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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The global aerosol-climate model ECHAM-HAM, version 2: sensitivity to improvements in process representations" *by* K. Zhang et al.

Anonymous Referee #1

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General comments:

Impacts of different parameterization changes in HAM2 on aerosol- and climate-related results from ECHAM are analysed. The basic approach in the paper is useful given large uncertainties that still exist for the representation of aerosol effects in climate models.

Unfortunately, the discussion of some features of HAM2 in the paper is confusing. Several parameterizations are emphasized as important model improvements in the abstract and elsewhere. After reading descriptions of these parameterizations and analyzing model results the reader is informed that these parameterizations are not

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actually used in HAM2. This includes the cluster and kinetic nucleation schemes in the boundary layer (p. 7553) and various changes for aerosol wet deposition (p. 7561). Finally, a different activation scheme is apparently used in HAM2 instead of the scheme which is described in the paper (Lin and Leaitch, p. 7563). No further information about this aspect of the model is provided although an important purpose of HAM2 is to provide improved modelling capabilities for climate.

Model components that are not used in HAM2, or which are still under development, can be better described in separate publications with a specific focus on certain model shortcomings and strategies for improvement. This would permit a more complete and convincing analysis of these parameterizations. On the other hand, it would be useful to include additional results for particulate organic matter in the paper. An interesting aspect of HAM2 is the replacement of the highly idealized treatment of organics in HAM1 by parameterizations for chemical and microphysical processes. Does this modification improve the agreement between simulated and observed concentrations for organic matter?

The discussion of model results in the paper generally lacks quantitative information. The analysis of impacts of parameterizations on model results is limited to a description of simulated concentration patterns etc. It is often difficult to decide whether a new parameterization leads to actual improvements in model results from the results that are shown in the paper.

Specific comments:

As explained in more detail in the following, the model overview is lacking detail. This is in contrast to the expectation of a synthesis paper according to title and abstract. Individual - sometimes developmental - components of HAM2 have been described in various other papers and so a sufficiently complete and detailed summary needs to be provided in this paper.

How is HAM implemented in ECHAM? What is the horizontal, vertical, and temporal

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resolution of the model? The model time step is important for the results (e.g., p. 7555, l. 5).

How are aerosol tracers affected by convection and other mixing processes in ECHAM?

Basic assumptions about aerosol chemical composition and size in HAM need to be clarified at the beginning of section 2. It is mentioned that particles are internally and externally mixed but this is not explained. What exactly are the mixing assumptions that are made for different types of aerosol?

How many tracers are advected in the model and what is the numerical approach for tracer transport? Perhaps Fig. 1 should be included here and used for the description of the basic approach?

According to Fig. 1, aerosol species can be soluble or insoluble. This needs to be explained. How are these defined and what are the relationships between these?

It would be beneficial to include a reference to Table 1 in the description of sea salt and dust parameterizations and other parameterizations on page 7550 because this table includes information that is relevant to the description of parameterizations in this section.

P. 7551, l. 17: How are mixing state and size of the particles accounted for in the radiation calculations? Are separate radiation calculations performed for each size mode in the aerosol scheme? References?

How do aerosols affect microphysical and microphysical properties of clouds?

What emissions are used for primary particles and what are the size and hygroscopic properties of the emitted particles?

P. 7552, l. 18: "The responses...are significant...and are consistently seen..." This statement is out of context. Such a statement should be made after the discussion of the actual results, if appropriate.

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P. 7554, l. 6: An important assumption for the parameterization of gas-to-particle conversion in HAM1 and HAM2 is the operator splitting between condensation and nucleation. This approach will work well if either condensation or nucleation rates are low. However, given that nucleation is so non-linear and that the time scales are often quite short for condensation and nucleation compared to the model time step, it is difficult to see how this approach can provide accurate results when nucleation and condensation occur at the same time. Although numerical errors may not be apparent for simulated aerosol mass, production rates for aerosol number concentrations are much more sensitive to numerical errors. How confident are the authors that results presented for HAM2 in Fig. 2 indeed provide evidence for model improvements, as claimed? Given the use of operator splitting techniques in HAM1 and in HAM2, this is questionable. Have the authors considered to use shorter time steps to investigate numerical errors of the operator splitting methods? Why are no results shown for nucleation mode number concentrations in the lower troposphere in Fig. 2, where aerosol number is important for CCN concentrations? Also, there are numerical methods available that do not require operator splitting. Has this been considered?

P. 7556: The partitioning of mass between the gas- and particle-phase for organic matter in HAM2 is not clear. Does this involve a numerical solution of the condensation equation, similar to condensation of sulphuric acid? What accommodation coefficients are used?

P. 7556, l. 22: What is the reason for the large difference in SOA yield between AeroCom and HAM2?

P. 7557, l. 25: This approach has been shown to produce large errors for the aerosol water content at low relative humidities (Kreidenweis et al., Environ. Res. Lett., 2008). How does this affect comparisons with the approach in HAM1 and the accuracy of radiation calculations in HAM2? It seems that these parameterization biases may contribute to the reduction in aerosol water content (P. 7558, l. 5)? Also, a figure should be included here for illustration of the differences, perhaps for zonal mean aerosol water

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content.

P. 7559, l. 24: It appears that dust emissions increase by a factor of 4 for only a few grid points. Increases for concentrations and aerosol optical depth seem to indicate weaker increases in Asian dust emissions than a factor 4?

P. 7560, l. 20: How is R used for mixed-phase clouds in the model? Is the same value of R used for the liquid and frozen portions of the cloud condensate or are there differences for in-cloud scavenging of aerosol through warm- and ice-phase processes in mixed-phase clouds? The coupling between HAM2 and the cloud microphysics scheme in ECHAM needs to be explained in more detail.

P. 7561, l. 6: What is the difference in global mean aerosol optical depth in Fig. 6a?

P. 7561, l. 26: The title of this section is misleading because effects of cloud microphysical processes on aerosols were already considered in the previous section.

P. 7562, l. 28, Fig. 7: Observations are available for cloud radiative forcings and should be included in this figure. Without the observations, it is not clear whether the model changes lead to improved model results or not. Some of the changes do not appear to be beneficial for the simulated cloud radiative forcing considering climate biases in ECHAM. On the other hand, it is not clear why results are included for the cloud radiative forcing in the paper because aerosols typically have relatively small effects on overall cloud radiative forcings compared to other, often very uncertain, processes. For instance, it may be possible to tune the autoconversion efficiency to produce much better agreement in results in Fig. 7. Hence, it would be better to remove or replace this figure.

P. 7563, l. 10: It is not clear why the absolute autoconversion rate should be considerably smaller? Since the mean precipitation rate is similar this would imply that the accretion rate is much greater? On the other hand, increases in cloud water path likely point at a reduced *efficiency* of autoconversion. Perhaps it would be possible to

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include more quantitative information, e.g. a cloud water budget? Furthermore, an increase in cloud water path may lead to increased in-cloud oxidation of sulphur species. Is there any evidence for increased in-cloud production rates?

P. 7564, Table 5: The dry deposition rates are considerably lower in HAM than in the AeroCom models. Add an explanation and a brief description of the dry deposition scheme in HAM.

P. 7565, Fig. A1: More quantitative information needs to be included, e.g. mean concentrations for all sites. There is no further discussion and so it is not clear why this figure is shown in the paper.

P. 7567, l. 6: The authors state that the aerosol number concentration is very well simulated but no evidence is shown. An integration over the size distributions would likely reveal large differences for the aerosol number concentration. Furthermore, the double logarithmic scale that is used in the figure is problematic. For instance, the total number and maximum of the size distribution are underestimated by about a factor of 10 for Ispra for DJF in Fig. 12. It should also be mentioned that differences between HAM1 and HAM2 are small compared to differences between model results and observations.

P. 7567, l. 17: What sites exactly are considered to be affected by heavy pollution here?

P. 7567, l. 25: Underestimates for aerosol number concentration in polluted regions and overestimates in more remote locations are to be expected because it is unlikely that the model fully resolves the horizontal scales of aerosol plumes. Insufficient horizontal resolution likely explains this kind of concentration biases. Note the similarities between simulated size distributions at different sites.

P. 7568, l. 10: Another factor that might affect this comparison is the location of the observations in the vertical. What is the height of the observations and corresponding model results in the figure? Perhaps the vertical resolution is too low in the model to

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sufficiently resolve the rather shallow layer of sea salt aerosol above the surface of the ocean? Simulated concentrations for the coarse mode seem to be systematically lower than the observed concentrations. Is this consistent with other results? According to these results (i.e. unrealistically low concentration and large particle sizes in HAM), one would expect the model to severely underestimate the aerosol optical depth over the ocean. However, the opposite is the case (Fig. 15), which indicates an inconsistency either in the model or in the way model results are compared with observations.

P. 7568, l. 24: These comparisons are very qualitative. At a minimum it will be necessary to include global mean values for a more quantitative assessment of model accuracy. An even better approach would be the additional use of Taylor diagrams.

P. 7569, l. 18: Again, more quantitative information needs to be provided in addition to merely describing differences in patterns in Fig. 16.

P. 7571, l. 8: How do these results compare to other estimates in the literature? If there are differences, what may be causing these differences and is there any indication that HAM2 produces better results than HAM1? Furthermore, no information is provided about the accuracy of the simulated relative humidity, which plays an important role for radiative forcings and aerosol optical depth.

P. 7572, l. 3: No evidence is provided in the paper to support this statement.

P. 7572, l. 14: Correct the reference.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7545, 2012.