Review for egusphere-2026-4291

Overview

This study compares the characteristics and environments of locally intense and widespread heavy rainfall events in the tropics using a combination of theory and observations. First, a spectral plume model (SPM) is developed (following Zhou and Xie 2019), which relates the thermodynamic profiles of the plumes and their environment to the convective mass flux of the plume ensemble. The predictions of this model are then validated against radar observations and reanalysis data from Darwin, Australia. The analysis makes extensive use of the stability–humidity plane introduced by Palmer and Singh (2024, 2025) to relate precipitation intensity and morphology to various environmental variables (CAPE, CIN, boundary-layer height) and the effective entrainment rate $\bar{\epsilon}$ from the SPM.

This is an extremely well put together piece of work. The writing is clear and concise; the methodology is sound; the figures are neat and effective; and the results are appropriately discussed in the context of previous work. As a reviewer, it is rare to receive a manuscript that is so close to being publication ready at the initial submission phase. I have no major revisions to recommend but have provided a comprehensive set of minor comments for the authors to consider. While I would be interested in their responses, there is probably no need for me to review the paper again. I thank the authors for putting together such a high-quality manuscript, which has made my work as a reviewer very easy on this occasion.

Minor Comments

L26–27: Variations on the statement that "the largest local rainfall intensities occur under different large-scale environments to those associated with the heaviest areal-average rainfall rates" appear several times in this paragraph. However, it is never actually explained what this means. How do the environments differ specifically? Did the three studies you mention (Louf et al. 2019, Singh et al. 2019, and Raymond et al. 2024) all find similar differences between the environments of local and widespread heavy rainfall? This is important context for your study.

L36: Here and elsewhere (e.g., L439), you can probably drop "NT", as there is only one Darwin in Australia today (apparently, there was another one in Tasmania for a brief period at the start of the 20th century).

L44: Traditionally, CAPE does not account for entrainment; however, several studies have described methods for calculating entraining CAPE (ECAPE) using an entraining plume model (e.g., Romps and Kuang 2010, Peters et al. 2022, Peters et al. 2023).

L45–46: I think the last part of this sentence should read "implied by the increase in CAPE".

L46–47: Entrainment also helps explain the sensitivity of convection to vertical wind shear (e.g., Peters et al. 2019, Peters et al. 2020, Jo and Lasher-Trapp 2022) and the depth of the sub-cloud layer (e.g., Mulholland et al. 2021, Takahashi et al. 2023, Peters et al. 2026).

L52: "shaping ~~the~~ tropospheric temperature and humidity profiles".

L63: Consider introducing the initialism "LNB" for level of neutral buoyancy.

L76: Consider changing "plotting the plume model and observations" to "plotting solutions to the plume model and observed properties of convection" (cf. the captions to Figs. 3 and 5).

L77–85: It is a bit unusual to give away quite so much of the results in your introduction. You could remove this text and append the opening sentence of this paragraph to the end of the previous one.

L86: "Stability–humidity plane" should use an en dash (-- in LaTeX) rather than a hyphen, since "stability" and "humidity" carry equal weight. The same applies to "radiative–convective equilibrium" (e.g., L126) and "convective–stratiform classification" (e.g. L262).

L 107: Is c_p the isobaric specific heat of dry air or are you accounting for the heat capacity of water vapour also? If the former, consider adding a subscript "d" to make this obvious.

L111–118: You seem to have dropped the subscript "p" for "plume" here (not sure if this was intentional; if so the change in notation should be explicitly noted).

L124: Presumably derivation of the mean temperature and humidity profiles requires an assumption of hydrostatic balance.

L169–170: I believe that an analytical formula for surface RH in terms of the surface temperature and LCL height can be defined by combing equations 22a and 22c from Romps (2017). The result equation will depend on both c_{pm} and R_m, which in turn depends on RH, making the equation implicit. However, I suspect that you could replace these with c_{pd} and R_d, respectively, without introducing a significant error (although I haven't tested this). If you retained c_{pm} and R_m, an iterative procedure could be used to get RH (perhaps this is the method you are using).

L185–186: I think you can delete "in order to evaluate the distribution of convective intensity", as it doesn't really fit here.

L197: You seem to be missing the subscript "e" from q and q^* here.

Figure 3: Elsewhere you've said "SPM" rather than "plume-spectrum model". Also, you should note in the caption what the white area in each figure represents.

L211: Also, the far top-left corner, which is presumably where RH < 2 %.

L222–223: Can you comment on the weak humidity dependence, which is most pronounced for high entrainment rate (lower saturation deficit). What explains this behaviour?

L238–239: I wonder, can these results concerning the sensitivity of effective entrainment rate to cloud base height be compared quantitatively with other theoretical studies (e.g., Peters et al. 2026) or the recent observational study of Murillo et al. (2026)?

L257–258: It would be good to include a map of the radar domain, with the 100 km range ring and ERA5 grid cells used in your analysis overlaid.

L264: "Quality controlled" doesn't need a hyphen in this context, as it is not acting as a compound modifier.

L268: Rainfall within the first few tens of kilometres of the radar may also be misrepresented due to the "cone of silence" (i.e., echoes above the maximum elevation sweep). However, I'm assuming that no minimum radius was applied in this study.

L276: You should specify if you are using 4-connected or 8-connected neighbours for defining convective cells.

L277: For small convective cells, the cell-mean rain rate (and therefore the maximum cell-mean rain rate) could be sensitive to spurious values associated with ground clutter or other non-meteorological echoes. Did you also try using something like the 90^{th} percentile of rain rates within each cell to see if this altered your results in any way?

L291: I think you mean "~31,400 km^2" here.

L297–298: I'm confused by this description of the ERA5 grid points used. First, why did you use a 150 km radius when you have limited the radar observations to a 100 km radius? Second, if you are using a circular area, how did you end up with a 12 x 12 square grid of points? It might make more sense to simply state that you used a 3° x 3° (12 x 12 grid point) area centred on the radar.

L302–303: It might be worth explicitly noting that, by using a pseudoadiabatic parcel ascent, CAPE (and CIN) in ERA5 neglects the negative contribution of condensate loading to parcel buoyancy and reduces the positive contribution of freezing (since much of the available water has been removed by the time a parcel reaches the freezing level). Both can be significant in very moist tropical environments (see, for example, Fig. 7 and 8 in Warren 2025). Previous studies have argued that so-called "reversible" (adiabatic) CAPE including ice is a more appropriate measure of conditional instability in the tropics (e.g., Williams and Renno 1993). Reversible CIN has also more recently been argued to provide a better representation of the inflow layer for convective storms (Murdzek et al. 2021).

L304–306: I believe this approximation also applies to the calculation of CIN, which means values may be significantly overestimated (Davies 2004).

L319–320: Did Palmer and Singh (2024) also use radar-observed precipitation, or did they use other observations (e.g., satellite) or reanalysed precipitation?

L325–326: It might be worth noting here (in addition to the caption to Fig. 5) the minimum sample size criterion that was applied in calculating the mean within each bin.

Figure 5: In the last sentence of the caption, change "as labelled" to "as labelled in (a)".

L354–356: I'm not sure I buy this argument concerning the difference in CAPE between the SPM and ERA5. My own work (currently under review) shows that instability in the Australian tropics is almost always surface based (at least in thunderstorm environments). I suspect that if you were to repeat your analysis using surface-based (SB) or mixed-layer (ML) CAPE, you would see the same behaviour. The sensitivity to saturation deficit may instead reflect the fact that the true lapse rate deviates more from a moist adiabat as the atmosphere becomes drier. You could easily assess this by plotting the actual 850–500 hPa temperature lapse rate in the stability–humidity plane (adding this as an extra panel in the top row of Fig. 6).

L375–376: Might there also be value in plotting CAF in the stability–humidity plane (adding this as an extra panel in the bottom row of Fig. 6)? Presumably, this would validate your statement that CAF is "highest for large values of $\bar{\epsilon}$".

L378: Is the CAF distribution fairly uniform, close to normal or highly skewed? It might be helpful to include a plot of the distribution with the limits of the four quartiles overlaid.

Figure 7: I recommend modifying this figure so that (i) the x axis is only labelled along the bottom row, (ii) the y axis is only labelled along the left column, (iii) the title ("Q1 CAF", "Q2 CAF", etc.) is only included for the top row, and (iv) there is only a single colour bar (with the same range) for each row on the right column. These changes will allow you to make the individual panels larger and easier to read. Also, I'm assuming that the smaller threshold of 20 radar scenes (compared to the 30 used in other stability–humidity plots) was used due to the smaller overall sample sizes when the data is split up by CAF quartile; however, this should be explicitly noted.

L386: It wasn't entirely clear to me what relationship you were referring to here. Consider rephrasing this sentence to make this explicit.

L390–391: An exception to this statement is the Q1 CAF, for which the maximum cell intensity (and mean cell area) shows some limited dependence on stability, with values mainly increasing from left to right (i.e., with increasing saturation deficit) rather than from top to bottom (i.e., with decreasing stability). Can you maybe comment on this?

L391: coincide → coincide<u>s</u>

L419–420: I note that the environments with high moisture variability (i.e., higher $\sigma\mathrm{SF}$) are also those with the largest domain-mean rainfall (Fig. 5b), convective rain contribution (Fig. 5e), and convective cell intensity (Fig. 5c,f). Could it be that the enhanced moisture variability is actually a signature of the convective cells themselves, whose impact on local humidity may be reflected in ERA5 via the assimilation of all-sky satellite radiances? If so, the moisture variability may be a response to (rather than a cause of) intense convection in these high-saturation deficit environments.

L436: Suggest changing "high localised intensity convection" to "localised high-intensity convection".

L472–474: I'm confused by this statement. I thought that you were using the pre-calculated CAPE from ERA5, which is derived using the full model-level fields, rather than data on pressure levels. The issue in this case is not the vertical resolution of the input data but rather the neglect of the virtual temperature correction (i.e., the contribution of moisture to parcel buoyancy), which will lead to an underestimation of CAPE and an overestimation of CIN. The neglect of condensate loading (implicit in the pseudoadiabatic parcel ascent) is another limitation that it may be worth commenting on here.