this natural component compare with the satellite-based "anthropogenic" tau distributions?

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