Review of Revision of "Secondary ice production affects tropical convective clouds under different aerosol conditions" by Sun and Colleagues

A current limitation of the manuscript is that the microphysical realism of the model is not demonstrated as clearly as it could be, given the largely process-oriented conclusions drawn from the simulations. While the paper provides useful comparisons against radar reflectivity and radiative fluxes across the different sensitivity experiments, there is comparatively limited use of available in situ observations to evaluate the baseline model state.

In particular, aircraft observations from the TWP-ICE/ACTIVE campaigns appear to be under-utilised in the main text for model evaluation. This is important because these datasets provide valuable constraints on both aerosol and cloud microphysical properties, and comparable model validation has been demonstrated in earlier work (e.g. Kudzotsa et al., 2016 and associated PhD thesis) for similar simulation periods, including cases overlapping with the 6 February 2006 storm examined here.

It would strengthen the manuscript to re-name one of the existing simulations as a clearly defined "control simulation", with a corresponding evaluation section that compares its model output against key observational constraints from aircraft, satellite, and ground-based measurements. At present, the simulations are primarily presented as a set of sensitivity experiments, without first establishing a baseline level of agreement with observations for a standard version of the model.

Specific additional evaluation metrics that would be valuable to include are:

- **Ice particle number concentration:** Comparison between the control simulation and aircraft-derived estimates, as already demonstrated in prior studies of the TWP-ICE/ACTIVE case. Where possible, it would be helpful to consider conditionally averaged comparisons over cloudy regions (e.g. non-zero ice water content), even acknowledging the sampling limitations of upper-tropospheric measurements.
- **CCN and aerosol constraints:** ACTIVE provides CCN and aerosol measurements that could be used to evaluate whether the simulated cloud droplet number concentration is consistent with observed aerosol–cloud relationships, particularly given the central role of prescribed N_d in the model formulation. Yes active CCN concentrations are not used to predict droplet number in the present simulation, but it is still possible to relate in convective cores the prescribed droplet concentration to an implied CCN concentration (e.g. the Twomey formula for a certain cloud-base ascent). Yes, there are no INP observations in ACTIVE/TWP-ICE but it is still possible to use the mineral dust

loadings observed to infer with a dust-aware INP scheme the active INP concentration for comparison with the Cooper scheme used (which was not based on measurements anywhere near Darwin).

- **Cloud droplet number concentration:** Dornier observations in ACTIVE include in situ cloud measurements (e.g. FSSP-based droplet concentrations in relevant cloud-intersecting segments, such as in weak cloudy stratiform ascent). These could be used to evaluate the model's representation of N_d in shallow cloud or stratiform boundary-layer conditions.
- **Cloud fraction and vertical structure:** Following previous TWP-ICE/ACTIVE analyses (e.g. Kudzotsa et al.), comparisons of vertical cloud fraction profiles would provide useful context for assessing the simulated cloud field.
- **Radiative fluxes:** While domain-mean radiative quantities are shown, additional direct comparison with observational estimates would further strengthen confidence in the simulated energy budget.

In total, incorporating these elements would likely require only a small number of additional figures (on the order of ~2–3 multi-panel figures), but would significantly improve confidence in the model's baseline realism and therefore in the interpretation of the sensitivity experiments.

Finally, a more explicit discussion of the treatment of heterogeneous raindrop freezing (e.g. Bigg-type parameterisation) would be helpful. In particular, clarifying how this formulation relates to the assumed INP activity elsewhere in the model would improve physical consistency. Some recent approaches (e.g. Waman et al.) explicitly link raindrop freezing to INP concentrations, and it may be useful to discuss how the present approach compares to these frameworks. The current discussion of SIP processes (around line 660) would also benefit from a more quantitative comparison with these studies.