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Comment on egusphere-2022-877

Anonymous Referee #2

Referee comment on "Seasonal controls on isolated convective storm drafts, precipitation intensity, and life cycle as observed during GoAmazon2014/5" by Scott E. Giangrande et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-877-RC2, 2022

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.

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).

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?

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.

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?

Minor comments:

L99: AGL?

L105: What does “toward relative updrafts” mean?

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

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).

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 similarto those for 25dBZ, contradicting to the numbers 90/30 minutes being different.

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?)

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.

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

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