

Interactive
Comment

Interactive comment on “A global modeling study on carbonaceous aerosol microphysical characteristics and radiative forcing” by S. E. Bauer et al.

Anonymous Referee #1

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This paper describes the results of a sensitivity study of the newly developed aerosol model MATRIX coupled with GISS GCM. The MATRIX model predicts both mass and number concentration of several aerosol populations. These quantities are thus used in the model to prognostically derive the size distribution based on a quadrature scheme. In the study, the authors have analyzed modeled aerosol radiative effects against assumed initial size of primary carbonaceous aerosols and mixing model of particles (core-shell vs. homogeneously internally mixed). Four cases with different reduction scenarios of carbonaceous aerosol emissions have also been conducted. Interestingly, the authors find that due to the effects of semi-direct and indirect effects none of these scenarios would bring a significant change in anthropogenic radiative forcing. The pa-

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per addresses an important issue regarding the uncertainty of aerosol radiative effects due to modeled microphysical processes and emission parameters. The research is also among a few such efforts using size-dependent aerosol module in global climate models. The following comments might serve the purpose to help the authors to improve their manuscript.

1. Certain details about the model and simulations are needed in the paper to allow the reader to better understand the results. How long did the simulations last? What were the forcings applied to the model other than that of aerosols? Did the authors use prescribed SST or a coupled ocean model? On the aerosol model part, several key findings of the paper could not be well understood without knowing the model structure (see following comments). Based on the previous paper, the authors used a so-called 1-point quadrature scheme (two moments, i.e., mass and number concentrations). This would be equivalent to a monodisperse model for each aerosol mode. Thus, how to derive a good characteristic size for microphysical and optical calculations (other than the simple arithmetic mean) of each mode becomes quite curious. The radiative/optical calculation of aerosol in quadrature scheme is often not the part of the quadrature solver itself and, therefore, handled using separate scheme for computational reason. The previous paper was not very clear about this. What are the major assumptions made in the radiative and optical calculations? Also, the authors mentioned in a few places that coagulation is an effective microphysical conversion for aerosols. However, this is quite a different viewpoint from the conventional knowledge that condensation is the dominant mechanism in aerosol growth. Is there any useful diagnostics to show the quantitative comparison of various sink and source processes of the key aerosol modes?

2. In the simulations against the size of primary aerosols, if the total emissions in mass were kept the same, then the change of geometric size (or any characteristic size used in calculation) actually represents a change in initial number concentration. Is the size factor more critical than the number as the authors argued or other way around? This

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is specifically interesting because of the result in CDNC as described in the paper. If the size were the primary factor, what would be the chemical/microphysical reason behind the change of CDNC? The current discussion seems not clear about this. For example, the statements of “Smaller initial OC/BC sizes lead to more coagulation between sulfate and BC” (p. 4553, Line 4-5) and “Larger initial OC/BC particles lead to less internal mixing of the aerosol populations” (p.4554, line 23) seem suggesting that the coagulation between carbonaceous aerosols and sulfate would change the number concentration of hygroscopic particles. This perhaps is not true because such coagulation combines a hygroscopic particle (sulfate) with a non-hygroscopic particle (carbonaceous) to form a new hygroscopic particle unless the model assumes that the sulfate-carbon mixture cannot be CCN. Therefore, the process itself should not change the number of hygroscopic aerosols.

3. In the mixing-state simulations, is this true that the model is actually processed with a homogeneously internally mixed scheme (from Table 1 and experiment setup), hence the core-shell is just an optional way in radiation calculation? Otherwise, the microphysical processes for these two types of aerosols are somewhat different.

4. In the emission reduction simulations, it is not quite clear that why the CDNC change with emissions in the BCOCBF occurred differently than in other cases.

5. Specific comments:

p. 4544, line 4: “direct, cloud-indirect and semi-direct forcing effects”, please use conventional terms here, also forcing and effect should not appear together.

p. 4545, line 15: “Most absorbing, . . .”, please rewrite this sentence. Line 27: “1.4- and 13 fold”, seem to me a typo here?

p. 4546, line 7: “pre-industrial and present day” to “present day and pre-industrial”.

p. 4547, line 22: “How important . . .”, please indicate “to what” or “in what” here to complete the sentence. Line 24, does “constrain our models” sound better than

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“constrain our simulations”?

p. 4553, line 22: “mixing concentration” to “mixing ratio”.

p. 4559, paragraphs before section 5: it seems to be the case that in the MATRIX model, BC and OC are always mixed with sulfate, nitrate and perhaps others. Is this the reason that BC is always in a small fraction in any of those BC-containing modes?

p. 4562, second paragraph: was AAOT of OC and mixtures part of the result?

p. 4568, line 11-14: when discussing warming the temperature change should be provided, otherwise, “radiative forcing” should be used instead of “global warming”. Line 21, should “und” be “and”?

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