Authors' response: "Downpours and deluges: Understanding the contrasting environments for localised and widespread heavy rainfall in the tropics" submitted to WCD

We thank the editor (Dr Juliane Schwendike), Dr Rob Warren and an anonymous reviewer for their time and consideration of this manuscript. The reviewers' comments are listed below in black and responded to individually in blue. Revised sentences are in red.

Response to Dr Rob Warren:

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.

We thank the reviewer for this suggestion. We have explained the "different large-scale environments" in more detail as follows:

But Louf et al. (2019) also found that the largest local rainfall intensities occur under different large-scale environments to those associated with the heaviest areal-average rainfall rates: intense local rainfall occurs in drier, subsiding environments, whereas heavy areal-mean rainfall occurs in moister, ascending environments. In the same vein, Hamada et al. (2015) showed that heavy precipitation and intense deep convection are not always co-located based on satellite observations. In idealised convection-permitting simulations, Singh et al. (2019) found that domain-mean precipitation increased with stronger large-scale ascent, while extreme updrafts were relatively insensitive to forcing strength and were not a simple function of instability. Combining observations and numerical simulations, Raymond et al. (2024) showed that the intense but localised "showers" and the weaker but widespread "rains" occur with different large-scale environments.

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 20 th century).

We have revised this following the reviewer's suggestion.

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).

We have included entraining CAPE as follows:

The increase in convective intensity may therefore be substantially smaller than that implied by the increased CAPE (Singh and O'Gorman, 2015). Several studies have incorporated entrainment to calculate entraining CAPE (ECAPE) and found that ECAPE predicts maximum updraft speed more accurately than conventional CAPE (Peters et al., 2022, 2023).

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

We have revised this following the reviewer's suggestion.

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).

This is a good point. We have revised as follows:

Entrainment may also link convective intensity to vertical wind shear and the depth of the sub-cloud layer. Stronger shear has been argued to reduce fractional entrainment, thereby preserving more undiluted air within the convective cores (e.g. Peters et al., 2019, 2020b; Jo and Lasher-Trapp, 2022; Maybee et al., 2024); similarly, a higher lifting condensation level can favour wider updrafts and reduced fractional dilution (e.g. Mulholland et al., 2021; Takahashi et al., 2023; Peters et al., 2026).

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

We have revised this following the reviewer's suggestion.

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

Following the reviewer's suggestion, we have introduced the initialism "LNB" for the level of neutral buoyancy at its first occurrence and use it throughout the revised manuscript.

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).

We have revised this following the reviewer's suggestion.

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.

We thank the reviewer for this suggestion. We have considered removing this text but have retained it because the brief summary of our key findings helps establish the motivation for the study and clarifies how our work addresses the knowledge gap outlined above. We believe this structure provides a clearer transition from the existing literature to the aims of the present study.

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).

We have revised this following the reviewer's suggestion.

L107: 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.

Here we also account for the heat capacity of water vapour. We have clarified this as follows:

…, c_p is the isobaric specific heat capacity of the dry air vapour mixture, g is the gravitational acceleration constant, and l_{v0} is the latent heat of vaporisation at a reference temperature T_0 = 273.15 K. For simplicity, we neglect any contribution of freezing to the moist static energy, and we assume no condensate (liquid or ice) is present in the parcel.

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).

Here, $h(z)$ is not the MSE for any individual plume, but for the MSE of the plume **that detrains at level** z**.** We have clarified this as follows:

Zhou and Xie (2019) showed that one could solve for $h(z)$, the moist static energy of the plume that detrains at level z. Note that $h(z)$ does not represent the vertical profile of any individual plume.

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

We thank the reviewer for identifying this unstated assumption. We have clarified this as follows:

From this, one may derive the mean temperature and humidity profiles assuming 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).

The reviewer is correct that we used the iterations from Romps (2017). Using c_{pd} and R_d does not change the results significantly, but for consistency, we retained c_{pm} and R_m. We have clarified this as follows:

An exact formula for the LCL was derived by Romps (2017), but it depends implicitly on the surface humidity, necessitating an iterative solution.

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

We have revised this following the reviewer's suggestion.

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

The reviewer is correct and we have added the subscript in the revised manuscript.

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.

We have replaced "plume-spectrum model" with "SPM" for consistency and revised the caption to explain the white regions:

The white areas in each panel indicate combinations of stability and saturation deficit that are not produced by the SPM within the prescribed ranges of precipitation and relative humidity.

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

The far upper-left corner corresponds to a small saturation deficit and therefore high, rather than low, relative humidity. Conditions approaching the left boundary are limited by the maximum RH prescribed in the SPM. We have clarified this in the Figure 3 caption.

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?

Here, CAPE is calculated for an undiluted parcel and therefore has little sensitivity to environmental humidity. In contrast, w_{ext} considers entrainment, which introduces a weak humidity dependence. We have commented this as follows:

Because CAPE is calculated for an undiluted parcel, it is largely a function of instability, with little humidity dependence, increasing downwards in the stability–humidity plane. On the other hand, the extreme updraft w_{ext} increases primarily with instability but also exhibits a weak dependence on humidity owing to entrainment, with larger values occurring in a drier atmosphere.

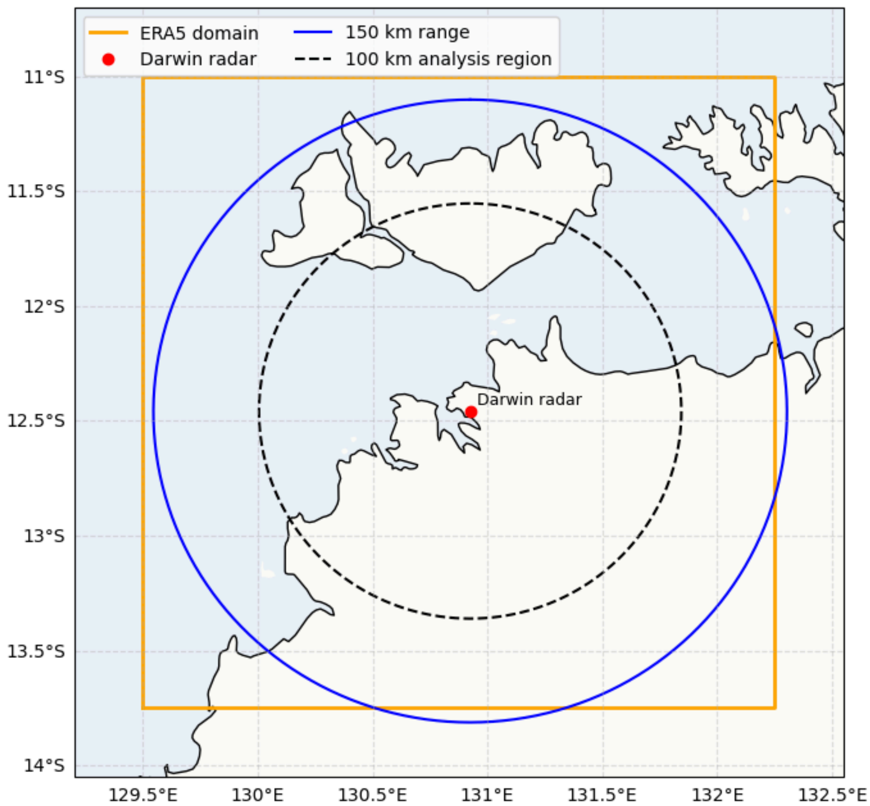
L238–239: I wonder, can these results concerning the sensitivity of effective entrainment rate to cloud base height be compared quantitatively with other theoretical studies (e.g., Peters et al. 2026) or the recent observational study of Murillo et al. (2026)?

We agree with the reviewer that quantitative comparison with studies like Peters et al. (2026) and Murillo et al. (2026) would be a useful direction for future work. However, the sensitivity obtained here is not directly comparable with those studies in the SPM. For a fixed CAPE, SPM gives similar w_{ext} when the cloud-base height is reduced from 1400 m to 700 m. Future work is needed to investigate these sensitivities before a meaningful quantitative comparison can be made. We have clarified this in the revised manuscript (Section 4.2) as follows:

In addition to a lower effective entrainment rate, higher values of the LCL and maximum BLH correspond to greater maximum cell intensity in the radar observations (not shown). This is consistent with previous theoretical (Peters et al., 2026) and observational studies (Murillo et al., 2026) relating boundary layer depth to the dilution experienced by convective updrafts. For the SPM, however, a fixed CAPE has a similar value of w_{ext} even when the cloud-base height is reduced from z_b = 1400 m to z_b = 700 m (not shown). This is because the SPM considers the effect of entrainment on the environment as well as the convective ensemble, and w_{ext} therefore increases with a wider entrainment distribution rather than a lower mean value of entrainment. However, this result is likely sensitive to our assumption of a uniform distribution of mass flux with entrainment; further work is needed to investigate different mass flux distributions based on cloud-resolving simulations.

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.

We thank the reviewer for this suggestion. We considered adding a map but decided it would not materially aid interpretation, as the spatial configuration is straightforward and already described in the Methods. Nevertheless, since this review process is an open discussion, for the benefit of interested readers, we provide the suggested map below.



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

We have revised this following the reviewer’s suggestion.

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.

The reviewer is correct that we did not apply a minimum-radius exclusion. Manual inspection indicates that some pixels affected by the cone of silence are already masked in the processed radar product. Nevertheless, we acknowledge that residual effects may remain close to the radar and have clarified this in the manuscript.

No minimum radius exclusion is applied, although manual inspection indicates that some near-radar pixels affected by the cone of silence are already masked in the radar product.

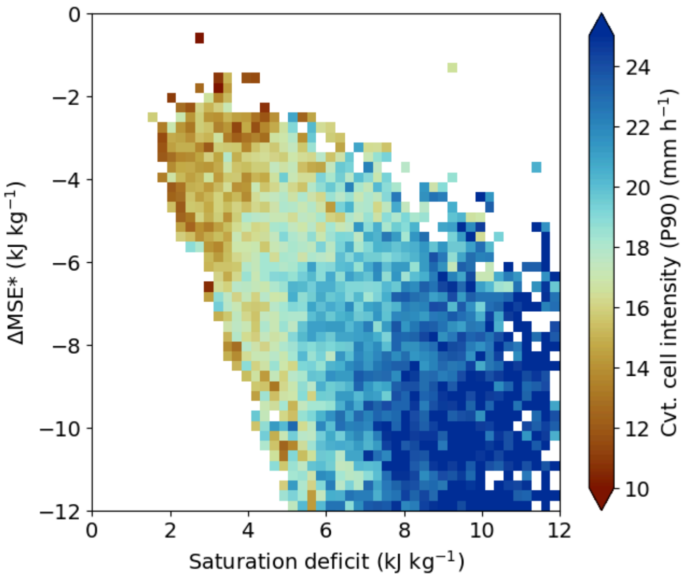
L276: You should specify if you are using 4-connected or 8-connected neighbours for defining convective cells.

We used 8-connected neighbours. We have clarified this as follows:

Instead, we identify convective cells as connected regions of convective pixels using eight-neighbour connectivity based on the Steiner et al. (1995) convective–stratiform classification.

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 nonmeteorological 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?

Following the reviewer's suggestion, we replicated our analysis with the 90th percentile of rain rates within each convective cell. The results remain largely unchanged:



We have commented on this sensitivity test in the revised manuscript:

As a sensitivity test, we alternatively use the 90th-percentile pixel-wise rain rate within each cell as a measure of local convective intensity. This produced results similar to those obtained using the cell-mean rain rate (not shown); therefore, we only present results based on the maximum cell-mean rain rate.

L291: I think you mean "~31,400 km2 " here.

The reviewer is correct. We have revised this in the revised manuscript.

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.

We thank the reviewer for this suggestion. The ERA5 domain was chosen to cover convective cells that intersect the 100 km radar domain, as these cells are retained in their entirety and may extend beyond the 100 km boundary (Lines 280–282 of the original manuscript). The reference to a 150 km radius was misleading because the ERA5 domain is square rather than circular. We have revised this as follows:

To characterise the large-scale environment, we use ERA5 data from a 3° by 3°area centred on the radar (11–13.75°S, 129.5–132.25°E; 12 by 12 grid points).

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).

We thank the reviewer for the detailed explanation and literature for CAPE and CIN. We have explicitly discussed these in the revised manuscript (Section 5):

In addition, CAPE and CIN are directly obtained from ERA5. Their calculation neglects the negative contribution of condensate loading to parcel buoyancy and reduces the positive contribution from freezing, both of which may substantially affect CAPE and CIN in moist tropical environments (Warren, 2025).

We have also added the following discussion to the manuscript (Section 5):

Future work could also consider reversible CAPE and CIN. “Reversible” (adiabatic) CAPE, which includes ice phase in thermodynamic calculations, has been argued to be a more appropriate measure of conditional instability in the tropics (e.g. Williams and Renno, 1993). More recently, reversible CIN has been proposed as 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).

We agree that in ERA5 both CAPE and CIN do not include the virtual temperature correction. We have revised this as follows:

Note that, for computational efficiency, the calculation procedure of CAPE and CIN in ERA5 uses an approximation that neglects the effect of water vapour on density. This may lead to an underestimation of CAPE and overestimation of CIN (Davies, 2004). Nevertheless, for simplicity, we use the CAPE and CIN provided by ERA5 and discuss these limitations in Section 5.

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

No, Palmer and Singh (2024) did not use radar-observed precipitation. We intended to say that the overall pattern is similar because both studies restrict their analyses to precipitating conditions. We have clarified this as follows:

Given that all 175,610 radar scenes are drawn from identified rainfall events, this result is consistent with that of Palmer and Singh (2024), who analysed reanalysis and radiosonde observations associated with precipitation exceeding 5 mm day^{-1}.

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.

We have noted this as follows:

The value of each 2D histogram bin is the mean of all scenes falling within that bin. Only bins with more than 30 scenes are included in the analysis.

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

We have revised this following the reviewer’s suggestion.

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).

Following the reviewer’s suggestion, we have added the 850–500 hPa temperature lapse rate to Fig. 6. Although the lapse rate shows some variation across the stability–humidity plane, it does not reproduce the pattern of ERA5 CAPE. This indicates that variations in the 850–500 hPa lapse rate alone cannot fully explain the sensitivity of CAPE to saturation deficit. We have therefore revised the discussion as follows:

This difference may be because CAPE integrates parcel buoyancy over the depth between the level of free convection and the equilibrium level, whereas our stability measure characterises only the 850–500 hPa layer. The 850–500 hPa lapse rate (Fig. 6c) does not exhibit the same pattern as CAPE (Fig. 6a), indicating that lower-tropospheric stability alone does not determine CAPE in ERA5. While the idealised SPM links CAPE solely to stability, CAPE is also influenced by the properties of the source parcel and the thermodynamic structure outside the 850–500 hPa layer.

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 $\overline{\epsilon}$ ".

Following the reviewer's suggestion, we have added the CAF to Fig. 6 and referenced this as follows:

A possible confounding variable in the above analysis is that of the convective area fraction (CAF). This increases strongly with the areal-mean precipitation and is therefore highest for large values of $\overline{\epsilon}$ (Fig. 6f).

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.

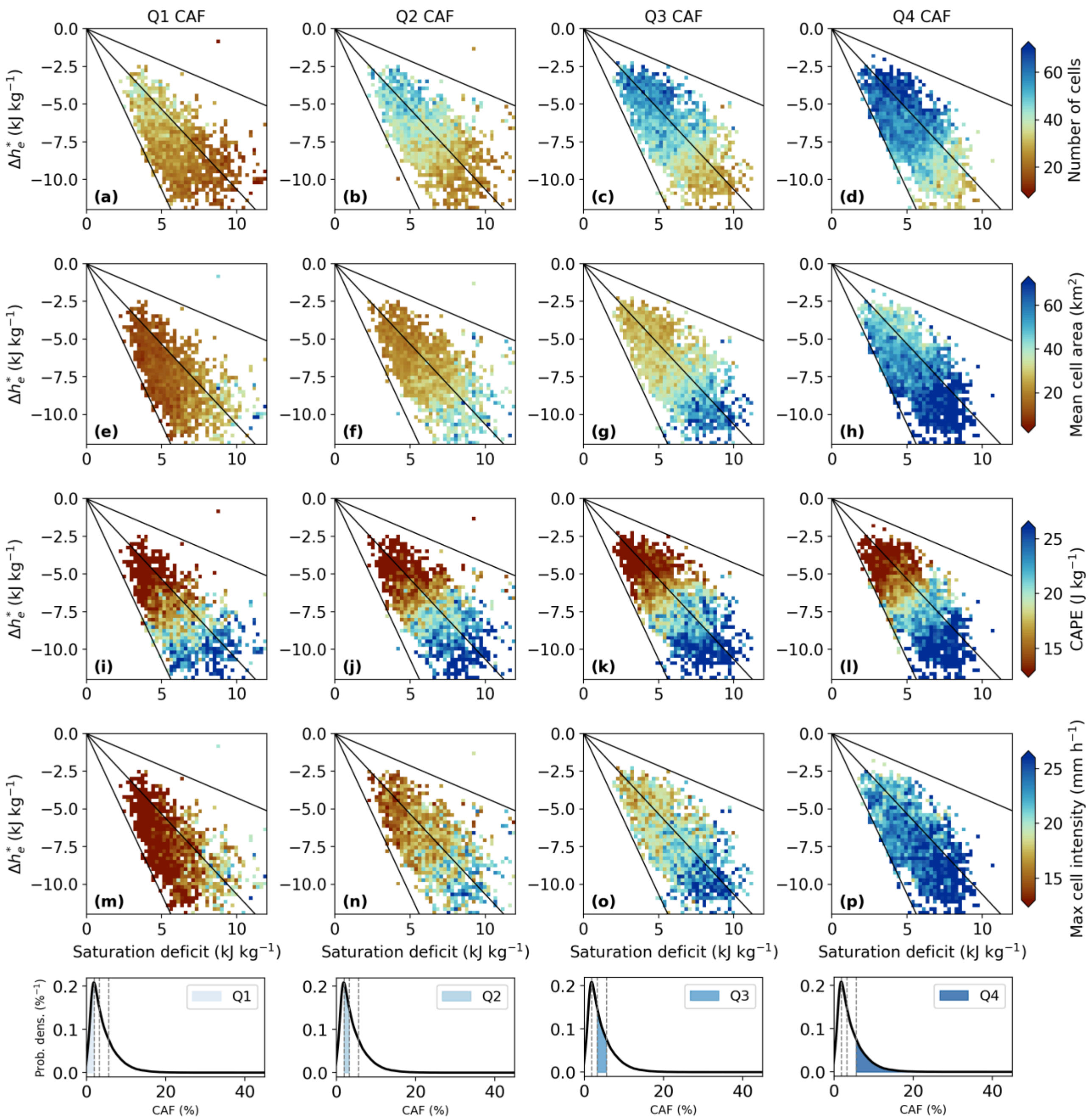
We have added the CAF distribution and the thresholds of the four quartiles in the revised Figure 7 (see more details in the response below).

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.

We thank the reviewer for the detailed recommendations. We have removed the x-axis and y-axis labels and titles for relevant panels in Figure 7. We use a single colour bar with the same range for each row. See below for the revised Figure 7.

The smaller threshold is because of smaller sample sizes. We have clarified this as follows:

Because partitioning the data into quartiles reduces the sample size within each group, we lower the minimum number of radar scenes required per 2D histogram bin from 30 to 20.



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

We have clarified this as follows:

Note that the above relationships of maximum cell intensity to the mean area of cells are specific to convective cells.

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?

We agree that maximum cell intensity and mean cell area show limited dependence on stability in the lowest CAF quartile. One possible explanation is that the small total convective area limits the range of possible cell sizes. We have commented on this as follows:

This pattern is weaker in the lowest CAF quartile, for which maximum cell intensity and mean cell area depend more strongly on humidity than on stability. At low CAF, the limited convective coverage favours smaller cells, which are expected to experience greater entrainment and therefore be more sensitive to environmental humidity.

L391: coincide → coincides

We have revised this following the reviewer's suggestion.

L419–420: I note that the environments with high moisture variability (i.e., higher σSF) 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.

The link proposed by the reviewer here is plausible. We would like to clarify that this study does not claim any causation. We have revised as follows:

The enhanced moisture variability may also partly reflect the effects of convection itself as represented in ERA5, for example through data assimilation, rather than a pre-existing environmental control. Although we do not infer causality from this relationship, this finding points to the possible importance of small-scale variability in the environment, unaccounted for in the SPM when characterising convective mixing.

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

We have revised this following the reviewer's suggestion.

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.

We apologise for the confusion here. We meant to refer to independently recalculating CAPE from ERA5 model-level fields with the virtual temperature correction included. We have revised this as follows:

The differences between CAPE in the SPM (Fig. 3c) and in ERA5 (Fig.6a) motivate further investigation in which CAPE is recalculated from ERA5 model-level fields with the virtual temperature correction included. Such a calculation would also provide improved sampling of boundary-layer conditions relative to standard pressure-level fields (Taszarek et al., 2021b).

We have added the following discussion to the manuscript:

Future work could also consider reversible CAPE and CIN. “Reversible” (adiabatic) CAPE, which includes ice phase in thermodynamic calculations, has been argued to be a more appropriate measure of conditional instability in the tropics (e.g. Williams and Renno, 1993). More recently, reversible CIN has been proposed as a better representation of the inflow layer for convective storms (Murdzek et al., 2021).

Response to Anonymous Referee #2:

General suggestion: little discussion of the role of vertical wind shear is incorporated, which is often considered to be an important control parameter on convective behavior. Especially for future investigators, I recommend that the authors reflect on this and consider discussing if and how this may play into the interpretations, perhaps in the conclusions section.

We agree that vertical wind shear is an important factor. Our preliminary analysis suggests that the shear conditions over Darwin vary among monsoon regimes. Isolating the influence of shear therefore requires a more detailed investigation beyond the scope of this study. We have added the following discussion to the manuscript:

In this study, we have focused on the thermodynamic links between entrainment, convection, and the large-scale environment. Vertical wind shear may additionally influence convective organisation, updraft width, and fractional entrainment (e.g. Peters et al., 2019, 2020b; Jo and Lasher-Trapp, 2022; Maybee et al., 2024). However, its role is difficult to isolate over Darwin because both the magnitude and vertical structure of shear vary across monsoon regimes, together with changes in the thermodynamic environment. Robust attribution would require separating these interacting effects across different monsoon regimes. We therefore leave a systematic investigation of the role of vertical wind shear to future work.

L272: 175,610 is approximately the total number of hours of this duration. Can you clarify if every hour of the dataset indeed meets the threshold for a rain event?

Here, 175,610 is the number of individual radar scenes within the identified rainfall events, not the number of hours. These scenes are retained after applying the rain rate and event duration thresholds. The original dataset contains substantially more scenes. We have clarified this as follows:

These identified rainfall events contain a total of 175,610 individual radar scenes. Only these radar scenes are used in the analysis.

L287-288: How is this exclusion accomplished?

The convective pixels are identified based on the Steiner et al. (1995) convective–stratiform classification. We exclude the stratiform pixels in the analysis. We have clarified this as follows:

For spatial characteristics of convection, we focus on the identified convective pixels and exclude pixels classified as stratiform. With this convective--stratiform separation, only moderate to deep convection is considered, and weakly or non-precipitating shallow clouds are excluded.

L290: This definition is identical to the description in L284-286, correct?

No, the convective rainfall contribution defined in L284-286 is the fraction of convective rainfall using only raining pixels. In L290, the convective area fraction (CAF) is the fraction of the entire radar domain classified as convective, with non-raining pixels included in the denominator. We have clarified the definition of CAF as follows:

where D_a is the total area of the radar domain, including non-raining pixels (~31,400 km^2)

Section 3.1: is the stratiform mask incorporated in any of these attributes of morphology?

The stratiform mask is not included in the morphology analysis. We have revised L287-288 (see response above) to clarify this.

Fig. 2: Add a legend for panel (b

We have added a legend for Fig. 2b in the revised manuscript.