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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Improvement and further development in CESM/CAM5: gas-phase chemistry and inorganic aerosol treatments" *by* J. He and Y. Zhang

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Reply to Reviewer 2's Comments

This paper discusses a collection of improvements relevant to the representation of aerosols in CESM/CAM5. The present paper represents a large collection of efforts and as such is an important contribution to the model development. However, the use of CESM in a fully coupled climate mode, while allowing for full climate feedbacks, also makes the comparison of very short simulations meaningless. The main reason is that two simulations that are slightly different (whether form initial conditions or slightly dif-

ferent forcings/emissions/chemistry) will generate meteorologies that are significantly different, but this only represents the inherent noise of the climate system. In the configuration used in this study, simulations of at least 20 years (and probably quite longer since some of those changes are relatively minor) would be necessary to start seeing differences that are above the natural variability of the system. As written, the paper cannot be considered for publication. I therefore suggest that the authors focus on the chemistry aspect of the study, and perform short simulations in which the meteorology is not affected by the changes in chemistry, or simulations with fixed SSTs (which would probably need to be on the order of 5-10 years). The length of the simulation should be defined such that the response in the system is above the natural variability of the reference case. Since I find that the paper will have to go over major revisions, I have only included a few additional comments.

Reply:

We thank the reviewer for constructive comments. While we agree with the reviewer that results from longer time simulations would be more robust, we believe that the 1-year simulations provide useful information for model development and sensitivity study. The differences among those 1-yr simulations are indeed mainly caused by changes in model treatments, rather than the inherent noise of the climate system. This was verified by performing t-test for simulation pairs with different model configurations.

To address the reviewer's comments, we performed 5-yr simulations with two different gas-phase chemical mechanisms (SIM and CB05_GE) both with prescribed SST. Those results are added in a new section (i.e., Section 6), Tables 6-7 and Figures 9-10. In addition, we performed a 5-yr simulation with the CB05_GE gas-phase chemistry using fully-coupled CESM/CAM5 and compare its performance with the same configuration but with prescribed SST. The results are shown in Tables 6-7 and discussed in Section 6.

Other comments

1. The paper would benefit from the addition of simple diagnostics such as global budgets and lifetimes

Reply:

Since the removal rates of some species were not included in the CESM/Cam5 output, it is not possible to accurately estimate the global budgets and lifetimes. To address the reviewer's comments, we have calculated global burdens of major chemical species, and compared them with those of previous studies in Section 6 and Table 6 in the revised paper.

2. Section 2.2.2: it seems that a major limitation in the evaluation of various methods is the unavailability of observations. Please comment.

Reply:

Despite limited data, observational data of new particle formation rates (J) are indeed available for evaluation of model predictions. As described in Section 3.2, we used the collected observations from Kulmala et al. (2004) and Yu et al. (2008) for model evaluation. Such data exist during different years over different regions. The evaluation results are shown in Table 4.

3. Section 2.2.4, line 23: where does the HCl come from?

Reply:

Simulated HCl mixing ratios result from HCl emissions and gas-particle partitioning of total chloride. This has been indicated in the revised paper.

4. Page 27728, line 24: Please add table with emissions

Reply:

Table 2 in Zhang et al. (2012) provided such detailed information regarding emissions used in this work. We believe that the authors are discouraged by any journals to repeat

the same information that has been published. To address the reviewer's comment, we have referred the readers to this table in Zhang et al. (2012) in the revised paper.

5. Section 4: tables 3 and 4 contain too much information. It would really help to provide this information in an easier format, maybe through bar graphs.

Reply:

While bar graphs can generally show well model performance, many bar plots are needed for the evaluation that we performed over different regions for many meteorological variables and chemical species. For some variables, the differences in performance statistics are small, which may not be clearly displayed in bar graphs. Considering the above reasons, we believe that the table summary of the performance statistics over different regions for many variables is the most appropriate and concise format to present our results.

6. Page 27731, line 5: what is the role of optical properties and water uptake on AOD biases?

Reply:

The aerosol optical properties are defined for each mode of the MAM based on Ghan and Zaveri (2007). Uncertainty in hygroscopicity of aerosol components and treatments of water uptake can result in uncertainty in AOD prediction.

To address the reviewer's comment, we have added above explanation in the revised paper.

7. Page 27735, lines 29-30: what are the % with respect to?

Reply:

The percentage differences are with respect to MAM_SIM. This was indeed indicated in line 25, "compared with MAM_SIM,. . ." in our ACPD paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 27717, 2013.