

Interactive comment on “Assessment of parameterizations of heterogeneous ice nucleation in cloud and climate models” by J. A. Curry and V. I. Khvorostyanov

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The authors may have some misunderstanding about the MPACE_B case. I just want to clarify a few things here. 1) The aerosol measurements (two modes of 72.2 and 1.8 cm⁻³) for MPACE_B were obtained by a CN counter from NOAA CMDL, which were surface measurements (see the documentation for the intercomparison of this case: <http://science.arm.gov/wg/cpm/scm/scmic5>). So, they are not cloud residuals or interstitial aerosols as the authors claimed. The composition was recommended to use ammonium sulfate.

2) If there is any soluble fraction, which is the premise of the KC scheme, in the

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aerosols of the coarse mode with mean radius 1.3 microns, they will immediately become droplets in cloud at RH of 100%.

3) The droplet number concentrations for the flight 9a were generally around 80-100 cm⁻³ (up to 120 cm⁻³; see Fan et al. 2009) and the total aerosol concentration measured at surface is about 140 cm⁻³. So we can see that most aerosols were CCN in the MPACE cloud case. The general CCN/CN ratio in the Pruppacher and Klett (1997) is nothing to do with this specific case. We are curious how droplet nucleation was done in the simulations of this study. What CCN concentrations did you input for this case? Since you assumed the specified aerosols as IN, then how did you specify CCN in the model? Why can the coarse mode aerosols not be CCN in cloud?

4) Based on our knowledge, Hobbs, Rangno and team measured high ice particle concentrations, not IN concentrations.

5) Your recommendation is valid. Although we did emphasize that the measured IN concentrations from the CFDC measurements were used in Fan et al. 2009, we should have stated something like “the used classical theory follows the T-Sw dependencies as in the KC scheme but with the measured IN from the CFDC measurements as the input”.

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