Reviewer #2

Reviewer: The authors have addressed most of my concerns. In particular, the validation now compares size-filtered model ice concentrations with the corresponding filtered aircraft observations, substantially improving the interpretation of the model-observation comparison. The additional validation and revised discussion of the dynamical effects also strengthen the manuscript. The description of the role of sublimational breakup is now more nuanced.

While I do not necessarily agree with every response to my comments, I believe the manuscript is almost ready for publication. The only criticism I would make is that the authors should make their claims more modest and objective in one or two places where comparison with the observations reveals a model bias. For instance, Waman et al. write " Furthermore, the simulated total INC agrees well with the coincident observations of NI400 from CIP and PIP (Fig. 6f), with their means differing less than ± 40% at all sampling levels." But inspection of the plot reveals the simulation is an order of magnitude smaller than the observations. "So "their means differing" must be simply referring to the two sets of aircraft observations. It gives the wrong impression that somehow the error of the simulation is only 40%. Also a brief explanation for why the CAIPEX microphysical properties plotted (e.g. droplet and ice concentrations) are much too low would be a good idea in the results section.

Also, there is the wrong use of the word "total" when applied to plotted ice concentrations. If only the component of ice concentration for particles greater than a certain size is being shown, then this can be referred to as a "filtered" concentration rather than a total one.

Overall the paper is elegant and makes a salient contribution to knowledge in cloud physics, combining the best modeling technology and the state-of-the-art satellite and aircraft observations. It is impressive to see ice concentrations from the new EarthCARE satellite being used for validation.

So I recommend publication subject to minor revisions.

Response: We thank the reviewer for their efforts in reviewing the manuscript thoroughly and providing a diligent review. We also appreciate the comments and suggestions given. It would help us to improve the quality of the manuscript. Below are point-to-point replies to the reviewer's comments.

Reviewer: For instance, Waman et al. write "Furthermore, the simulated total INC agrees well with the coincident observations of NI400 from CIP and PIP (Fig. 6f), with their means differing less than ± 40% at all sampling levels." But inspection of the plot reveals the simulation is an order of magnitude smaller than the observations. "So "their means differing" must be simply referring to the two sets of aircraft observations. It gives the wrong impression that somehow the error of the simulation is only 40%.

Response: We appreciate the reviewer's suggestion. The text in the revised manuscript is now modified accordingly (at lines 372-374) as,

"Furthermore, the filtered INC (NI_{400}) observed from CIP and PIP agree with each other to within ± 40% at all sampling levels (Fig. 6f), while the simulated NI_{400} is about 70% lower than these observations at relatively weaker updrafts ($w > 1$ m s^{-1}, Fig. 6f). In regions of higher convective updrafts ($w > 2$ and > 4 m s^{-1}), this agreement improves further (supplementary Fig. S4)."

Reviewer: Also a brief explanation for why the CAIPEX microphysical properties plotted (e.g. droplet and ice concentrations) are much too low would be a good idea in the results section.

Response: We thank the reviewer for this suggestion. A brief explanation is now added in the results section (at line 370-374) for both the droplet and ice concentration underestimation.

In the revised manuscript, the modified text now reads as:
..." whereas the simulated CDNC is about 80% lower than the observed values (Fig. 6d), likely because the CCN activation scheme of Segal and Khain (2006) may not represent the high aerosol loadings over the Indian subcontinent. Furthermore, the filtered INC (NI_{400}) observed from CIP and PIP agree with each other to within ± 40% at all sampling levels (Fig. 6f), while the simulated NI_{400} is about 70% lower than these observations at relatively weaker updrafts ($w > 1$ m s^{-1}, Fig. 6f). In regions of higher convective updrafts ($w > 2$ and > 4 m s^{-1}), this agreement improves further (supplementary Fig. S4)."

We also note that the broader uncertainties, particularly in SIP representations in numerical models are already discussed in the concluding paragraph of the manuscript (line 710-716).

Reviewer: Also, there is the wrong use of the word "total" when applied to plotted ice concentrations. If only the component of ice concentration for particles greater than a certain size is being shown, then this can be referred to as a "filtered" concentration rather than a total one.

Response: We thank the reviewer for this careful observation. We have replaced "total INC" with "filtered INC" throughout the manuscript (especially for CAIPEEX in Sec. 3.1.1) wherever ice concentrations for particles greater than a threshold size are being referred to, to clearly distinguish them from the total ice number concentration across all particle sizes. We note that for the ORCESTRA case, "total INC" is retained as the EarthCARE CPR retrieval provides total ice number concentration without any size filtering.