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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

Anonymous Referee #1

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This paper discusses the outcome of an improvement by the authors to the aerosol model (the Modal Aerosol Model 7 or MAM7) of the Community Earth System Model (CESM). The authors have successfully incorporated several sophisticated components to replace the more simplified counterparts in the current MAM7/CESM. These include a gas chemistry package, an aerosol thermodynamics module, and a new ion-based aerosol nucleation module. Besides, the authors have also performed a sensitivity simulation using altered emissions. The effort is to assess the performance of the CESM model after the introduction of these new aerosol and chemistry components.

I admire this effort that covers many detailed aspects of model prediction of aerosols

and tropospheric chemistry. On the other hand, however, the paper is mostly about model development rather than science findings. Noticeably, all the model components introduced into the CESM have been developed previously and mostly used in other frameworks, thus what the paper describes is the outcome from coupling these components with a different model framework. This would still be invaluable. The work has provided an arguably better platform than the current CESM in modeling aerosol and chemistry interaction with climate. The evaluation of such a model improvement needs to be recorded in literature. A relevant question here is whether the authors would want to consider journals that specifically solicit works dealing with model techniques and evaluations such as Geoscientific Model Development. If not, I would suggest the authors reconsider the scope of the paper and focus more on science features in discussion. My (most general) comments here are for the authors to consider when making their revision, either for ACP or resubmitting to another journal.

The authors used the fully coupled CESM configuration to run their simulations. This is a configuration includes a full ocean GCM coupled with the atmospheric model along with other components in a transient mode. Surprisingly, with this configuration, all the simulations were integrated for only one year. In the discussion of modeled results, the authors spent quite an effort on changes of many climate variables from surface air temperature to wind speed. Arguably, this is not adequate because of the short integration time and very long ocean response. Note that the model was cold started and forced by an introduced new and different forcings than the standard 1850-2000 run (it is understood that the last output of this run was used as initial condition for the simulations). In my opinion, the emphasis of the effort should be, as stated by the authors, to assess the aerosol and chemistry predictions introduced by the new model components. Such an effort, however, would likely suffer from the first year response of modeled transient climate after a cold start, which involves interactions between aerosol, air chemistry and meteorology/climate as well. It would be much better if the authors use a configuration using prescribed SST, run simulations for at least three years, and then remove all the irrelevant discussions of climate variables (the changes

are hardly to identify anyway from Table 3) from the paper but those of chemistry and aerosol features.

The improvements in terms of aerosol and chemistry model predictions are mostly limited in comparison to observations (see Table 4), while the conclusion drawn in abstract and Conclusion section about this is much too optimistic to me. Realizing the very high demanding in computation of this new model, the best purpose this effort could serve is to provide certain information for the improvement of simplified schemes, because practically it would come to the usage of the latter types to conduct aerosol-chemistry-climate interaction simulations. Therefore, the emphasis perhaps should be on the relative differences between various model configurations.

There is an argument that the emission uncertainty could be the reason for much of the remaining problem of the new model. This is also too simplistic to me. By introducing those sophisticated model components, the uncertainty is multi-dimensional (a good example is the scale inconsistency of fast chemistry in the current model). In addition, the sensitivity simulation using alternative emissions were not clearly explained in the paper.

The current discussions appear to have too many leads, while detailed connections among various chemical or physical processes were not clearly analyzed. For example, simulations of tropospheric chemical features seem being improved much significantly over Europe than other places, the reasons behind this, however, has not been explained or stated clearly. More quantitative comparisons between precursors and products in different continents (could be selective) would help (note that the information in Table 4 is incomplete, see specific comment).

Several specific comments.

1. How many variables are prognostic and included in tracer advection? This would be useful for the reader to estimate the workload of the new model.

2. How were the photolysis rates calculated, using any J-table (note, not the nucleation rate)?

3. P. 27720, L17-19, "...mass accommodation coefficient...can be measured...", the authors might want to indicate the high uncertainty in this type of measurements, as being demonstrated later in the paper by a difference in orders of magnitude in the adopted values of sulfuric acid and others.

4. P.27728, it may help if the authors could further explain the derivation of the initial condition for the model, was the ending result of B1850-2000 run used?

5. Section 3.3, although the evaluation procedure might have been explained in detail in the cited publication, it would still be useful if the authors could explain briefly about this procedure, for instance, whether the data of 2001 or multi-year average data were used in the comparison, etc..

6. P.27732, Section 4, how did the modeled aerosol results in the MAM_SIM differ from those in Liu et al. (2012)?

7. Many of the gas chemistry comparisons were done by comparing the new model results with those of MAM_SIM, this is not informative in my opinion. Many of the chemical fields in MAM7 are prescribed using climatological data derived from MOZART model or alike based on my understanding, and this is done for reason. The authors should compare the chemistry features to the "mother" model of chemical fields used in MAM7, such information would be useful for the aerosol modelers to decide whether they should use alternative climatology, and if not, which aspect in the current climatology need to be improved.

8. P.27733, the last paragraph and other places, is SO4= here in aerosol or aqueous phase?

9. P.27735, L20, "The aforementioned changes...", changes due to what?

10. P.27746, L25, "CCN... cmˆ(-2)", is this referred to the column loading?

11. P.27747, last two lines and following, “Large biases . . .due to uncertainties in model input (e.g., meteorology and emissions). . .”, this deserves a thorough analysis, or at least citations to support this statement.

12. P.27749, 5.6; it is not quite clear that what emission inventory the authors had used in this simulation, and what the differences are between the alternative emission inventory and the default one. If the authors selectively adjusted emissions for certain species, what are these species then and how was the adjustment done and based on which works?

Table 4. Noticeably, the listed regions for each variable are not always the same, for example, East Asia numbers were only listed for CO and SO2, especially not for ozone and many other gaseous and aerosol species. The authors might want to list the results from a same collection of regions.

Figures. The color scale in several figures was not always selected to show the necessary details, e.g., Fig. 7 and 8.

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