**Atmospheric
Chemistry
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Discussions

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

Anonymous Referee #2

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In their manuscript "Aerosol–cloud interactions studied with the chemistry-climate model EMAC", the authors present a series of studies with the chemistry-climate model EMAC in which they vary the aerosol activation and cloud cover schemes. The focus of the study are the differences between simulations using a standard representation of Kohler theory using osmotic coefficients and an implementation based on Kappa-Kohler theory. The authors find significant differences in simulated climatological fields of cloud properties, precipitation and radiative fluxes across their simulations and conclude on "best" model configurations based on comparison with a range of observational datasets.

Unfortunately, the study fails to attribute the large differences between the simulations to specific physical or chemical effects. The presented analysis is entirely focused on

global model results, which do not help to understand the huge discrepancies – CDNC burdens using the different activation schemes differ by a factor of 4-5, much more than one would normally expect from composition effects assuming corresponding choices of Kappa values and osmotic coefficients. Furthermore, many differences in the results appear to be attributable to different model configuration in different tuning states, which are no reflection of the actual processes of interest. I therefore cannot recommend publication of this manuscript in ACP and limit my comments to major issues.

Major issues

- The differences between the different activation approaches are huge. No attempt is made to explain this in appropriate detail. As presented, implementation errors or inconsistencies in the choices of kappa and the compositions used for the selection of osmotic coefficients seem at least as likely to explain the differences as an actual "chemical effect". Unless this fully explained, the presented analysis of climate variables and the related conclusions are irrelevant.

- There exist a number of well-defined test cases that have been used to validate activation schemes with detailed parcel model results (see e.g. Ghan et al., 2011) but no attempt is made to test the used implementations against such test cases. Due to the large differences, it will not be possible to validate both schemes. The fact that the description of the Abdul-Razzak Ghan scheme ("The calculated SC_κ is applied to the parameterization of the water condensation rate (dw/dt) of the activated droplets in STN and the hygroscopic growth is then defined by" Eq 2.) seems to suggest that Eq. 2 is solved, while the supersaturation estimation in this scheme is in fact empirically formulated from parcel model simulations, does not add confidence in the implementation.

- Clearly, the different base model configurations are in different tuning states.. Attribution of improved agreement of the model to specific activation or cloud

cover schemes is fairly arbitrary, as they will depend on the initial tuning settings. Superior agreement in climatological parameters can only be attributed to specific model parameterizations after retuning – in other words, structural improvements become only evident after parametric uncertainty has been reduced as much as possible.

- A large part of the manuscript is devoted to difference due to different cloud cover schemes. Issues with this scheme are well documented. Citing Stevens et al., JAMES, 2013: "This scheme includes prognostic equations for parameters of the assumed distribution and yields a realistic present day climatology, but is not used in standard integrations because it generates a very strong climate sensitivity due to behavior that appears unrealistic, but is not well understood."

- The overall presentation of the results is not sufficiently robust and detailed. To give just a few examples: observational datasets are only loosely referred to and cannot be attributed (e.g. "MODIS"); ice nucleation of aerosol is eluded to in the model description and never mentioned in the analysis; the representation of updrafts, key for aerosol activation is not even discussed; Other parts are confusing, such as Figure 1.