Responses to Reviewers, "Seasonal Controls on Isolated Convective Storm Drafts, Precipitation Intensity, and Life Cycle As Observed During GoAmazon2014/5".
Giangrande et al., https://doi.org/10.5194/egusphere-2022-877
Submitted to Atmospheric Chemistry and Physics (ACP).
Responses prepared by: S.E. Giangrande

General Comments:

We would like to thank the reviewers for their comments and suggestions. We have attempted to address many reviewer concerns, several having changes that can be incorporated into this manuscript. We have noted where an answer is possible, however some of these questions/efforts are (or best intended as) a subject of ongoing investigation (i.e., lack of sufficient data, or speaking on these themes may be early/rushed for inclusion, etc.). We have combined our responses to the two reviewers in a single document. Our responses are provided in ***bold/italics*** text. The authors note that they also have made a few (minor) changes/edits (these will be included in our tracked changes) to the Appendix and text in response issues / notion errors found by an internal review, by non-reviewer communications made possible from the online posting, and other checks to the manuscript in the intervening period.

Detailed Responses to Reviewer 1:

General Remarks: This study examined the variability in cloud features that develop and propagate over DOE-ARM GoAmazon2014/5 campaign observation site (T3) and SIPAM radar system. The cell features are tracked with well cited track methodology with a few alterations. While numerous past studies have investigated the seasonal-composite cloud properties, this study analyzes the cell life cycle using radar cell tracking with the addition of complimenting observations datasets. The results of this study indicate that there was a difference in the mean lifetime of cells that form in the wet and dry seasons. The DCC updrafts were found to be more intense during the dry season and generally DCCs reached peak intensity earlier in their life cycle. The usage of the simplified updraft model was an appropriate addition to further investigate the physical reasons for differences in the draft characteristics. The physical explanations describing the results were sound.

This manuscript was well written and has significant findings regarding deep convective cell development during the wet and dry season in the Amazon. Overall, this manuscript is of scientific value because it characterizes the life cycle of convective cells in various environments, analyzes the evolution of draft properties, and uses profile-based vertical air velocity information to further the understanding of deep convective cell dynamics. It is recommended that this manuscript be accepted with minor revisions.

We thank this reviewer for their time and detailed comments. We hope we have addressed their questions (and provided a few additional details) with our responses.

Major Comments:

1. Is it possible to use statistical resampling methods to increase the sample size of your cells in the wet and dry seasons?

This is a good question. Increasing the sample size (select resampling, esp. for RWP displays) was something the authors considered prior to our submission. For a few considerations, we presented the ARM observations without resampling, or much sub-sampling and/or tests for signature robustness. Mostly, we considered subsampling aimed at avoiding nonindependent data overwhelming cumulative properties, focused on ensuring single events/hits included consistently. A different approach we also considered prior to submission was subsets wherein we randomly removed events, withheld extremes, varied CFAD thresholds, to also lend confidence in these relative signatures. One simple example is provided below, as relevant to this reviewer for a later question on CAPE/CIN controls. Here, we partitioned the Wet/Dry season according to median CAPE/CIN (so, approx half samples in each). This particular breakdown may be associated with added physical insight, however it is useful as a demonstration that when many key observations are removed, we often would suggest similar CFAD-relative properties (here, even if the half removed are potentially among the strongest/weakest).

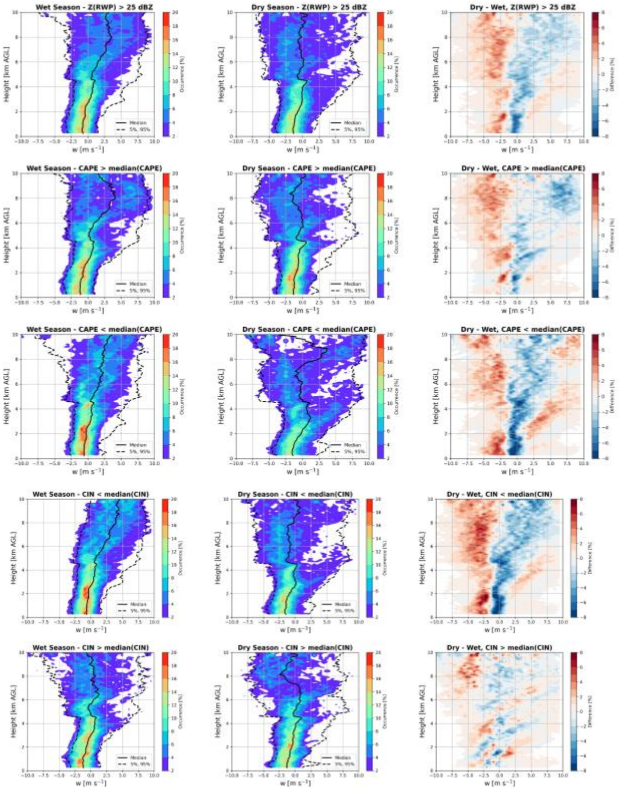


Figure. Example of CFADs for higher/lower CAPE/CIN median values.

While not a focus of this study, the authors could be tempted to suggest from these displays that stronger (more negative) CIN events possibly show exaggerated proposed Wet and Dry conditions and contrasts (if convection fires, it is stronger) – That is, we find stronger-relative convective events overall (to our previous plots), but also enhanced Dry - Wet contrasts (stronger Dry updrafts below, Wet aloft) under limited 'hit' conditions with stronger CIN. However, the absence of strong properties (and weakest displays of contrasts) during low CIN conditions may reflect ambiguity stemming from overall weaker convective behaviors (both seasons, more ubiquitous cells). Similarly, it is tempting to suggest that higher CAPE subsets show stronger Wet season properties, including indications of stronger W aloft, updraft and downdrafts; However, since Dry season storms seem to be impressive whenever conditions they are present, it may not impact the relative Dry - Wet differences as when relative behaviors are sampled from our lower CAPE events (where Dry cells may be strong if they initiate, but Wet cells are more ubiquitous/weaker). Ultimately, these are second-level questions we think are useful to address in ongoing work that could dive deeper into the specific events and nature/goodness of the hits for increasingly smaller numbers of events / case studies.

2. With the criteria used to define a cell of interest in this study, how many cells were removed for failing one requirement?

Another good question; Overall, 'tracking' properties with radar and/or generalizing lifecycle sensitivity is contingent on cell selection and duration.

In this study, we focused on (i) the longest-lived cells (resiliency requirement) that we argued the SIPAM radar was (ii) suitable for sampling (aka, Z threshold requirement). The need for (iii) daytime/isolated is also a limiting/tricky requirement, e.g., how to handle 'split/mergers'. Finally, we focused on cells enabling reasonable (iv) samples for dynamics ('overpass'/hit requirements). We note in the manuscript there may be additional unintended consequences, i.e., 'Wet' season storms that meet these requirements may represent a more extreme behavior (i.e., less representative of the average cell in the population) than 'Dry' storms.

To the reviewer question, our requirement for longevity (i.e., cells survive a significant # SIPAM scans for its lifecycle to be 'trackable') is important – a majority of trackable cells meeting Z thresholds do not survive more than 30 minutes. Thus, short-lived cell counts can be excessively large and dominate composite behavior (contingent on Z/season, "hundreds of thousands" of qualifying objects). It is perhaps more informative to speak in terms of the most stringent requirement (to the authors) that was that we required that our cells 'hit' the RWP; This greatly reduces the number of possible cells. These counts were on the order of 1400 cells (wet) and 300 cells (dry) for all events - this includes split/merge situations, shorter-lived cells, and some eventual MCS-type situations. Thus, our curated dataset is a reminder of how difficult (order of 25-50 total cells per season per year) it can be to capture these events in a favorable location.

3. How is echo top height defined? Is it 18 dBZ or another threshold?

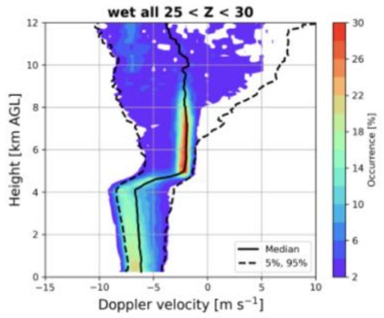
The authors used the height where Z drops below 10 dBZ as the echo top height ETH threshold. We have clarified this in the revised manuscript. This has been used by previous studies w/ the authors (i.e., Wang et al., 2018; 2019).

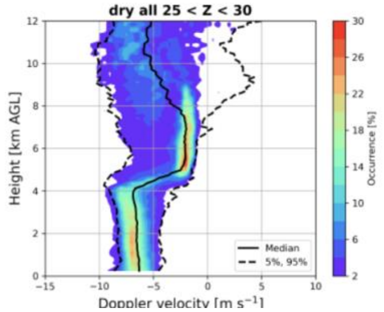
> ***Wang, D., Giangrande, S. E., Bartholomew, M. J., Hardin, J., Feng, Z., Thalman, R., and Machado, L. A. T.: The Green Ocean: precipitation insights from the GoAmazon2014/5 experiment, Atmos. Chem. Phys., 18, 9121–9145, https://doi.org/10.5194/acp-18-9121-2018, 2018.***
>
> ***Wang, D., Giangrande, S. E., Schiro, K., Jensen, M. P., & Houze, R. A. (2019). The characteristics of tropical and midlatitude mesoscale convective systems as revealed by radar wind profilers. Journal of Geophysical Research: Atmospheres, 124, 4601– 4619. https://doi.org/10.1029/2018JD030087***

4. Is the choice of 250 retrievals (line 115) for the CFADs based on previous testing or a common approach?

This choice was subjective, based on visual inspection of CFAD behaviors aloft that included (apparent) additional noisiness. We have clarified this in the revised manuscript. Before submission, as alluded above, we tested different sample sizes from 100 to 500, though otherwise the differences did not dramatically alter CFADs. One consequence for this choice is that we may not include potentially useful data above certain altitudes (say, 8 km) where relevant signatures still appear coherent with the adjacent data. This is important to our draft discussions as we reach altitudes above 10 km where we cut off our displays for sampling, beamwidth, and other considerations that previous studies express some caution in extending results further.

For example, we sample stronger graupel signatures and downwards air motions for the Wet and Dry seasons, but the signatures are far more pronounced to lower altitudes with the Dry season (i.e., more bulk graupel dominating the echoes to lower heights). For example, we provide an image below for Mean Doppler Velocity CFADs for a 25 < Z < 30 dBZ subset that favors graupel. Since our 'counts' threshold cuts off our typical displays at altitudes lower than 10 km, we are often not presenting all extended examples vertically (that we may believe are often coherent) in being conservative to previous sampling considerations. However, we have attempted to modify our revised text to remind readers that select signatures for stronger drafts do occur outside of our plot ranges.





5. If you replicated this approach using satellite measurements, would the cell counts be similar?

This is a difficult question to answer. The authors speculate that a satellite T_IR approach (i.e., T colder than 235K) may be matched to identify deep convective cell rainfall – working from that premise (sensitivity study), we suspect this may have been done by others? (esp. MCS events – perhaps not for the GoAmazon satellite/radar for isolated cells). However, isolated cells have anvils that likely will overlap in time (convolved) and add different complexity for tracking elements. Thus, it is not clear how the authors would have modified our thinking differently, esp. to also add select satellite options that may convey updraft strength, etc., ie., "overshooting tops", given the differences in frequency of these relative to radar lifecycle composites. We have had the good fortune of following in the footsteps of others who have performed (Amazon) satellite/radar cross-comparisons, or other thoughts, so we can point the reviewer to a few of those ideas:

Laurent, H., Machado, L. A. T., Morales, C. A., and Durieux, L., Characteristics of the Amazonian mesoscale convective systems observed from satellite and radar during the WETAMC/LBA experiment, J. Geophys. Res., 107(D20), 8054, doi:10.1029/2001JD000337, 2002.

Machado, L. A. T., & Laurent, H. (2004). The Convective System Area Expansion over Amazonia and Its Relationships with Convective System Life Duration and High-Level Wind Divergence, Monthly Weather Review, 132(3), 714-725. Retrieved Dec 15, 2022, from https://journals.ametsoc.org/view/journals/mwre/132/3/1520-0493_2004_132_0714_tcsaeo_2.0.co_2.xml

Machado, L. A. T., Calheiros, A. J. P., Biscaro, T., Giangrande, S., Silva Dias, M. A. F., Cecchini, M. A., Albrecht, R., Andreae, M. O., Araujo, W. F., Artaxo, P., Borrmann, S., Braga, R., Burleyson, C., Eichholz, C. W., Fan, J., Feng, Z., Fisch, G. F., Jensen, M. P., Martin, S. T., Pöschl, U., Pöhlker, C., Pöhlker, M. L., Ribaud, J.-F., Rosenfeld, D., Saraiva, J. M. B., Schumacher, C., Thalman, R., Walter, D., and Wendisch, M.: Overview: Precipitation characteristics and sensitivities to environmental conditions during GoAmazon2014/5 and ACRIDICON-CHUVA, Atmos. Chem. Phys., 18, 6461–6482, https://doi.org/10.5194/acp-18-6461-2018, 2018.

Dworak, R., Bedka, K., Brunner, J., & Feltz, W. (2012). Comparison between GOES-12 Overshooting-Top Detections, WSR-88D Radar Reflectivity, and Severe Storm Reports, Weather and Forecasting, 27(3), 684-699. Retrieved Dec 15, 2022, from https://journals.ametsoc.org/view/journals/wefo/27/3/waf-d-11-00070_1.xml

6. Would sub-setting the DCC events during the wet season (dry season) further by additional environmental conditions lead to any new findings about draft characteristics?

Yes, likely (see CIN/CAPE response above) – but, this would need to be tempered by limits imposed when slicing datasets too thin, as well as the hidden complexity these controls add. So, we think there is potential, but we also note that apparent easier, low-hanging parameters such as CAPE or CIN (as above) also introduce different controls, aka, CIN and CAPE are also highly seasonal, thus filtering using these parameters might skew characteristics towards 'transitional' or other environments that may promote stronger cells (those perhaps not maintaining the same spirit of Wet vs Dry contrasts in mid-level humidity that we stressed in our offering). As in our response to Reviewer question #1, there is evidence that higher/lower CIN or CAPE may be important, but motivate different tests than those in the current study.

Separately, RWP overpasses are a limiting factor for those aspects of this study. Our previous efforts (those that considered point vs spatial properties from radar) suggest we may require order of (10)s of event-profiles per 'control' group that we compare. Because these data are limited, it is easiest to work in 'seasonal' or bulk terms than finer subsets of CAPE, CIN, etc., with the GoAmazon datasets. Improved efforts may be possible with future Amazon/ATTO tower RWP datasets. However, the authors are aware of ongoing efforts attempting to summarize the larger SIPAM / radar-cell populations (statistical) for similar onset, lifecycle, environmental condition, aerosol condition, or seasonal/other contrasts. We look forward to what those studies reveal, as the number of sampled cells may be more appropriate to narrower ranges of controls (perhaps more smoothed behaviors, lifecycle-speaking).

7. The introduction describes why understanding DCCs is important for climate model improvement and potential changes in the cloud population with potential climate change. There was no connection from the findings of the study back to these broader impacts.

Noted. An improvement to the understanding of convection does not necessarily translate into model improvement. We have revised the introduction language to dial back its more 'marketing' aspects.

Minor Comments:

1. Line 149: 40 km2 -- km^2

Fixed.

2. Line 190: Not sure if the text needs to include specifics on how to interpret the lines of the figure.

   Removed.

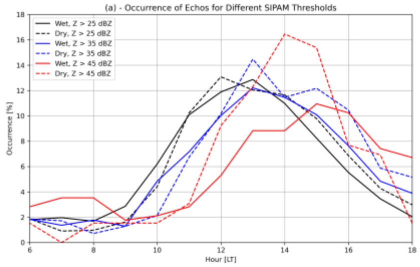
3. Line 200: Could you include the specific time that there is a more rapid transition to deeper convection or annotate it in the figure?

   Fixed.

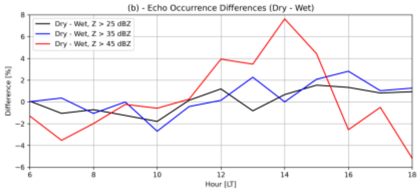
4. Lines 208-214: The arguments for 25 dBZ and 35 dBZ discussion were different but the lines on Figure 3 looked very similar. Perhaps a difference plot will help illustrate the argument.

The authors think the source of confusion is our poor phrasing. We have attempted to revise this line. For completeness, we plot a difference for the particular items the reviewer was mentioning, but just for the reviewer.

(a) - Occurrence of Echos for Different SIPAM Thresholds



(b) - Echo Occurrence Differences (Dry - Wet)



5. Line 226: Perhaps reference the specific values being discussed to guide the reader.

   Fixed.

6. Line 234: An explanation of the meaning of light rain / periphery may be needed. How is intensity inferred from Figure 4?

   Agree. We alluded to this in line (114), but have clarified in the revised manuscript.

7. Line 295: To later ETH-- wording is unclear

Agree. Fixed.

8. Line 323: Is the choice of 100 parcels based on previous testing or statistics, or computational limitations?

The choice of 100 parcels was arbitrary. We have clarified in the revised text. It was a compromise between having enough parcels to attain robust statistics, though not having too many such that the code ran too slow. We have repeated our analysis with 1000 parcels (not shown), but this had no change to the offered results.

9. Is there much variability in the fractional entrainment rate of tropical convection? If a range of entrainment rates are utilized, are the results similar?

Yes, there probably is variability; To the author's knowledge, this variability has not been well-documented in the literature. We note the stochastic entrainment in this model was specifically designed/intended to create (what we hoped as) a reasonable range of variability in entrainment. For instance, there are some “lucky” parcels that would not experience many entrainment events as they ascend, and will consequently have small overall fractional entrainment rates. In contrast, other “unlucky” parcels will experience many entrainment events and thus have larger overall fractional entrainment rates.

Figures:

10. In the figures it might be useful to include the sample numbers of wet season and dry season cells in the titles or legend.

    Fixed. Included in a legend.

11. Figure 2 subpanel d: Planet--Planetary

    Fixed.

12. Figure 8 (6, 7): The font sizes of the axis labels, ticks, and color bars are very small

    Thank you for the suggestion. We have changed the layout from landscape to portrait to enlarge the figure.

13. Figure 8: There is a way in python to ignore that panel h, unless it is empty for a reason? Perhaps in the figure caption explain why it is empty.

    We have removed the panel “h” from the figure. There are no storms in the last phase of their lifetimes that provide a ‘hit’ of the T3 site.

14. Figure 9: Color bars for the plots or specify the values of contour values

Fixed.

15. A figure showing the methodology of the tracking method would be a good addition to visual the steps of the tracking method.

    In this instance, we did not want to add another manuscript image, but are modifying the text to point readers to previous manuscripts that provide such images for this method, e.g.,

    Vila, D. A., Machado, L. A. T., Laurent, H., & Velasco, I. (2008). Forecast and Tracking the Evolution of Cloud Clusters (ForTraCC) Using Satellite Infrared Imagery: Methodology and Validation, Weather and Forecasting, 23(2), 233-245. Retrieved Dec 15, 2022, from https://journals.ametsoc.org/view/journals/wefo/23/2/2007waf2006121_1.xml

<u>Detailed Responses to Reviewer 2:</u>

Using the observational data collected during the GOAmazon campaign, this work characterizes a certain type of deep convective events over the Amazonnear the city of Manaus. Compared with previous studies---many of which contributed by the lead author---this manuscript focuses on the isolated, diurnally forced local events peaking in the afternoon that are different from the nighttime storms originating from elsewhere. The statistics synthesized here are useful as a background/benchmark for model comparisons. I think this manuscript is suitable for the ACP; I only have a few comments which should be easy to address.

We thank this reviewer for their efforts and suggestions/questions, and we hope we have addressed their concerns in this combined response.

Major comments:

1) The expressions "melting level" and "aloft" are used multiple times in the manuscript. The actual height of the melting level is not explicitly called out; it can be identified through the abrupt transition in the draft velocity or frequency in some of the figure panels, but not so obvious in some of the other panels. Similarly, for aloft, it's probably better to give a more precise height range (including in the abstract).

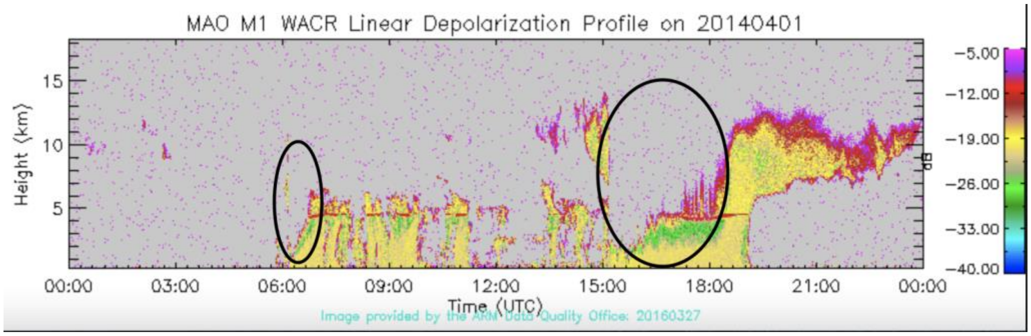
Thank you for the comment. We have attempted to clarify on these themes (more specific call-outs to heights, ranges, values used) throughout the revised manuscript.

2) Figure 2b shows no/little clouds in the mid-and lower-troposphere around 1600 LT, and the text in L199-201 states that this is caused by a rapid transition to deep convection in the dry season. I am not sure why the mid-and low-level clouds would disappear when a rapid transition occurred. In the dry season, Fig.4f indicates that the number of events hitting the T3 site drop to zero at some point; Is the absence of mid-and low-level clouds related to the small sample size?

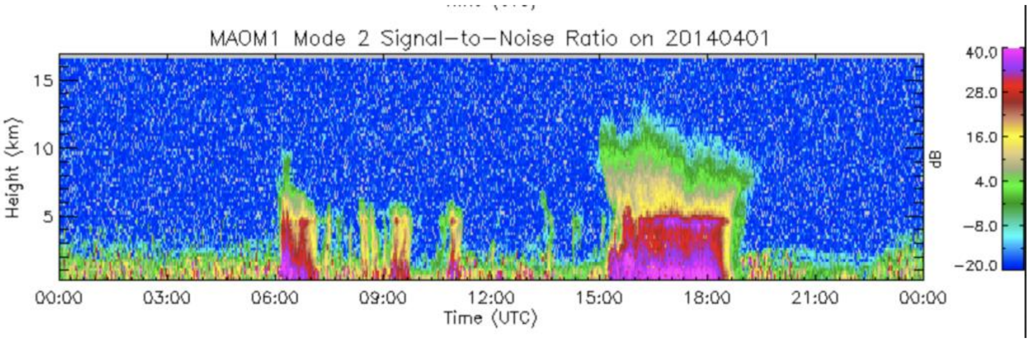
This answer is complicated; Our discussions associated with Figures 2 and 4 are not directly linked – the discussions are communicating different ideas as viewed from different radars (albeit collocated) in terms of sensitivity and the sense of lifecycle in the respective plots. The authors will attempt to clarify what we think the reviewer is asking, however we are unsure if the manuscript is confusing, or requires a significant text change (aka, needs more statements on limitations of radar sampling).

Figure 2 is a diurnal composite (in UTC) drawn from the WACR-ARSCL, and the W-band radar is sensitive to shallower and/or higher clouds (anvil); However, it is heavily attenuated in rain when deeper convection is overhead. This radar would not well-sample columns having rain, i.e., extinguished in modest rainrate R > 1-3 mm/hr, etc., to above the melting layer. Thus, if there was a 'hit' to the WACR, there would be little echo associated with those times contributing to these plots (above 1-2 km, etc.). For example, consider an Amazon WACR image below (taken from the ARM Plot Browser Website,

https://dq.arm.gov/dq-plotbrowser/) with circled areas representing the locations where the WACR is heavily attenuated in rain:



In contrast, Figure 4 represents RWP 'hits' that are the situations exclusive to single cells having Z > 25 echoes (precip) that 'hit' the T3 RWP, and these hits are presented in terms of relative lifecycle time. The RWP is mostly sensitive to precipitation-sized media, and not as sensitive to anvil/cloud at peripheries of that precipitation, etc. For the same event above, below is a corresponding RWP image:



As to how this relates to our sense of radar/sampling: We agree that there may be absolute instances in UTC time (WACR, Figure 2, i.e., 16 UTC) that do not apparently register/summarize 'deeper' cloud/echo immediately overhead. If a deeper rain cell with R > 1-3 mm/hr was overhead, the WACR would be heavily attenuated, and there would not be much echo from those events to plot to the mid-upper levels (ARSCL does not include the RWP – yet). Thus, what is genuinely contributing at any column in Figure 2 is often more the times adjacent that provide an indication of the shallow, weaker congestus, and periphery deep/juicier anvil echoes that eventually are those associated with a 'hit'. In that way, the lens of the WACR and our Figure 2 discussion/inference is less about hits individually, and more suggestive to the process/timing/building or 'deepening', i.e., when shallow shifts to congestus or anvil signatures advected from nearby deeper cells start to be more prevalent overhead.

3) I'd like to suggest adding grid lines to Figs. 4-8, or at least adding the w=0 line, making the figures easier to read.

Thank you for the suggestion, grid lines were added to the figures.

4) I've looked at somelong-time (1-year) high-resolution (dx~1km) CRM output and noted that there wasn't any buoyant plume that can be distinguished from the environment. Although buoyant plumes (and bubbles) have been important conceptually and have been used in CRM studies to initiate convection, gravity waves seem to be very efficient to diffuse the horizontal buoyancy gradient in the free troposphere (at least when f is small). This observation made me reconsider the use of traditional plume models in general. I am not disputing the fact that plume computations can be informative as used here; I am glad to see that some of the new elements developed by Peters and colleagues have been incorporated into the model; I acknowledge that there isn't a valid alternative of the plume approach (except for the computationally expansive CRM/LES options). Still, I am interested in what the authors think regarding this point. In simulations for shallow convection we do see plumes/bubbles. Is there any evidence suggesting that isolated storms driven by diurnal forcing behave like rising buoyant bubbles?

This is a difficult question (and ongoing work!). First, we agree with the reviewer that there is evidence in the literature that the behavior is more 'buoyant bubbles' than plumes (i.e., see Romps, Hernandez-Deckers, Varble, and many others). However, the authors may suggest that our use of this STM (parcel model) is opportune in that there is also no need / requirement to claim / assume things to behave as a plume or thermal – That is, we think there is an argument that this STM approach may fit both situations and/or the equations are of the same form with both models – for other references on this, one may also see the 'thermal chain' manuscripts of the authors.

Minor comments:

L99: AGL?

Fixed. The text now clarifies what this means.

L105: What does "toward relative updrafts" mean?

Fixed.

L126: Is the lowest 1 km chosen according to the PBL height shown in Fig. 2d?

This was initially our default; it was not specifically selected for those reasons.

L155-156: Are these numbers of events different for Z>25dBZ vs 35dBZ? It may be handy to have these numbers repeated in Tables 1, 2 as well as Fig. 4f (they are kind of buried in the text).

We mention the event counts in approx. line 226 of the original manuscript, but can call attention to these counts in the Tables (easiest place to add references).

L157-162: It’s probably better to call out in the beginning of L157 that these numbers are estimated using the Z>25dBZ threshold. L161 seems to be saying that the numbers for Z>35dBZ are similar to those for 25dBZ, contradicting to the numbers 90/30 minutes being different.

Fixed. We have modified the text accordingly.

L193-195: The reduction of dry season cloud cover is also associated with a slightly greater increase in surface temperature as in Fig. 2c. (To double check: Is this surface temperature or surface air temperature?)

The temperature plotted is the surface soil temperature (ARM SEBS). We will clarify this in the revised text.

L269: Bardakov et al. (2022; JGR Atmosphere) recently noted in a set of CRM experiments that air parcels in downdrafts were mostly from mid-troposphere, which may be relevant. Another related point: There’s a paper by David Romps that studied the lifetime of high clouds in models and concluded that subsaturation expressed in terms of specific humidity is more useful than RH. At low temperatures, the low saturation vapor pressure limits how much condensate can evaporate regardless of the RH. This is consistent with Bardakov et al’s observation.

Thank you for the comment. We attempted to clarify the discussion on the downdrafts in particular as based on some follow-up discussions. We observe relatively stronger downdrafts in all seasons, but for the Wet season, we found that some instances aloft are associated with altitudes > 10 km, and lower-relative Z values that sometimes are not as well captured with our CFAD displays that prioritized Z > 25 dBz.

L287-288: “Interestingly, ...” Is this simply because the air density decreases with height so that the updrafts cannot support large hydrometeors?

Yes. Re-reading the statement, it was perhaps less ‘interesting’, and possibly more ‘expectedly’? We have removed the word.

L313-314: “Late cell phase samples... unavailable...” There’re no storms passing over T3 in Q4 of their lifecycle?

Yes, correct. During the dry season, there were no cells in the last stage of their lifecycle that provided a hit over T3. The panel has been removed in the revised image.