Review for egusphere-2026-4291 – Revision 1

Overview

I am satisfied that the authors have addressed the comments raised in my original review and thank them for their comprehensive response. Rereading the paper in full, I noted a few other minor errors, ambiguities, and text refinements that I think are worth considering before the paper is published. I am therefore recommending further minor revisions; however, I do not see a need to review the manuscript again before publication.

Minor Comments

L16: The terms "downpour" and "deluge" don't actually refer to localised and widespread heavy rainfall. In general usage they are consider synonymous, although Google suggests that the term deluge is more commonly used in the context of rainfall that leads to flooding. As such, I'd recommend removing these terms from both this sentence and the title of your paper. Personally, I'm not a fan of the "Hook: Subtitle" format for journal articles. I usually find the Hook to be a distracting waste of words. My advice would be to just use your subtitle: "Understanding the contrasting environments for localised and widespread heavy rainfall in the tropics".

L33: How did the large-scale environments of localised "showers" and widespread "rains" identified by Raymond et al. (2024) differ? You've clarified this for some of the other studies discussed in this paragraph but not this one.

L113: "dry air–vapour mixture" needs an en dash. There are also a few places where hyphens need replacing with en dashes:

- "precipitation–humidity" on L229
- "convective–stratiform" on L268 and L296–297.
- "stability–humidity" on L462 and L495.

L130–131: Surely only Eq. 5 can be solved for h_e^* since this variable does not appear in Eq. 3.

L141–143: I suggest revising the second part of this sentence (after the colon) as follows: "$m_b(\epsilon') = M_0/\epsilon_0$, where M_0 is the cloud-base mass flux required under conditions of radiative–convective equilibrium and ϵ_0 is a characteristic entrainment rate."

L150: Please replace ϵ in this equation with $\epsilon(z)$. I kept reading $\epsilon(z - z_b)$ as the entrainment rate at height $z - z_b$, rather than the product of $\epsilon(z)$ and $z - z_b$.

L168–169: When you say "low levels" and "upper levels", do you mean "below 10 km" and "above 10 km", respectively? If so, consider revising to make this explicit. Also "upper levels" doesn't need a hyphen here as it is not acting as a compound modifier.

L181: Change "uniform" to "constant" and "$z = z_b$" to just "z_b".

L186: Change "humidity" to "specific humidity".

L193: I assume that z_t represents the cloud top (LNB), but this has not been stated anywhere.

L211: This line shouldn't be indented.

L222–223: There seems to be a stray $\bar{\epsilon}$ at the end of this sentence. I suspect you meant for it to go after "effective entrainment rate".

L233: The dependence of w_{ext} on humidity is weak across much of the phase space but becomes more significant for high entrainment rates ($\bar{\epsilon}$ > 0.25 km^{-1}). This makes physical sense, as strongly entraining plumes will only be able to maintain large vertical velocities if the entrained air is relatively moist (and therefore dilution of updraft buoyancy is limited).

L240: Consider changing "Although we note that" to "We note, however, that".

L266: You can delete the acronym BoM as it isn't used again.

L287–288: Delete "based on the Steiner et al. (1995) convective–stratiform classification" (the classification algorithm has already been noted on L272).

L295: It might be worth noting that large convective systems (e.g., long squall lines) may still be truncated by the 150 km maximum range of the radar data.

L304: Here and in Eq. 15, you could denote the total domain area as simply D (dropping the subscript "a").

L323: In ERA5, CIN is calculated as minus the integrated negative buoyancy below the LFC, such that values are positive. Personally, I much prefer this convention as it means that higher (i.e., more positive) CIN corresponds to stronger inhibition; however, many studies (particularly in the US) define CIN without the minus sign in the equation, such that values are negative.

L334–335: It still isn't clear to me what data Palmer and Singh (2024) used for precipitation. You said in your reply to my previous comment that they didn't use radar data, but what did they use (satellite, reanalysis, rain gauges, etc.)?

L371–372: Replace "level of free convection" with "LFC" and "equilibrium level" with "LNB".

Figure 9: Similar to Fig. 7, it might be useful to add a second row of panels showing the distribution of each variable with shading showing the four quartiles. Alternatively, you could add this as an inset to each panel and then create a single legend for the figure (although this would mean using the same colours for panel c as for panels a and b).

L432–433: I think you mean to say that $\bar{\epsilon}$ generally decreases with increasing boundary-layer depth (i.e., with increasing mean and maximum BLH and mean LCL).

L436: Rather than "(not shown)", you could say "(cf. Fig. 5c and Fig. 8a,c)".

L447–450: Here and in Fig. 8d, I recommend using σ_{SF} to represent the standard deviation of the saturation fraction.

L472–473: Since the 100 km radius only applies to the radar observations and not the reanalysis data (for which you have used a 3° x 3° area), I recommend replacing "within a 100 km radius of a radar site in Darwin, Australia" with simply "for Darwin, Australia".

L510–511: The way the start of this sentence is written implies that "reversible" (adiabatic) CAPE includes the ice phase by definition, which is not the case. Parcel ascents can be modelled as adiabatic, with all condensate retained in the parcel, or pseudoadiabatic, with all condensate

assumed to immediately fall out of the parcel. In either case, the ice phase (and associated latent heat of fusion) can be included or excluded. Including a mixed-phase region for an adiabatic ascent with ice introduces a source of entropy, making the process irreversible, which is why the term "adiabatic" is preferrable to "reversible". I recommend revising the last three sentences of this paragraph as follows: "Future work could also consider adiabatic ("reversible") instead of pseudoadiabatic CAPE and CIN and account for the ice phase. Adiabatic CAPE with ice, has been argued to be a more appropriate measure of conditional instability in the tropics (Williams and Renno, 1993), while adiabatic CIN has been found to provide a better representation of the inflow layer for simulated convective storms (Murdzek et al., 2021)."

References: Capitalisation needs fixing in the paper title for some reference (e.g., Allen et al. 2014) and in the journal name for others (e.g., Brooks et al. 2013).