<u>Review of "Secondary ice production affects tropical convective clouds under different aerosol conditions" by Sun and Colleagues</u>

Major Comments

The study is stimulating to read. There are some fascinating findings about SIP impacts on deep convection, especially with the apparent effect on latent heating and vertical motions. The updrafts seem to be invigorated by SIP in this deep convection.

Yet, regarding realism, the paper is not very convincing because there is no validation of the ice concentration, which is the topic of the paper, nor of the LWC. It is the ice concentration that the SIP determines, and all the effects on the cloud from SIP occur via the ice concentration.

Yes, I guess there may have been little sampling of the convective cores by the aircraft in ACTIVE. But validation of the convective outflow is still possible in the weak ascent.

Yes, before 2011, there were no tips on the aircraft probes. But it is still possible to validate the filtered ice concentration for ice particles larger than a certain size (e.g. about 0.4 mm), after correcting the data with the Field et al. (2006) inter-arrival time method (see Figure 5 of Korolev et al. 2011). Moreover, the supercooled LWC is coupled with the ice concentration in the mixed-phase region, and the LWC can be validated easily.

So the question arises, how do we know the predicted ice concentrations shown in the sensitivity tests are realistic ?

For this reason I recommend major modifications to the paper, with addition of validation of cloud properties such as LWC and ice concentrations against any coincident aircraft data from flights on the simulated day in ACTIVE. And similarly, predicted active IN concentrations, if in situ measurements exist, should be validated too.

Also, it is unclear how raindrop-freezing is treated and whether it is related to the IN conditions. This is pertinent as the study funds a large role for raindrop-freezing fragmentation.

Detailed Comments

1. Introduction

Line 11: you could also mention that tropical deep convection redistributes many other tracers, like aerosol species and trace gases. It governs the vertical gradient of temperature and humidity in the troposphere, and determines where the tropopause is, by the radiative-convection equilibrium.

An atmosphere without deep convection would be almost impossible to imagine.

Line 44: It might be a good idea to insert "as reviewed by" before the references given since the studies by Field et al., 2017; Korolev and Leisner, 2020; Huang et al., 2022 are not observational studies per se and are merely functioning as review papers effectively here.

Line 75: Where it is written "Huang et al. (2025) linked CCN impacts to changes in SIP and subsequent storm electrification", the two-part paper by Phillips and Patade (2022) did this as well.

2. Method

2.1 Model Description

Line 105: How do you treat ice-ice aggregation ? Is the treatment consistent with Connolly et al. (2012) observations of the aggregation efficiency and its temperature dependence ? Hopw do you treat sticking efficiency for aggregation ?

Line 112: Is raindrop-freezing in collisions between supercooled raindrops and ice crystals represented ? Chisnell and Latham (1976) and others subsequently predicted that it must be essential. Blyth et al. (1997) and Hallett et al. (1978) observed supercooled rain in association with ice multiplication subsequently, consistent with the Chisnell and Latham prediction.

Amazingly, some models in the past have completely omitted collisional raindrop freezing (E.g. RAMS).

Line 114: I am guessing there might be a typo here: "Homogeneous freezing of cloud droplets occurs below –38 °C" . In fact, homogeneous freezing of cloud-liquid happens over a narrow range of temperatures, depending on droplet size, with most cloud-droplets (about 20 microns) freezing at about -37 or -36 degC. See the plot in Pruppacher and Klett (1997). Do you mean "below the -38 degC level "?

2.2 Model set-up and design of the simulations

Line 160: Where it is written "with a model time step of 75 seconds", I wonder if there might be a typo here. The model referred to here seems to be the cloud-resolving model of resolution 1.5 km. But that time-step seems unusually coarse.

Let us assume there is no typo. The horizontal Courant number would be 30 m/s (peak horizontal velocity, such as around the gust front) divided by the numerical solution maximum speed of 1500/75 = 20 m/s. So the Courant number would exceed 1. But I guess the semi-Lagrangian scheme with ENDGame (semi-implicit for some physics) and numerical limiters (van Leer limiter?) avoids the CFL condition for numerical stability. Parcels in the model can go by more than one grid-spacing per time-step with Lagrangian treatment.

So, even though the CFL condition for stability does not apply to such schemes, accuracy with such long timesteps must be an issue. Microphysical processes in reality happen on faster time-scales than 75 sec (e.g. precipitation fallout, vapour diffusion).

With the vertical motions, updraft speeds of 20 or 30 m/s and a fine vertical resolution (e.g. 200 m) would imply vertical Courant numbers of more than 10.

Is there robustness of the cloud statistics predicted with respect to the choice of time-step ? One would expect the variability inside the storm to be underestimated by the model, with peak heating too weak and peak updrafts too slow, with such Courant numbers.

2.3 <u>Observational data</u>

Bizarre that there is no mention of aircraft flights. There were two planes in ACTIVE, which must have sampled layer-cloud properties even if they did not sample the convective cores above the freezing level. Most of the ice particles in the convectively generated layer-cloud are probably generated in the convective cores.

Some validation against aircraft data needs to be shown for the simulated cloud properties for the range of vertical velocities observed.

Why include aircraft in a field campaign when the modellers do not use the aircraft data to compare their models with ? How do we know the ice concentrations are accurate ?

And if SIP is being modelled, why is there no comparison of the predicted IN concentrations with any in situ CFDC observations ? Or was there no CFDC deployed in ACTIVE ? How do we know Cooper (1986) is applicable to that day and location ?

Imagine that for whatever reason, the primary ice is drastically under-predicted: then the SIP would likely be over-predicted, since the ice multiplication tends to continue until a maximum ice concentration is reached near the onset of water saturation.

Need to make note of the cloud-base temperature. What does that imply about the balance between warm-rain and ice-crystal processes in the contributions to surface precipitation ? Gupta et al. (2023) found that the warm rain process prevails (80%) in simulations of Brazilian tropical deep convection.

3. Results

3.1 SIP impacts on radiation under varying CCN conditions

Line 196: These simulation names are introduced for the first time without any explanation. What is the difference between allSIP-200 and allSIP-400 ? Is it the aerosol concentration ?

Line 217: weaker enhancement of what ?

Line 224: "broader vertical extent" would be better written as "deeper vertical extent". Similarly, "a broader yet optically thinner distribution" should be "a deeper yet optically thinner distribution". "Broad" has connotations of horizontal coverage, whereas I think you are talking about the vertical aspects of distributions.

Line 231: It is written that "Overall, Nd = 400 cm–3 shows a better agreement with the observation". Surely, it is possible to say if aircraft data from the case are consistent with this droplet number ? Need some validation of cloud properties in situ.

Figure 2: the caption seems wrong. Are all the panels showing OLR, with (a)-(c) being observations and the rest the model ? You have not used visible imagery at all here because the entire figure is about the longwave imagery. Delete "visible".

3.2 SIP impacts on precipitation under varying CCN conditions

Line 286: It is written that "SIP accelerates glaciation, shifting water mass from the liquid to the ice phase during the early storm stage, reducing the efficiency of warm-rain production and suppressing peak rates." More detail here would be good. Is the idea that supercooled liquid is accreted onto extra ice precipitation (boosted by SIP) that falls relatively slowly and melts to form smaller drops that are more likely to evaporate before reaching the surface ? Or is it that the extra crystals from SIP cause the supercooled liquid to evaporate in regions of weak ascent, so that less liquid can be accreted onto warm rain ?

3.3 SIP impacts on cloud microphysics under varying CCN conditions

Line 363: It is written that “Because warm-rain processes dominate in clean air”. But how do you know this to be true ? There is no tagging tracer for the component of precipitation from the warm rain process.

4. **Conclusions**

Line 607: You could compare the relative abundances of SIP mechanisms with Deepak Waman’s papers from 2022 to 2024. He shows some budgets from using tagging tracers for components of ice concentration from various SIP mechanisms.

It is worth saying that the tropical cloud-base, coupled with an optimal aerosol concentration, can cause many supercooled raindrops to be upwelled aloft, which then supports raindrop-freezing fragmentation. What is the order of magnitude of the supercooled raindrops above the freezing level ? If there are hundreds per Litre, that would explain why raindrop-freezing fragmentation is so prolific here.

It is unclear whether the model resolves heterogeneous raindrop-freezing and collisional raindrop-freezing separately. If it does, does the model relate the heterogeneous raindrop-freezing to the IN activity (e.g. by modifying the Bigg scheme) ?