Interactive comment on "Aerosol–cloud interactions studied with the chemistry-climate model EMAC"

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Response to reviewer 2

We thank the reviewer for the constructive and valuable comments, and will revise and improve the manuscript soon as your comments.
In response to the comments:

General Comments

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 is 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.

→ We apologize for insufficient explanations for the differences of aerosol activation and cloud properties and parts of the conclusions. We admit our failure to describe coherent conclusions based on the simulated climate variables. We wanted to present to what extent cloud properties and climate parameters can be generated with different critical supersaturation algorithms (i.e., osmotic and κ- Köhler method). The intention of the manuscript is to provide a sensitivity test. In the revised manuscript we will refocus on the investigation of physicochemical aerosol effects on cloud droplet nucleation processes rather than testing different cloud cover schemes, which distracts from the main message. Furthermore we will provide box-model calculations to demonstrate the differences in Sc calculations and explain how these propagate into large differences in cloud properties in the PBL. A more detailed description of Sc calculation and of implementation of ARG in the model will be added.

• 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 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.

→ As you mentioned, there are many studies to evaluate parcel model results with well-defined test cases of ARG cloud droplet nucleation parameterization. Ghan et al. (2011) also presented various models, from cloud-resolving to global models, which have applied the ARG cloud droplet nucleation parameterization. Therein, table 3, 9 global models applying the ARG parameterization are listed (e.g. CCM1, CAM-model family, HadGEM-UKCA, etc.). We have attempted to apply the well-validated aerosol activation scheme in our EMAC model (GCM) to simulate aerosol cloud interaction and try to improve EMAC model simulations of clouds and climate. Also the κ-method has been applied in EMAC model and evaluated with observations (Pringle et al., 2010b).

In fact the manuscript is about how sensitive cloud-aerosol coupling is towards Sc calculation. We agree that because of the large sensitivity of model-results on Sc more details concerning the implementation of ARG in EMAC are needed and we will also provide box-model calculations for Sc.

• 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.

→ We agree with your comment that improved agreement between the model results and the observational data was not clearly attributed to aerosol activation or cloud cover schemes. To avoid the confusion about which scheme causes which effect, we will discuss only the RH-simulations (i.e., RH-STN and RH-HYB) in the revised manuscript. As your comment, depending on tuning states tuned climatological parameters could be better, however which is not our purpose of the present study. Our model results are a kind of primary test before tuning model parameters that we can be aware of magnitude of propagated impacts generated by different Sc.

• *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."*

→ **We aim to address the activated aerosol effects on clouds and climate, and acknowledge that this can be influenced by the choice of cloud cover scheme. We agree that the discussion of different cloud cover schemes distracts from the main objective of the manuscript. Therefore, we will exclude some distracting comparisons and analyses such as the ST-simulations (i.e., ST-REF, ST-STN, and ST-HYB), i.e., and focus on the RH-simulations (i.e., RH-STN and RH-HYB), and will more convincingly discuss the differences related to the droplet activation schemes.**

• *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.*

→ **We will provide more detailed descriptions for the model results and observational data, and important parameters relevant to aerosol activation (e.g., the representation of vertical updrafts).**