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Interactive comment on "A global off-line model of size-resolved aerosol microphysics: II. Identification of key uncertainties" *by* D. V. Spracklen et al.

Anonymous Referee #1

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This paper examines the impacts of some microphysical processes and primary emissions on simulated CN and CCN number concentrations using a recent global aerosol microphysics model. As the issues discussed in this study have not been explored in such a systematic way in the published literature, it should be published after some revisions.

Some general comments are: (1) It is a sensitivity study by design. The bounding cases do not necessarily reflect the uncertainties in the current understanding of the processes. Instead, they are largely hypothetical. Readers should be cautioned about

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this. (2) This paper is the second in a series of three. It would be more appropriate to put out such a sensitivity analysis as the third, after model description and validation.

The specific comments are: (1) The three-month simulations start from Oct. 1. It is conceivable that the meteorological fields are quite different in other parts of the year. Can this change the conclusions? Please comment. (2) On p.3447, the sentence starting with “Comparisons with measurement would look poor even if Ĕ” is not meaningful to me. If a model gets the maximum wrong, then it is plainly wrong. The usage “look poor” is confusing. (3) The panels in Fig. 5 are not labeled or described in the caption. Which ones are polluted or clean are not clear when the figure is first cited. (4) Aren’t nucleation rate and critical cluster size determined by the same factors (e.g., concentration, temperature, etc.)? If so, why are they treated as separately in section 5.1? (5) On p.3449, it is interesting to see that despite more clusters, you will end up with less particles larger than 3 nm because of longer growth time. This conclusion may strongly rely on how accurate the coagulation efficiency of clusters composed of 10 - 100 molecules. I would suspect it is fairly uncertain in that regime. Please comment on this and caution the readers if necessary. (6) On p.3450, how low cloud is when activation is considered? Is that warm liquid cloud or warm liquid plus mixed-phase cloud? (7) On the same page, the sentence starting with “Effectively, we are simulating Ĕ” is confusing. Do you actually use different supersaturations? That is not what I read from other parts of the paper. (8) On p.3452, please comment on the shortcoming of assuming a constant so-called effective scavenging diameter and how the base line value is chosen. Is the nucleation scavenging rate scaled against precipitation rate? (9) Why is sulfur emission varied by only 25%, as opposed to 10 times for sea-salt? Is there any way to justify such a different treatment?

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 3437, 2005.