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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Aerosol–cloud interactions studied with the chemistry-climate model EMAC" *by* D. Y. Chang et al.

Anonymous Referee #3

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Review of "Aerosol–cloud interactions studied with the chemistry-climate model EMAC" by D. Y. Chang, H. Tost, B. Steil, J. Lelieveld

The study by D. Y. Chang et al. attempts to compare two cloud droplet nucleation parameterizations which differ in their representation of the activity of water in solution in the chemistry-climate model EMAC and a reference model setup without a coupling between aerosol and clouds. The outcome of such an activity would be interesting and welcome but due to several shortcomings such as the missing tuning of model parameters for the different model setups, the study does not present results that would allow a sound comparison. I cannot recommend the manuscript to be published in the present form.

General Comments:

1. Model parameters need to be tuned when the model is changed so that the shortwave, longwave and net fluxes, shortwave and longwave cloud radiative effects, cloud cover, liquid water path etc. are within observed ranges. Only then can model results that were produced with different parameterizations be compared to observations. Some simulations in the paper show unrealistic values for global annual mean cloud properties. Large parts of the analysis and conclusions in the paper are not meaningful because of this lack of model tuning. Only after tuning would the attempted assessment of the performance of the different simulations be possible.

2. There are indications that there is an implementation problem in the STN-simulations (cloud droplet nucleation parameterization which uses the osmotic coefficient to describe the activity of water in aqueous solution). There is a difference of approximately a factor 2 between the activated aerosol fraction and CCN between the STN and HYB simulations. This difference is the same for aitken, accumuluation and coarse mode i.e. independent of size although the solute effect should be more important for smaller particles i.e. the difference should decrease with particle size. The activated aerosol fraction in the lowest model levels is close to 1 for aitken, accumuluation and coarse mode, CDNC concentrations in the lowest model levels are very high and the global, annual mean CDNC burden is very high.

3. The unrealistic cloud properties in the STN-simulations are mentioned in the paper but no sufficient explanation is provided.

4. Section 2 should be extended to describe also the methods used in (e.g. Taylor diagram, skill score) and the observational data used. This information is now in different parts of the paper and not enough details for the observational data are provided.

Specific Comments:

P21976, L12: The difference in the cloud radiative effects can also be the results of the lack of tuning of the model for the different setups.

P21977, L26: The reference to Penner et al., 2004 does not fit to this sentence.

P21978, L3: The semi-direct effect refers to absorbing aerosol heating of the atmosphere below clouds, interstitially between cloud droplets or above clouds. The changed absorption by cloud droplets or ice crystals by uptake of aerosol is the in-cloud droplet and ice crystal absorption effect.

P21978, L15: Provide here a range for the forcing of AIE.

P21978, L19: Aerosol or aerosol-cloud-interactions are not mentioned in the cited papers.

P21979, L9: This sentence is unclear.

P21979, L24: How substantial are the discrepancies among the solute effects in Rose et al. (2008)?

P21981, L19: The double moment cloud microphysics scheme (Lohmann et al., 2007) uses prognostic variables for cloud droplet number and mass (as well as ice crystal number and mass).

P21983, L9: Unclear. In M7 the modes are constrained by reallocating aerosol particles that exceed the upper size limit of a mode to the next larger mode.

P21984, L7: Remove Abdul-Razzak and Ghan (2002) as there a parameterization for cloud droplet formation in sectional models is described.

P21985, L16: It should be mentioned here that the focus of the paper is on liquid clouds.

P21986, L6: Which cloud microphysics scheme is used in the REF simulations?

P21987, L5: This sentence is unclear. The parameterization by Khairoutdinov and Kogan (2000) describes the formation of precipitation from cloud water/droplets (auto-

conversion) and accretion of small droplets.

P21987, L7: parameterization instead of Parameter

P21987, L24: What are variances of humidity in this context?

P21987, L27: Do you mean vertical and horizontal resolution?

P21988, L4: Which climatology for SST and SIC is used?

P21988, L14,L17: For which years?

P21988, L19-26: For these three objectives tuning of model parameters is necessary for the different model setups.

P21989, L13: Provide details and references for the MODIS, CloudSat and CERES satellite products.

P21989, L23: Provide details and a reference for the ISCCP satellite products.

P21990, L4: It should be stated clearly that the STN and HYB setups are referred to as aerosol-cloud coupling or aerosol-cloud interactions throughout the text.

P21990, L6: Why are the vertically integrated CDNC twice as large in the STN-simulation as in the HYB-simulation?

P21990, L7: Unclear. Does the REF-simulations use CDNC(ICNC) in the cloud microphysics calculations?

P21991, L20: Is this definition of cloud radiative effect also used for the model variables? Or are the cloud radiative effects of the model calculated as the difference between all sky and clear sky radiative transfer computations?

P21992, L19: Which years? Provide a reference for the MODIS observations.

P21992, L20: Is the difference in AOD between the simulations only due to different deposition rates or also due to changes in the emission rates?

P21995, L7: Is the overestimated sea spray burden due to overestimation of emission rates or underestimation of aerosol removal?

P21996, L24: Which global mean values? Why are the results presented only for three modes?

P21997, L4: Is the computation of vertical velocities the same for both CDN parameterizations.

P21997, L7: Why are almost all available particles activated in the lowest model levels in the aitken, accumulation and coarse (soluble?) modes in the STN-simulation? Such high activated fractions would indicate an aerosol limited regime but this is unrealistic for global mean values.

P21998, L4: Why is the sensitivity to the choice of the parameterization for the activity of water in solution so much higher than in Pöschel et al. (2009)?

P21998, L20: Activated fractions are shown in Fig. 5 up to an altitude of 18 km. Are there liquid (mixed-phase) clouds at this altitude in the model? Are the activated fractions computed from the model supersaturation or are they diagnosed at a specific supersaturation?

P21999, L21: Why are the STN-simulations so insensitive to the aerosol composition?

P22000, L20: Are the aerosol types shown dominant by mass or by number?

P22000, L23: The continental regions could have more meaningful names e.g.: North Africa, South Africa, Europe, South East Asia, South America, Arctic and Siberia.

P22001, L13: To which region(s) does this sentence refer to?

P22001, L15: CRE depends on more than aerosol composition. In particular different meteorological conditions in CR2 and CR5 may be responsible for differences in the cloud radiative effects in CR2 and CR5.

P22003, L7: To which region does this sentence refer to?

P22003, L23: How good are the observations of CRE for region CR4 and CR6?

P22004, L11: A description of how the Taylor diagram and the skill score are computed should be put in a method section e.g. which areas are compared at which time spans, which observational datasets are used etc. Also the description from the supplement for Taylor diagram and skill score should be incorporated in the manuscript.

P22005, L10: Are desert areas included?

P22008, L6: Without tuning of the model (and fixing possible implementation problems) for the different setups such a conclusion cannot be made.

P22008, L11: The distribution of cloud droplet number concentrations is not compared to observations. Why is it considered to be realistic in the HYB-simulations?

P22026: It should be mentioned that the approximation for sckappa,i≈exp((4 Aˆ3/27 kappai Diˆ3)ˆ0.5) for the HYB-scheme is valid for kappa values >0.2 and critical supersaturaions <1%. What is Di? There is a factor 2 difference in A between the STN- and HYB-schemes as in STN the aerosol particle radius and in HYB the aerosol particle diameter is used but the same symbol is used for both schemes. For the above conditions Bi and kappai should be almost equivalent. How do the values of Bi and kappai values compare to each other for the same aerosol distribution and composition? How large are the discrepancies between the critical supersaturations computed with the STN- and HYB-schemes? Table S1 should be incorporated in the manuscript. P22028: Shortwave, longwave and total net flux top of the atmosphere should be shown in Table 5.

P22036: Specify which satellite data are shown in Figure 3.

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