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Interactive comment on "Controls on phase composition and ice water content in a convection permitting model simulation of a tropical mesoscale convective system" *by* C. N. Franklin et al.

Anonymous Referee #2

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The authors compare simulations of a tropical MCS observed during a recent airborne field campaign with the in situ measurements between 0 and –40 C, where liquid water and ice could coexist (although there appears to be no liquid in the observations). There is substantial uncertainty as to how MCS updraft microphysical processes operate in nature, and improving process-level knowledge is a worthy research goal within the scope of ACP. Observations from multiple campaign flights have been reported by Leroy et al. (2015), as cited, but this appears to be the first analysis of the relationship of dynamics and microphysics observed during a flight. Overall, I am an interested reader, but I found it difficult to maintain attention on such a long paper for several rea-

sons. First, it appears that the baseline simulation simply does not capture the event well at all, contrary to the authors' claims (in the abstract for instance), and sensitivity tests have similar errors across the board (e.g. Fig. 15). Second, several aspects of the observations appear notably odd (such as large updrafts without any additional ice content), but the authors focus on narrow elements of the observations without explaining why such apparent oddities are present. These factors combined make it difficult to be interested in nearly twenty figures comparing the simulations and observations, and even lead this reader to feel that the sensitivity tests may be futile or ill conceived because the simulations are so far off the mark. Below I suggest the major steps that could help develop this manuscript in my estimation. Minor comments are then listed in case they are helpful.

Major comments

1. The MCS evolution in observations and simulations needs significantly more description. Highly averaged satellite data in Fig. 3 indicate that there are plenty of images that could be shown to us to see what OLR evolution looks like in the observations and the simulations. The reader needs to see these to understand if the simulated system appears far too large (in addition to being far too cold on top) compared with the observations. Is this a system coming off the ocean in observations and simulations? Is it of a similar size and duration? I would recommend showing OLR images before, during and after the aircraft sampling times used in this paper, both observed and simulated. It feels decidedly odd that these were omitted. This needs to be remedied and re-reviewed.

2. It is difficult to continue this review without understanding how the system simulated relates to the system observed in terms of overall shape and top OLR structure. Right now, it appears to me, based on the figures shown, that the observed system is weak (Fig. 11), with low cloud top heights and surprisingly warm OLR (Fig. 3). Is this even an MCS? The simulations on the other hand do look like an MCS in terms of OLR and updraft strengths, but cloud top height seems low to me for a tropical MCS at 12.5 km

with hardly any change with time. How is cloud top height defined in the observations and simulations? How do the underlying structure of cloud top heights observed and simulated compare, and what is the uncertainty in differences of definition between observations and simulations?

3. I will continue by assuming that the observed system is a small, weak system and the simulated system is a big, strong MCS, as appears to be the case from all indications in Figs. 3 and 11. Moving to the objective of this study, the title of the paper refers to phase composition, but this topic is not clearly explored. Only Figs. 13 and 14 (really one figure together) show liquid water content as a function of updraft velocity, but as far as I can tell there are no measurements of phase and no other analysis of phase. Can the authors explain why they chose to focus on phase composition and why with this data set and this case study in particular? Also, what is a "high ice water content"? The updrafts shown here seem to have low ice water content. The authors refer to some other papers, but those seem to be focused on radar reflectivity.

4. The authors seem to view this modeling study as an exercise in manipulating their simplified microphysics (primarily) to better agree with the observations (unsuccessfully I would say) without investigating whether processes are actually likely to be active based on the observations. For instance, the absence of an observed bright band leads to a suggestion that particles are heavily rimed. (I think a tropical MCS should have a bright band, which to me seems another indication that the observed systems is not really an MCS. If the authors had a bright band simulator, I expect the simulated case would have one.) Later, graupel is removed from the model. What do the observed particle images look like? Do they indicate heavy rime? Is graupel observed? Leroy et al. (2015) show particle images, so I assume that they exist for this flight. Please describe what is known about the hydrometeor particles based on the flight data.

5. Past literature on updraft microphysics seems to be largely ignored, as do particle size distributions themselves. The last sentence of the paper concludes that there is a need to better represent the "observed bimodal ice size distribution" but we are never

shown a size distribution in the paper, either observed or simulated. How do we know that either observed or simulated are or are not bimodal and that this is important?

6. The concern of the authors with model dynamics is likely well founded. Some discussion of past model resolution studies would be helpful. Question: why bother with this exercise if the resolution of this model is too coarse to properly represent the updrafts observed, given that such updrafts are the only location where phase composition is interesting? If the authors do believe that the updrafts are grossly misrepresented dynamically, why spend so much time examining details of what occurs within them microphysically? Do the authors have evidence that this model is adequate to sufficiently represent uprafts being compared with observations? Why should I not conclude that this is the wrong tool to study phase composition in a tropical MCS?

7. Throughout the abstract, broad claims are made that are not clearly limited to these particular simulations. For instance, the last sentence of the abstract states that "... the entrainment and buoyancy of the air parcels is controlled by the ice particle sizes, demonstrating the importance of the microphysical processes on the convective dynamics." I think the authors mean in this particular system simulated by their particular model, which does not appear to resemble the system observed as far as I can tell. The statements made in the abstract need to be more carefully delineated to refer to their particular model with coarse resolution and one-moment microphysics, especially given the apparently poor resemblance of results to observations in almost every way shown (e.g., updrafts, reflectivities, OLR, ice mean size, ice water content, and ice water content versus updraft strength). I credit the authors with showing these myriad flaws of their simulations (that is truly useful), but I would be more interested to see conclusions related to what model factors need to be changed to improve the simulations rather than conclusions about whether ice size controls updraft strength, given the unrealistic nature of the simulations.

Minor comments

1. Page 8, line 14: How well are cloud bases observed by satellite? Cloud base throughout this system is at 3 km? That seems quite high to me for a tropical MCS. Over ocean?

2. Page 9, line 32: CloudSat IWP uncertainty is less than 25%?

3. Page 11, first paragraph: There is a lot of discussion of divergence and convergence here, but to me the peaks above 15 km in Fig. 5 look like oscillatory gravity waves. What evidence do the authors have that the peaks in motion above 12 km are not dominated by oscillatory motions?

4. Page 16, line 1: Both rain and ice appear bimodal to me; could they be related to one another?

5. Figure 15: These observations need some explanation. There is a 10 m/s updraft with less than 90% RHI between –20 and –30 degrees C? Is there a problem with the observations? Fig. 12 shows IWC remaining low to 15 m/s at 0 to –5 degrees C. I think a section should be devoted to noting and explaining such features when these observations are first shown. Are they somehow atypical? Is this strange strong updraft(s) associated with some aspects of the chaotic and odd diameter trends shown in Fig. 17?

6. I found it difficult to follow and maintain interest after the jump from Fig. 12 to Fig. 16 on page 20.

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