Response to the reviews (manuscript egusphere-2025-2839)

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We are extremely grateful and sincerely thank the two Reviewers for their exceptionally thoughtful, careful and constructive comments on our manuscript. We feel priviledged to have benefited from such high-quality reviews. We also thank the Editor for ensuring such an efficient and effective review process. Below, we explain how we have addressed the Reviewers' comments and questions in the revised manuscript.

Main issues addressed in the revised manuscript:

- All Reviewers' comments and suggestions (point-by-point responses below).
- We clarified the transition from thermals to clouds by introducing the concept of 'cloud shoots', which correspond to incipient cloud bases formed immediately after thermals overshoot the lifting condensation level; we now connect the lengthscale and density of clouds before merging to the lengthscale and density of cloud shoots (or saturated thermals). We added new figures: Fig. 9c and Fig. S5 in the SI.
- We revised figure 10 so as to better illustrate the interplay between thermals and clouds for different strengths of merging.
- We clarified the physical interpretation of β (discussion in section 5 + new section 6.2).
- We clarified the interpretation of mass flux variations at cloud base (discussion in section 7.1 + new Fig. S6 in SI).
- We clarified the physical reason why β constrains the cloud fraction.
- We discuss hypotheses about how the merging occurs in practice and underline the need to clarify this further in the future.
- We checked the consistency of notations, numbers, colors throughout the manuscript. All figures are now color-blind safe.

Point-by-point responses to the Reviewers' comments are included below.

Point-by-point response to Reviewer 1's comments

We thank Reviewer 1 for their very careful reading of the whole manuscript, their throughtful comments and their help in ensuring a better readibility of the paper.

Summary

In this manuscript, airborne observations from the EUREC4A field campaign are used to study size distributions of thermals at several heights in the subcloud layer, and of shallow (and deep) cumulus clouds rooted in these thermals. The authors find that across a range of sampled environmental conditions, the thermal chord size distributions are well characterized by a single exponential relation close to the surface, and a sum of exponentials higher in the subcloud layer and near cloud base. Similar doubly exponential functions successfully fit the cloud-chord distribution near the cloud base.

Based on these observations, it is proposed that the double-exponential chord distributions are the result of thermal merging and cloud merging (the study's main conclusion). Several tests are carried out to support this hypothesis: First, it is shown analytically and by numerical experiment that by letting elements from a single exponential size distribution overlap, a second exponential of merged objects can emerge.

Second, based on the observed size distributions and the theory, the properties of the "unmerged" distributions (its length, density and merging efficiency) can be inferred. From these, it is found that the lengths of the predicted unmerged thermals matches well the lengths of surface-layer thermals and that the subsequently predicted thermal density matches well the observed (merged) thermal density. And third, the thermal density reduces with height in the subcloud layer, as expected by the merging process.

The authors go on to make several inferences from this theory (additional conclusions):

1. It is suggested that an observed double-exponential size distribution in cloud-base chords is due to a second merging process, where the clouds rooted in the second exponential distribution of thermals merge after reaching their condensation level. The two exponentials are then associated with two observed cloud-top populations: A shallow (single-thermal, single-cloud, non-drizzling) mode and a deep (merged thermals, merged clouds, drizzling) mode.

2. Weak inferred thermal merging relates to high thermal densities, and high thermal densities correlate strongly to mass flux and mesoscale vertical motion. It is suggested that strong mass fluxes thus develop when thermal merging is weak, and that when mass fluxes rise, the subsequent mass convergence will promote thermal merging and thus stabilize the mass flux.

3. Thermal merging and semantically distinguished cloud patterns relate: "Gravel" falls in the high thermal-density, high mass-flux regime and "Flower" in the low thermal-density, weak mass flux regime.

4. The factor beta, which is argued to measure the area of influence of a thermal through its induced circulation, predicts variability in observed cloud fraction well, because D0*L0 is rather constant at the value that maximizes cloud density. It also relates to cloud-top cloudiness, through an assumed interpretation of beta as the efficiency with which thermals attract their neighbors.

This is an excellent summary of our findings.

As this long summary indicates, the manuscript introduces many new ideas on the interaction of thermals, cumulus clouds and circulations, based on both novel data and new, creative analysis. These appear to open totally new ways to understand canonical quantities of shallow cumulus fields, such as cloud fraction, and its spatial organization. The manuscript will thus definitely deserve publication in ACP. At the same time, we believe the above conclusions could stand substantially stronger if the authors could further clarify the manuscript's key arguments, and streamline their presentation.

Thank you very much for these comments, and for pointing out so efficiently the arguments that needed to be clarified.

Our main comment is that while we find the main conclusion consistent with the presented evidence (and agree that it's a very likely outcome), there is no direct evidence that the observed size distributions develop from thermal merging. We do not ask the authors to present such evidence. But we do think a manuscript about thermal merging should suggest how, from a population of thermals in the surface layer, the thermal merging actually happens. Do the thermals grow in size and intersect? Attract each other (as hypothesised through beta)? Are they forced together by imposed mass convergence? Are more thermals triggered as one rises through the subcloud layer? And most importantly: How are such mechanisms captured by the idealized analytical and numerical merging experiments, and are they thus appropriate tests for whether thermal merging occurs in nature? Depending on the answer to these questions, we also wonder if the authors would phrase their main conclusion as strongly as saying that "analytical calculations and statistical simulations demonstrate that the two exponentials result from objects merging", as they presently do in the abstract.

Indeed, we don't have data showing exactly how merging works. However, the analytical model relies on the general condition that merging occurs when the basins of attraction of convective objects (the "basin of attraction" being defined as the region where a given thermal can capture its neighbours through the circulation it creates) overlap, and then explores the consequences of this condition. The statistical simulation is a little more prescriptive by relying on the condition that objects merge when they physically overlap (without any asumption on the physics underlying this merging), but still the focus is on the consequences of this condition on population statistics. The consistency of the predicted and simulated thermal densities in Figure 7d and the consistency of the predicted and observed thermal densities in Figure 8a suggest that merging is occurring in nature. Based on this, we keep our line in the abstract, "analytical calculations and statistical simulations demonstrate that the two exponentials result from objects merging". However, in the final discussion of the paper, we write: "In addition, this study demonstrates the role of merging in the size distribution of thermals and clouds, but does not show how exactly the merging (or the contact between two adjacent objects) takes place. It should be clarified with additional observations and/or simulations."

Similarly, the sub-conclusions rely on "cloud merging" (as explained around line 320), but despite our best efforts reading Figure 10, we were unable to deduce where or how exactly "cloud merging" occurs. The authors suggest a population of merged clouds exists already at the cloud-base height, but that its "pre-merging" length scales are those of the second thermal population. So where did the clouds actually merge?

We address this issue by discussing further the properties of saturated thermals (about 20 % of all thermals at cloud base): these thermals have, on average, a size ($L_1^{TH,sat}$, $L_2^{TH,sat}$) and density that are roughly similar to the size and density of clouds before merging: L_0^{CLD} is bi-modal (when there is only one cloud population $L_0^{CLD} \approx L_1^{TH,sat} \approx L_1^{TH}$, and when there are two cloud populations, $L_0^{CLD} \approx L_2^{TH,sat} \approx L_2^{TH}$), and $\mathcal{D}_0^{TH} \approx \mathcal{D}_{sat}^{TH}$. Saturated thermals can thus be considered as 'cloud shoots'. Their density varies together with the total thermal density (Fig. S5). When it is high, the density of cloud shoots is also high, which favors their merging and the formation of larger cloud bases. Since most saturated thermals are buoyant (84% of them have a mean positive vertical velocity at cloud base) they grow easily; while the growth of thermals is limited by the height of the subcloud layer, the growth of clouds is less limited and clouds can deepen more, which induces a circulation that becomes more intense than the circulation induced by thermals. We think it is the reason why the cloud shoots merge quicker (or more easily) than the thermals, and form large cloud bases. This is amplified by the fact that the cloud-induced circulation leads to a mass convergence near or below the cloud base, which can increase the thermal density even more. This understanding is supported by additional figures (Fig. 9c, Figs. S4 and S5), and encapsulated in a (revised) cartoon (Figure 10). The cloud shoots now appear more clearly (white clouds on the figure), as well as the cloud-induced circulations. We also changed the way we represent the merging of thermals across the depth of the subcloud layer to make it more compelling.

We have many other comments, some of which may reflect us wrongly parsing the manuscript. We do, however, have the general impression that this might partially arise because some of the presentation could be sharpened. We have tried to divide our comments into detailed comments that we would welcome answers to, and detailed comments which we suggest the authors implement in the manuscript, but which do not require answers unless the authors wish to reply.

Detailed comments

Lines 104 – 110: Is there a physical motivation underlying the adopted thermal definition? Are the presented results sensitive to this definition?

We used the thermal definition proposed by Lenschow and Stephens (1980), which is based on humidity fluctuations. During the course of our study, we also considered definitions based on combined fluctuations of humidity and vertical velocity and/or relative humidity (to detect saturation). Whatever the thermal definition used, we always find that the chord length distribution is well fitted by a sum of two exponentials at cloud base (compare for instance Figures 1 and 3). However, when we consider several variables to detect thermals, the number of detected thermals decreases drastically (e.g. by a factor of about 5 if we add a vertical velocity or saturation criteria), which makes the fitting of the statistical distributions challenging for individual flights. Therefore, to ensure that the statistical distributions of thermal chord lengths are well fitted for each individual flights (and not only when aggregating the data of the whole campaign), and that the thermal sampling is large enough to look at statistical relationships between thermals and clouds for different cloud patterns, we used the thermal definition that provides the largest samples, i.e. the definition proposed by Lenschow and Stephens (1980).

Line 123: Are p1 and p2 proportions in amount of thermals, or in area of thermals?

In section 5.1 we now write: "fitted by a mixture of two exponential functions in (number) proportions p_1 and $p_2 = 1 - p_1$ [...]." We also discuss (lines 250-257) the interpretation of p_1 and p_2.

Lines 130-135: How are "cloudy thermals" different from the "clouds" studied later in the manuscript? Could these cloudy thermals help explain how the merged thermal population translates into a cloud population, which then merges again? And if not, could the authors just totally ignore the cloudy thermals and updrafts, given that they play no further role in the manuscript?

Yes, the 'cloudy thermals' help understand how the thermals translate into cloud populations. They correspond to those thermals (about 18% of the total) that are saturated; we consider them as 'cloud shoots', i.e. the incipient cloud bases that form immediately after the thermals saturate and can eventually merge if they form close enough to another shoot or an existing cloud. The density of cloud shoots is strongly correlated to the total density of thermals (SI Fig S5). The idea that these saturated thermals correspond to cloud shoots is further supported by the fact that the density of cloud shoots $\mathcal{D}_{sat}^{TH}$ and $\mathcal{D}_0^{CLD}$ are of a similar order of magnitude (Fig. 9c). This discussion is now presented in section 6.3.

Table 2 & Figure 5: There are multiple flights in which the table and the figure do not appear to agree. For instance, for RF05 p1CLD = 0.5 in Table 2, while it is 1 in Figure 5. Because of line 359, we believe P1 should be 1? We might have overlooked more such inconsistencies, and wonder if the authors could double check their terminology.

In the case of a single population, $L_1 = L_2$ whatever p_1 or p_2 provided that $p_1 + p_2 = 1$. In this case (and only in this case), the fitting procedure can return any p_1 value (including 0.5 or 1). To prevent this discontinuous behaviour, and be more consistent with the analytical calculations of section 5.1 which are continuous around this particular case ($p_1 = p_2 = 0.5$ and $L_1 = L_2$ in the case of a single population), we imposed a 'penalty' in the fitting procedure so that the p_1 estimate varies more smoothly and tends to 0.5 (instead of 1) in this particular case. All the analysis has been done with the fit parameters calculated with a penalty, but for some reason the fits reported in Fig. 2 and Fig. 5 of the submitted manuscript were calculated without the penalty (thank you for catching this!). The two figures (and the new figure S3 of the Supplement Information) have been corrected and are now fully consistent with the Tables and the other figures.

Line 143: What is the reason to adopt a resolution of 25 m for the thermal identification?

Our turbulence measurement are available at the same frequency (25 Hz, 4 m resolution) as those of Lenschow and Stephens (1980), and a 25 m segment includes 6 measurement points. By choosing that thermal chords are at least 25 m long, we ensure that our thermal definition is consistent with that of Lenschow and Stephens (1980), so that we can compare our statistics with their results.

Line 145-150: Could the authors explain how the cloud-base widths were calculated from the horizontal remote sensing?

The text now says: Using the hydrometeors classification derived from the synergy of the lidar-radar remote sensing over a range of several kilometers away from the aircraft (Delanoë et al 2021, Bony et al 2022), we detect the length of cloud segments, or chords, along the line of sight of the lidar-radar measurements, perpendicular to the aircraft trajectory. The horizontal resolution of the hydrometeors classification along the line of sight of the radar and lidar is 25 m. A segment (or chord) corresponds to at least 2 continuous points associated with cloud or drizzle, i. e. reflectivities lower than 0 dBZ.

Line 153-154: Given the many different populations (initial, merged and unmerged thermals and clouds, as well as their distributions' proportions, lengths, densities and effective factor), it would be helpful to the reader if a full introduction to all parameters, and their relation (at least for thermals) were presented together near eq 1. For instance, this explanation of L might be better fitting closer to its first mentioning in line 127.

We added near eq. 1: Following these notations, the mean size of thermal chords is given by $\mathrm{L} = \mathrm{p}_1\mathrm{L}_1 + \mathrm{p}_2\mathrm{L}_2$. Moreover, if N is the total number of thermal chords intersected by the aircraft along a horizontal distance of $\mathcal{L}$, the mean thermal density for this distance is given by $\mathrm{N}/\mathcal{L}$.

170-171: Does the approximation of L2CLD of the boundary layer height have an interpretation?

We have hypotheses in mind but clearly the interpretation of this feature requires further investigation.

Line 190: Could the authors define the merging efficiency explicitly in this paragraph? Since it is a somewhat abstract concept, we also suggest to elaborate somewhat on the introduction of the effective radius. For instance, by already giving a feeling for typical sizes of beta (as later e.g. reported in figure 8).

To clarify the physical meaning of the effective factor β, we added a few lines at the beginning of section 5 (lines 202-211), and a new subsection (6.2: Physical interpretation of β – in which we mention the range of β values).

We have two questions and two remarks regarding Figure 6:

Figure 6b is not discussed in the text at all, but is intriguing: Should not p2 increase as merging probability rises? Can the authors elaborate and include the figure in the text (or ignore if they see fit?)

We added (lines 250-257): "Although the physical meaning of L_1 and L_2 is clear (these length scales correspond to the mean chord lengths of unmerged and merged thermals, respectively), the physical meaning of p_1 and p_2 is not so clear. When $\beta\mathcal{D}_0 L_0 \to 0$ (i. e., no merging), equations 8 and Fig. 6b show that $L_1 = L_2$ and $p_1 = p_2 = 0.5$. However, when $L_1 = L_2$, any values of p_1 and p_2 satisfying $p_1 + p_2 = 1$ (including $p_1 = 1$ and $p_2 = 0$) would describe the same (single) exponential size distribution. Therefore, p_2 should not be interpreted as the proportion of thermals in the second population (it is just an asymptotic approximation) and the ratio $\frac{\mathcal{D}_2}{\mathcal{D}_1+\mathcal{D}_2}$ is a better measure of the proportion of merged thermals than p_2. In addition, the influence of merging on the size distribution is best described by L_2 - L_1 or (as will be shown later, Fig. 8b) by the non-dimensional quantity $\frac{L_2-L_1}{2L_0}$, and the absence of merging is best described by $L_2 \rightarrow L_1$ or by the density of merged thermals $\mathcal{D}_2 \rightarrow 0$. "

Could the authors elaborate more on what explains the peak value in the overall density? We can imagine that if one adds thermals, then the density first increases, but then so much merging would occur that adding more thermals reduces the density. Yet it is harder to imagine how this would work if one instead increases the merging efficiency by increasing the L0 or beta – why would then the density of thermals ever increase?

You give the correct interpretation of why $\mathcal{D}^{TH}$ is initially positively correlated with $\mathcal{D}_0$, and why, after a critical point is reached, $\mathcal{D}^{TH}$ becomes negatively correlated with $\mathcal{D}_0$. If $\mathcal{D}_0$ is fixed but β or L_0 increases, then $\beta\mathcal{D}_0 L_0$ increases and $\mathcal{D}^{TH}$ decreases (the dashed line on Figure 6c shows that $\beta\mathcal{D}L_0$ increases sub-linearly with $\beta\mathcal{D}_0 L_0$). Therefore, $\mathcal{D}$ is a monotonically decreasing function of β and L_0 that exhibits a maximum as a function of $\mathcal{D}_0$. Note that the inferred values of $\beta\mathcal{D}_0 L_0$ in observations tend to lie above the critical point and therefore we are most interested in how varying β would diminish $\mathcal{D}^{TH}$ rather than how β might enhance $\mathcal{D}^{TH}$.

Annotations a, b, c are missing.

Added.

6c: Can the authors be more specific in the caption about what the legend items mean? The legend says it's D1 and D2, but the legend has only products of L0*beta.

Done.

Lines 285-287: Can the authors motivate how mesoscale circulations could enhance the thermal density in ways that the merging theory does not capture? Why would mass convergence in the subcloud layer yield different merging behaviour than what the theory can capture? Could that not be expressed through a larger beta?

Yes. We mention this possibility in section 6 (lines 319-322: "These factors probably include the influence of the low-level convergence associated with the circulations created by cloud updrafts or shallow mesoscale circulations such as those revealed by George et al. (2023), which can increase the thermal density below the clouds (Rousseau-Rizzi et al, 2017) but are not included in the merging theory, nor in the simple statistical simulations.") and in section 6.2 (lines 347-348) and in section 8.2 (lines 628-636): "Another interesting feature is the underestimate, compared to observations, of the thermal density predicted by theory in situations of maximal thermal merging or minimal thermal density after merging (Fig. 8a). This discrepancy suggests the influence of additional processes in the control of the thermal density. These processes might include the influence of mesoscale circulations, which concentrate thermals in ascending branches (as shown by Fig. 11b at the scale of a 200 km circle), or the presence of cold pools, which may concentrate thermals and thus favor thermal merging at their edge. The discrepancy may also result from the mass convergence induced by clouds in the subcloud layer, which may influence the distribution of thermals beneath clouds and thus thermal merging but is not adequately accounted for by the effective factor of thermals (because it arises from clouds). These influences will need to be studied".

Figure 8 d) This figure suggests that the variability in beta explains virtually all the variability in thermal merging efficiency itself. Indeed, it is later mentioned that L0*D0 appears constant. Beta is hypothesized (through arguments presented in the appendix which appear entirely plausible) to denote a radius of attraction of a thermal or cloud, but insofar as we understand from the main text, it is essentially treated in the analysis as an additional free parameter which makes the analysis work, but which could in principle have many different interpretations (for instance, could beta not just be the enforced merging of thermals by an imposed mass convergence?). Hence, is there a risk that the authors' interpretation of the factors that control thermal merging (and thus subconclusion 4) leans rather heavily on the somewhat enigmatic beta, and not on the directly interpretable thermal density or their sizes themselves? And would the arguments strengthen if the authors spent some more space and time explaining and underpinning what beta is? The analysis in the appendix seems like a nice starting point.

We added a subsection (6.2: Physical interpretation of β), in which we included part of the material that was previously in Appendix A, on the different physical interpretations of β. At the beginning of section 5 (lines 202-210), we also elaborate on the different physical processes that can contribute to β.

Figure 8 d) The inter-flights variability is higher than the intra-flight variability. So, the "thermal property", beta does not vary much within a single flight. What does this mean? Is beta caused by a scale larger than the scale measured within one flight (30 km, line 107). Is this also affected by model choices? Is the coverage fraction of thermals in Eq. 11 (from which beta is inferred) a single value per flight, or is it calculated per thermal? In the first case: does this reduce the beta intra-variability?

β^{TH} is based on the coverage fraction of thermals and the thermal merging efficiency. These two quantities are estimated based of a thermal population. They are estimated for a flight pattern that is long enough to sample a large number of thermals. This flight pattern is either a rectangle at cloud base (each flight includes 2 to 4 such rectangular patterns) or a L-pattern flown in the subcloud layer (there are 2 such patterns per flight). For this reason, our β^{TH} estimates are statistical properties and cannot be estimated for each thermal (a single segment of 30 km samples a population of thermals, but might not be long enough to allow for a robust characterization of the thermal size distribution). However in the future it would be interesting to connect this quantity to characterizations of the circulation induced by each thermal.
In the text, we added in the section on open questions and perspectives (lines 639-645):

"[...], this study emphasizes the role of thermal- and cloud-induced circulations in shaping mesoscale organization and cloud patterns. Within the analysis framework presented here, these circulations are conceptualized by the effective factor β, which quantifies the basin of attraction exerted by a convective object on its surroundings and influences the merging process as if convective objects had an effective size β times their actual size. The processes that govern β remain to be clarified. For instance, how should we interpret the fact that inter-flight variability of β^{TH} is larger than its intra-flight variability (Figure 8d)? A deeper investigation into the dependence of β on convective object properties and environmental conditions would help answer this question. "

If these are open questions, this can also be noted in section 8.2 instead of close to Figure 8d.

Lines 315-320 and Figure 9b: We struggled with the interpretation of this figure. Could it be made clearer if each estimate of L0cld were accompanied by its own estimate for Lth1 and Lth2 (where appropriate) from the same flight?

We revised Figure 9b to make it simpler. We removed the size range of subcloud-layer thermals (we just present the range of cloud-base thermals with the shaded areas), and we now indicate more clearly the presence of 1 or 2 cloud populations (in the marker itself). We also added a figure to the Supplementary Information reporting, for each flight, the L_1^{TH} and L_2^{TH} estimates that are associated with each L_0^{CLD}: L_0^{CLD} coincides with L_1^{TH} when the flight is associated with only one population of clouds, and L_0^{CLD} coincides with L_2^{TH} when the flight is associated with two populations of clouds. It shows that when there is only one cloud population, the clouds are primarily rooted in unmerged thermals, but when there are two cloud populations, the clouds are primarily rooted in merged thermals (which does not exclude that clouds topping unmerged thermals also merge with clouds topping merged thermals). We did not include this additional figure in the main text (and did not replace Figure 9b) because it conveys basically the same message as Figure 9b, and because we liked presenting the time series of L_0^{CLD} in Fig. 9b as we presented the time series of L_0^{TH} in Fig. 9a.

Figure 10 forms a very helpful visual overview of the merging process, but is first used only in section 8.1. Could it be used before, for example, around lines 355-360? And more generally, to explain how merging is suggested to happen? Later, a similar comment holds for Figure 13, which is not used in the text at all.

We now present Figure 10 at the end of section 6 (lines 391-402): "A schematic of the impact of the merging process on thermals is represented in the lower half of Figure 10: turbulence near the surface produces a large density of thermals. As they rise across the depth of the subcloud layer, some of them merge and become wider. This results in two thermal populations coexisting in the subcloud layer and near cloud base: those that have merged (of length scale L_2^{TH}), and those that have not merged yet (of length scale L_1^{TH}). As a result of merging, the thermal density decreases with height. The thermals that overshoot the lifting condensation level (about one out of five on average during EUREC^{4}A) saturate at their top and form 'cloud shoots' whose base has initially the same size as the saturated thermals that produced them ($L_0^{CLD} \approx L_1^{TH} \approx L_1^{THsat}$ or

$L_0^{CLD} \approx L_2^{TH} \approx L_2^{THsat}$, Fig. 9b). As will be shown later (Fig. 12c), a higher density of thermals ($\mathcal{D}^{TH}$) is associated with a higher density of saturated thermals $\mathcal{D}^{TH}_{sat}$ (Fig. S3, consistent with the fact that when the density of thermals is high, the boundary layer is moister and the LCL is lower) and thus a higher density of 'cloud shoots' ($\mathcal{D}_0^{CLD}$, Fig. 9c). When cloud shoots form close to each other (which occurs more easily when thermal merging is weak and thus the thermal density around cloud base is high), they can merge. It forms larger bases and leads to the formation of wider and deeper clouds."

Moreover, at the end of section 7.2 (lines 501-508), we write: "Vertical and plan views of the interplay between thermals and clouds are represented schematically in Figures 10 and 13. The left-hand side of the cartoons correspond to a case of weak thermal merging (and thus high thermal density), and the right-hand side to a case of strong thermal merging and low thermal density. The two sides thus correspond to Gravel- and Flowers-types of mesoscale organization, taking into account that the very shallow clouds topping unmerged thermals (represented in the middle of Fig. 10 or around deep clouds in Fig. 13) are also part of these patterns. In the Flower case, deep clouds are represented with an extended cloud coverage at their top (a shallow anvil): it results from the water detrained from the convective core during the lifetime of the convective clouds, which can be particularly long in situations of strong thermal merging and large β^{CLD} (Fig. 12d)."

Lines 365-370: To reach conclusions 2 and 3, the authors find that the density of thermals is decisive for cloud-base mass fluxes, and that merging thermals reduces the mass flux. This seems counterintuitive: Why would merging two individual thermals, each with a size and a vertical velocity, not conserve their mass flux upon merging? And why would this be different for thermals than for clouds? Perhaps some of the suggested answer might lie in figure A1, where the authors show that merged thermals are assumed to have overlap, and thus a smaller area – if this is so, could the explanation be clarified and moved to the main text? Finally, given the authors' own statement that mass flux is expected to scale with the area fraction of clouds, why then do they suggest Dth, rather than Dth*Lth (and later Dcl*Lcl), to be the factor that controls mass flux?

We clarified this important issue by adding a figure to the Supplementary Information (Fig. S6): The mesoscale mass flux M_b inferred from ATR measurements at cloud base (section 7.1, Figure 11a) increases with the thermal density $\mathcal{D}^{TH}$. This co-variation can be interpreted by noting that $M_b \approx w^{TH}_{sat}.\mathcal{D}^{TH}_{sat}.L^{TH}_{sat}$, where $\mathcal{D}^{TH}_{sat}$, L^{TH}_{sat} and w^{TH}_{sat} are the mean density, length and vertical velocity of cloudy thermals (or cloud shoots) inferred from turbulence measurements during each flight. This approximation provides mass flux estimates that explain well the flight-to-flight variations of M_b ($R^2 = 0.97$). Consistently, it correlates with $\mathcal{D}^{TH}$ almost as strongly as M_b does ($R^2 = 0.68$ vs 0.74). The correlation with $\mathcal{D}^{TH}$ of each term of the approximated mass flux estimate ($\mathcal{D}^{TH}_{sat}$, L^{TH}_{sat} and w^{TH}_{sat}) shows that the increase in mass flux with $\mathcal{D}^{TH}$ is primarily due to the increase in cloud shoots density $\mathcal{D}^{TH}_{sat}$ and, to a lesser extent, to the increase in vertical velocity w^{TH}_{sat}. "

In the main text (lines 441-447), we also added: "The flight-to-flight variations in M_b can also be interpreted as a result of variations in the thermal population. Noting that

M_b can be well approximated by the product of the mean density, length and vertical velocity of cloudy thermals $M_b \approx w^{TH}_{sat}.\mathcal{D}^{TH}_{sat}.L^{TH}_{sat}$ (Fig. S6 of the Supplementary Information), it appears that M_b variations are primarily governed by $\mathcal{D}^{TH}_{sat}$ variations (and to a lesser extent by w^{TH}_{sat} variations), which are roughly proportional to variations of total density of thermals $\mathcal{D}^{TH}$ (Fig. S5). In other words, a weaker thermal merging is associated with a higher density of thermals ($\mathcal{D}^{TH}$) and saturated thermals ($\mathcal{D}^{TH}_{sat}$); this leads to a higher density of cloud shoots ($\mathcal{D}^{CLD}_{0}$, Fig. 9c) and thus promotes cloud merging and the formation of wider cloud bases (L^{CLD}_{2} increases), which eventually leads to a stronger mass flux."

Lines 390-391: Does this mean that the left graph in Figure 10 is expected (or measured) to contain more rain than the right graph?

We didn't mesure the rain rate, unfortunately, but the fractional rain area (measured for each flight, Table 2) is larger in the Gravel case than in the Flower case. Radtke et al. (2021) suggests that in the trades, the mean precipitation amount (as opposed to the rain intensity) scales with the precipitation fraction or number of precipitating cells. Therefore one may expect the precipitation amount to be larger in the Gravel case than in the Flower case, but our precipitation statistics might not be robust enough to conclude about this.

Line 392: "Fig 12a shows [...] the situations with only one cloud population (and thus no cloud merging by definition)". These also stand out in Figure 12b – maybe it is worth mentioning this explicitly?

Done.

Section 7.3 (building towards conclusion 4) presents an intriguing framework diagnosing cloud fractions at both cloud base and cloud top from β_{cld}, the ratio of inflow and outflow layers of a cloud. These ideas are presented quickly and base themselves on purely theoretical arguments made in the appendix, but are then used to suggest fairly fundamental and seemingly important constraints on cloud fraction. Like with β_{th}, we wonder if the conclusions would strengthen if the authors took some space in the main manuscript to explain in more detail why they believe this is so. The observation data underlying this section is also introduced in passing, and could be integrated more fully in section 2.

We added (lines 524-531): "Then, how to physically interpret the fact that β^{CLD} constrains the cloud fraction? The f_{max} limit corresponds to the maximum cloud fraction for which the clouds' basins of attraction remain non-overlapping. In a cloud field with an area fraction $1/\beta^{CLD}$, then any new clouds born in the domain would necessarily be within an existing cloud's "basin of attraction" and would therefore merge with that cloud (in the simplest case where $\beta^{CLD} = 1$, a new cloud born in a region with a cloud fraction of unity would necessarily imply overlap and merging with existing clouds and no further increase in cloud fraction). Another interpretation is that the circulation induced by clouds likely promotes a mass convergence around their base that favors the merging of thermals and thius decreases the cloud base fraction (Fig. 10).

Line 538 (and other places in the concluding section): It is suggested that the behaviour of the observed size distributions across environments might be studied in LES. Since the authors find that the size of the small thermal population is on the order of (100-120 m) in the subcloud layer, which is decidedly smaller than the effective grid spacing of most LESs of shallow convection – is LES actually a suitable tool for such analysis? Might the authors'work not rather raise the question whether thermals are properly parametrized? Moreover, it might cause modelers to reconsider the designs of their experiments. Therefore, the authors could consider mentioning these potential impacts.

A spatial resolution finer than 100 m will certainly help describing the thermal field as has been done here with observations. However, even with a grid spacing of 100-120 m, we expect to find two populations of objects (with characteristic length scales larger than in observations) and an interplay between thermals and clouds that may be qualitatively similar to the one inferred from observations. We are actually exploring this issue by analyzing LES simulations, and the results will be reported in a future paper.

While interesting, the appendix is dense and was hard to parse. We have several specific questions, but also recommend the authors to more fully embed their explanation of beta in the main text, given its novelty and importance to their interpretations:

Equation A1 and figure A2: Is the horizontal velocity u1 induced by thermal 1 dependent on the distance to thermal 1? It does not seem so. Could the authors justify why this is reasonable? Is the distance between thermals interacting so small that it should not matter? Are there mechanisms that make the reach of indraft very wide?

In all the statistical analysis, we have considered a unidimensional case (thermals are only characterized by their width). Noting x the direction of the flight track and z the vertical, our thermals are then invariant by translation along the y direction. This is reasonable as long as thermals have more a linear structure, with an open cell network of ascending regions, rather than a 'bubble structure' with an axisymmetric geometry. Moreover, it is necessary to ensure consistency between the model for β, and the rest of the statistical analytical computations where we have computed size distributions along the x direction only and assumed unidimensional thermals. Mass conservation then imposes that u_A is independent on the distance to the thermal. If thermals had an axisymmetric geometry, u_A would decrease as 1/r, so indeed our choice of geometry for the thermal is important.

Line 625: Does this assume that the $T_{transit}/T_{life}$ time of an updraft is equal in both thermals, and should we expect this? Is there an assumption that u1 equals u2 and is this a necessary assumption?

This indeed assumes that the fraction $T_{transit}$ / T_{life} is the same for the two thermals. We could expect this to be similar, since : 1)the vertical velocity within thermals (that controls $T_{transit}$) is known to have much less variability than the size of the thermals. 2) the lifetime of the thermal can be hypothesized to be controlled by mesoscale or large scale factors such as relative humidity. There is no assumption on u_A and u_B, if L_A and L_B are different they are actually also different because of mass conservation.

Lines 643-644: “The merging between those effective updrafts acts as shown on figure A1.” However in 604: “However, this view is partly erroneous.” This seems contradictory, therefore it could be explicitly mentioned/showed in A1 that the size of Thermal A is the effective size.

Figure A1 has been redrawn and now shows the merging for $\beta = 1$ and $\beta > 1$.

Should there be a downdraft near the thermals? And if the downdraft is further away than the other thermal, could the authors comment on why? In line 604, the assumption that the downdrafts are small/far away seems to be implicit.

Yes, there is an assumption that the downdraft is far away, which has limitations. In Poujol (2025), another model is introduced for the circulation around convective updrafts which accounts for the presence of the downdrafts. It will be the object of a separate study ; the circulation in this model is similar to the one shown on the new figure 10. Applying this model to the merging process has been done in Poujol (2025) and yields formally similar results with h/H = h'/H = $1/\pi$ ($\pi = 3.14$). So accounting for the downdraft does not change the physical interpretation of the results. We believe the simple model presented here with no downdraft is more pedagogical and helps to keep the focus on the statistical merging process.

In Figure A1 it is shown that the area of thermal AB equals the area of thermal A + the area of thermal B – overlap area. However, the analytical merging model is based upon thermals moving towards each other. Looking at Figure A2, it seems like the authors assumed that the horizontal distance moved by the thermals is negligibly small. Is this indeed assumed?

While the thermals move towards each other, the ”center of velocity” of the ensemble of the two thermals (defined as their center of mass, but using w as the variable instead of density) remains fixed (just as in the two body problem in mechanics). Therefore, assuming that w is more or less uniform (most of the variability is in the thermal size rather than velocity), the geometric center is located at the same place as the ”center of velocity” and it also does not move during merging. So, if two thermals merge, they both move but their geometric center does not move, so that the merged thermal center stays at the same position.

Detailed comments (no need for response)

Figure 1d. Could the authors elaborate in the text what this figure shows? It is not referred to at present.

Done.

Line 108: Could the authors clarify here whether the flights are around 300 or around 600 meters, or are they between 300 and 600 m (as higher flights are between 600 and 800 m).

We now write: ”near the sea surface (at a height of about 60 m, 11 flights), within the sub-cloud layer (in the middle of it – around 300 m – and near the top of it – around 600 m, 16 flights) and just above the cloud base level (between 600 and 800 m, 17 flights).”

Table 1 RF05 surface layer has '-' instead of numbers. Could the authors explain this? Did RF05 not fly in the surface layer, not detect thermals or something else?

We write: "The flights (or flight segments) without data are indicated by '-': no turbulence data are available for RF20 (failure of the inertial navigational system) and on the near-surface leg of RF14 (humidity measurements of bad quality), the near-surface was not sampled by the aircraft in RF05, RF07, RF08, RF09 and RF17, and the subcloud layer was not sampled during RF16."

Figure 3: Can the authors explain in the text from where the vertical velocity data are retrieved? From the turbulence measurements aboard the ATR?

In section 2, we now say more clearly: "The aircraft measured turbulence (including horizontal and vertical velocity, inferred from the measurements of a five-hole nose radome) and humidity at a fast rate (25 Hz) using...".

Figure 4: there are no figures c and d. The caption and line 161 refer to 4c-d.

Corrected. We now refer to Fig. 1b and Fig. S3 of the SI.

Figure 4a: it seems like p2 should be bigger than 0.1

The values are correct.

Figure 4b: Are the annotated proportions correct? There does not appear to be a the second population.

Yes they are. When there is only one cloud population, $L_1 = L_2$ and p_1, $p_2 \approx 0.5$ (Fig. 6).

Line 157: Where in table 2 can one find the mentioned +/- 15% variability in L1 and L2 between flights? In the table, it appears the 1st cloud population's std is 4, while it is 25106 for the second cloud population.

$\pm$ 15% was only for L_1^{CLD} and we had forgotten to report the variability in L_2^{CLD}. In the revised version, we removed these numbers because looking at Table 2 or Figure 2 is a better way to appreciate the variability in L_1^{CLD} and L_2^{CLD}.

Line 251: "pre-specified value". And line 253: "pre specified value f_{TH}". It would be nice if the pre-specified value would be defined when first mentioning it.

Done.

Figure 8 a) y-axis is D from observations. C) y-axis is measured D. Are these the same quantity?

Yes. We now use the same word in both panels.

Figure 9a (and also figure 4, and subfigures in figure 2 and figure 5) - some y axis labels appear to be cut off?

Corrected.

Lines 375-376: the authors refer to highlighted flights. At first it was unclear to us that the five flights are associated with sugar patterns and are highlighted with green and that the six flights are associated with Gravel/Flowers patterns and are highlighted in yellow & red. The authors could help the reader by mentioning this more explicitly.

Done.

Figure A2: Some lines are thicker than others – is this needed?

The lines are thicker when the composition of the two wind fields generates a stronger wind than the wind before the interaction.

Spelling mistakes:

an updraft instead of a updraft
Corrected.
48. George et al. (2023) should be placed in brackets.
Corrected.
101. Ascending anomalies which are called thermals
Done.
119. No brackets around Lenschow and Stephens (1980)
Done.
127. superscript ‘th’ in Figure 1 and ‘TH’ in Figure 2, Table 1, line 127, ... Moreover, the authors could consider explicitly defining it as a reference to thermal.
Done.
284: Nevertheless instead of Neverthess.
Corrected.
448: Feb instead of Fev.
Corrected.
601 and 644: Figure instead of figure.
Corrected.
Supplement: homothety instead of homothethy.
Corrected.
Supplement: an updraft
Corrected.

Point-by-point response to Reviewer 2's comments

We thank Reviewer 2 for their very careful reading of the whole manuscript, their throughtful comments and their help in ensuring a better readibility of the paper.

Summary

In this study, the authors examine the behaviors of thermals, and the cumulus clouds they support in the trade-wind region. They use a combination of observations taken from multiple platforms during the EUREC4A campaign to examine the interplay between thermals and clouds. Specifically, they find that their size distribution can be explained by the sum of two exponentials which they further determine to be related to "merged" and "unmerged" object (i.e., cloud or thermal) populations. This theoretical framework describing the relationship between merged and unmerged populations controlling the total behavior of trade thermals and clouds comes from extensive analytical calculations that are further validated through comparison with a one-dimensional statistical model. The attraction between clouds due to their convective circulations, helping this merging to happen, is also mathematically detailed. Once tested, the authors show the strength of this merging population framework through interpreting the EUREC4A (and later the deeper convective MAESTRO) observations. They find that the behaviors can be described well as interactions between merged and unmerged populations of clouds and thermals. Such interactions appear to shape the characteristics of the mesoscale cloud patterns observed in the tropics (e.g., gravel vs. Flower behaviors) and set the upper bound on the amount of cloud cover, which has crucial implications for trade cumulus feedback.

This is an exceptionally well-crafted and thoughtful study, expertly grounded in the literature and employing a wealth of cutting-edge observations as well as classical theory and statistical models. The results have the potential to revolutionize how the field thinks about tropical cumulus clouds; their development from and interaction with thermals; and how this interplay shapes cloud organization and ultimately influences the climate. In particular, the detailed analytical calculations and elegant theoretical framework developed in this work is a significant step forward in describing how cumulus, and potentially deeper convective clouds, develop, organize, and persist. This will likely be a major contribution to the field, advancing the fundamental understanding of cloud behaviors with critical implications for how they will respond under future climates and how we can improve their representation in high resolution models. I have noted places where analysis details could be clarified and otherwise urge prompt publication of this excellent manuscript.

Detail Comments

General: Please provide R2 values for scatter plots and be consistent about using R2 throughout (later figures use R instead). Assume these are all Pearson (Line 443) correlations at 95% confidence? Worth mentioning that somewhere as well.

Done.

Main Text

Figure 2, 5: Following the Q-Q plot in Figure 1, please provide the R2 values for the individual flight fits.

Done.

Line 50: It might be worth noting that many clouds are not classified into any of these four patterns (e.g., Schulz 2022)

True, but this sentence is about the 'most prominent patterns'.

Line 132: What instrument are you getting the vertical velocity from? Apologies if I missed this in the methods.

We added in section 2: "The aircraft measured turbulence (including horizontal and vertical velocity, inferred from a five-hole nose radome measurements) and humidity..."

Figure 4, Lines 159-161: It looks like the figure described in the text and the caption do not match the actual figure. However, it sounds like a very intriguing result, so please include.

We added to the Supplementary material (Fig. S3) a figure similar to Fig. 5 but considering non-drizzling clouds only. In that case, $L_1^{CLD} \approx L_2^{CLD}$ and the distributions are well fitted by a single exponential.

Figure 5: It might be helpful to indicate the heights (or at least the dominant types of legs that were flown) in each flight. It could help to illustrate your point about change in exponential behavior with height (i.e., one to two populations).

All panels of this figure show the cloud chord length distributions at cloud base (this level was sampled several times during each flight). The PDF is fitted by one or two exponentials depending on the cloud types in presence.

Figure 6: Please describe in more detail what is being shown in the different panels in the text. I think I follow your logic, but it would help to have your reasoning more explicitly stated. Also please label the panels (a-c).

Done.

Line 193: Just to be clear, the betaD0L0 is the ratio to determine if there is merging?

Yes. We added: "These physical arguments thus suggest that the product $\beta \mathcal{D}_0 L_0$ describes a merging efficiency."

Line 217-220: where are you looking in Figure 6? Would be helpful to describe in a bit more detail how you see this.

We now write: "These calculations, illustrated by Fig. 6, thus show that the merging of thermals that are characterized initially by an exponential size distribution of length scale L_0 produces a second population of thermals, and that the size distribution of the thermal population after merging can be represented by the sum of two exponential functions, characterized by two length scales L_1 and L_2. In the absence of merging ($\beta \mathcal{D}_0 L_0 = 0$), $L_1 = L_2 = L_0$ (Fig. 6a) and $p_1 = p_2 = 0.5$ (Fig. 6b): the size distribution can be represented by a single exponential." and later: "There is therefore a critical merging efficiency of thermals beyond which the merging becomes so efficient in producing larger but fewer thermals that the densities of thermals before and after merging become anti-correlated (Fig. 6c)."

Line 224: I don't understand how you got this result, could you please add a little more detail?

The value of $\beta\mathcal{D}_0 L_0$ that maximizes $\mathcal{D}$ is found by solving the equation $\frac{\partial \mathcal{D}_1}{\partial(\beta\mathcal{D}_0 L_0)} = 0$ (added to the text).

Line 232, eq. 9: Would you please explain how you get to this in more detail? And why you are using a Lambert W function here?

We obtain this equation by combining equations 3 and 4 to eliminate L_0 and obtain an implicit equation for $\beta\mathcal{D}_0 L_0$.

Line 234, eq. 10: How do you get to the L0=sqrt(L1L2) result here?

We added: the second expression for L_0 is obtained after a multiplication of equations 3 and 4, followed by a first order Taylor expansion of the exponential function.

Line 259-261: Do you think the merging here is assisted by mesoscale circulations in the real world? E.g., Janssens et al. 2023, 2024

In the real world yes (it is discussed later in the paper), but by construction it is not the case in these simulations with a homogeneous $\mathcal{D}_0$.

Line 268, Figure 7c: Please add the statistical comparison here, such as reporting the R2. It looks like theory is underpredicting the medium to larger sizes a bit. I would have thought it should be better at the larger sizes based on the assumptions, which focuses on capturing the behavior in the larger size tail (if I understood correctly). This is particularly true for the merged case. Is there a reason we would expect this disagreement?

R^2 added. In figure 7c, the theory is overpredicting L_2 for large values of $\mathcal{D}_0 L_0$. For a given size distribution, we indeed expect the model to better match observations for large lengths. However, here, it is not the same comparison : each dot on the figure corresponds to different parameters. And in the computation of L_2, we have used an approximation based on equation S73. This equation is a strong constraint for small values of $\beta\mathcal{D}_0 L_0$ (see eq S74) and so we expect the theory to work better in this case. This corresponds to the situation when L_2 is relatively small and $L_2 \approx L_0$. On the contrary, when $\beta\mathcal{D}_0 L_0$ is very large, equation S73 is a very loose inequality and so does not constrain L_2 well. So it is not surprising that we observe a worse agreement with the numerical statistical model in that case. Solving analytically the exact equations for L_2 would be great to alleviate this problem, but we were not successful in this.

Line 293-294: Please add R2 to know how much variance is explained with this linear proxy.

Done.

Line 296: Please report the R2 for this correlation

We did not report it because since the relationship is non monotonic, we would need to add four R2 values (for L, for B, for weak merging, for large merging), with the risk of distracting the reader.

Figure 8: Please add R2 for all these plots. Is the dashed line in d the best fit line? Otherwise, it looks like the dashed lines are 1:1? Are the error bars here and throughout (e.g., Figure 11) for a single standard error (or deviation), so 68% confidence?

Done.

Figure 9, Lines 308-321: It would be helpful to provide a little more detail in how to interpret this figure. It looks like the caption is inconsistent with what is being shown as well, please clarify. Specifically, for 9a, LTH1 and LTH2 are mentioned but not shown and the colors are green and brown in the caption but gray in the figure.

Sorry for the confusion with the colors (corrected). Regarding the caption of panel (a): the mention of L_1^{TH} and L_2^{TH} was just a reminder of how L_0^{TH} was computed, but since it was misleading, we don't mention it anymore. Figure 9b has been simplified: we indicate more clearly (with a number) whether a flight was associated with one or two cloud populations, and we don't mention the range of L_1^{TH} and L_2^{TH} in the subcloud layer (we show it only at cloud base). Finally, we added a Figure to the SI (Fig. S4) showing more explicitly the relationship between L_0^{CLD} and L_1^{TH} in the presence of a single cloud population, and between L_0^{CLD} and L_2^{TH} in the presence of two cloud populations. The caption has been rewritten so as to clarify the interpretation of the figure.

Line 344-346: It's exciting to have two observations of the mass flux to compare here. Would it be possible to show the scatter between Mb estimates from the two different methods in the appendix? Or at least report the R2? Both would be preferable if doable.

A detailed comparison between the two Mb estimates is available in the supplementary material of Vogel et al. (2022, Fig S2d). We added the R^2 values for each relationship.

Figures 10 and 13: These are very helpful and elegant diagrams, please make sure to reference in the text somewhere and thank you for including.

Sorry about that. Done.

Figure 11b: why are some of these scatter points in gray?

We added to the caption: grey markers correspond to the flights whose mixed layer depth suggests that they were influenced by cold pools.

Line 346-348: Are you inferring this from comparing 11a to b? Might be helpful to color the points by Mb in 11b or something so it is more explicit.

We prefer keeping the figure as it is for the sake of homogeneity with the other panels. Since M_b and W_b are shown side by side, and the flight numbers are indicated on each figure, the reader can easily check the correspondance between M_b and W_b.

Line 348-350: Are you referring to the weak positive relationship here? Would be helpful to add the R2

Given the small sampling size (18 flights at most) and the many factors that can affect the correlation between quantities derived from observations from two different aircraft (with different flight patterns) and from different days (associated with a lot of meteorological variability), this figure intends to show relationships that are more qualitative than quantitative. Therefore, we feel that reporting R^2 values is unnecessary and potentially misleading for this figure.

Line 356-357, 11b,d: Please consider adding the R2 here as there is a fair amount of variability in this relationship.

Same response.

Line 363: Also, Janssens et al. 2024?

Added.

Line 368-369: This is quite an interesting hypothesis; I hope you pursue this idea further.

Thank you.

Line 390: Please indicate which panel in Figure 11 you are highlighting here.

Done.

Line 430-436: This is very interesting. Do you think that the merging helps to transition the organization types? i.e. from something short lived, like Sugar and Gravel, to something long lived like Flowers? This is what happens in Narenpitak et al. 2021 and you discussed seeing something similar between flights 15 and 16. Might be worth mentioning the Lagrangian evolution implications somewhere (if not already in the discussion).

Yes, it is exactly what we think, and it is discussed in the Summary section.

Figure 14b: Please report R2 to be consistent.

Done.

Appendix A

Line 606-607: Is this consistent with the discussion in the main text of having the thermals merging closer to the cloud base and not at the surface? This would seem to merge at the base but still have separate updrafts aloft.

Our model should be valid when thermals are extremely coherent structures with $T_{life} \gg T_{transit}$. This is a simplification especially for thermals, for which T_{merge} and T_{life} are in general of the order of $T_{transit}$. Therefore, as the thermals approach each other they probably also rise and may even detach from the ground, which is not accounted for in our model. As a result we expect to find more merged thermals higher in the subcloud layer. Also, thermals grow by entrainment which we have neglected in our model and can lead to more merging in the upper layers. So to summarize, this mathematical model is a very simplified view, which probably deserves some further investigation.

Line 627-628: I don't understand what this is saying, please clarify

We noticed that there was an error in the original manuscript (the distance that needs to be considered is between the centers of the updraft and not only between the edges); the correction results in a slightly different expression for β (the 1/2 factor disappears in the different equations defining β), and in a different γ value in Figure 14b (3/2 instead of 3), but it does not change anything else in the manuscript, nor in the supplement, because to estimate β from observations we use the coverage fraction (eq 11).

We also hope the revised text clarifies the calculation: "In other words, taking into account the influence of the thermal-induced circulations on merging amounts to replace the actual updrafts by effective objects whose size is the actual size of the updrafts multipled by β."

Supplemental Material

S11: Please explain why this is the “actual” coverage?

We added: ”By definition, the size of effective updrafts is larger than their actual size by a factor of β. The actual coverage fraction is thus obtained by dividing the coverage fraction of effective updrafts by β.”

Section S2.1: I think I am missing the definition of P0 and P0eff that are used in these equations. I couldn’t find them here or in the main text, apologies. Would be very helpful to have explicitly stated somewhere to follow the substitutions made later in the calculations.

We added: ”We will note $\mathbb{P}_0^{eff}$ the associated probability function for the exponential population of effective updrafts before merging: $\mathbb{P}_0^{eff}(x) = \mathcal{D}_0 \mathbb{S}_0(x)$”.

S13: Please explain how Pmerging becomes P0eff?

We now write: ”Now, we compose this equation by the logarithm function. We use the fact that $\mathrm{d}y$ and $\mathrm{d}z$ are infinitely small, as well as the properties of the logarithm function, to transform the left member as follows:

$$\ln(1 - \mathbb{P}_{merging}(x, y)\mathrm{d}y) = -\mathbb{P}_{merging}(x, y)\mathrm{d}y \tag{1}$$

and the right member as follows :

$$\ln\left(\prod_{z=-y}^{x} (1 - \mathbb{P}_0^{eff}(y)\mathrm{d}y\mathrm{d}z)\right) = \sum_{\text{all}\,\mathrm{d}y\,\mathrm{d}z} \ln(1 - \mathbb{P}_0^{eff}(y)\mathrm{d}y\mathrm{d}z) = -\int_{z=-y}^{x} \mathbb{P}_0^{eff}(y)\mathrm{d}y\mathrm{d}z \tag{2}$$

We therefore obtain the following equation:...

S16-20: Please share a little more detail in how you do this. I think it is the same strategy you apply later as well, right?

The development added as an answer to the previous question probably has already clarified how the product transforms into an integral. We also added a step between the (original) equations S16 and S17 to make the computation even clearer ($= \int_0^\infty -\mathbb{P}_{merging}(x, y)\mathrm{d}y$).

S22-S24: I don’t follow how S22 collapses into 23 and 24, please share a little more detail

We have now added an equation after the (former) equation S22 making it clear that the result is an exponential size distribution: $= \frac{\beta \mathcal{D}_0}{L_0} e^{-\beta \mathcal{D}_0 L_0} e^{-\left(\mathcal{D}_0 + \frac{1}{\beta L_0}\right)x}$ and have clarified the derivation of the following equations.

Above S25, S65: It would be very helpful to discuss how you used homothety here and potentially include a diagram. I am not familiar with this technique, and it seems crucial for this calculation and how you connect it to the framework used for the observational analysis.

The homothetic transformation of a size distribution is now more detailed (between the former equations S24 and S25, which now correspond to S28 and S30).

S27: Please clarify how you derive the density.

We added: "By comparing with the expression of $\mathbb{P}_1^{eff}$ we also obtain the density of non merged updrafts" (just before equation S32, which was formally numbered S27).

S28: Please say a little more about the "simple arithmetic calculations" here

We now write: "If we solve for the equation $\frac{\partial \mathcal{D}_1}{\partial(\beta \mathcal{D}_0 L_0)} = 0$ we find that..."

S41: Please clarify how you get to this, it seems like we have lost some constants.

Sorry, this equation is obviously wrong as you noted. This is only a writing mistake because the other equations are right after. It is now corrected (new eq S46-S47).

S60-62: Please clarify how you get these approximations.

We only use the approximation once between (former) equations S60 and S61 (now S66 and S67). For clarification, we haved replaced "for which $\mathcal{D}_0 x \gg 1$" by "for which $e^{\alpha \mathcal{D}_0 x} \gg 1$". Note that another typo was corrected in (former) equation S60 (now S66): $e^{-\frac{x}{L_0}}$ should have been $e^{-\frac{x}{\beta L_0}}$.

Discussion of S69: Please consider including this figure showing the simulations match the equation well, would be very valuable to have. Maybe add in an appendix for the main text?

We now write: "Moreover, this expression has been validated against numerical experiments (section 5.2 of the main manuscript) and equation S75 matches well the simulations. Proving mathematically equation S75, by relaxing the hypothesis of a constant α, remains a perspective for future work."

Typographical Comments

Main Text

Figures 4, 7d, 9a, 11a,c: The edge labels/legends have been cutoff for these.
Corrected.

Line 48: "(George et al., 2023)"
Done.

Line 69: "the HALO"
Done.

Line 101: "anomalies called thermals"
Unchanged.

Caption for Figure 2: "shown on Fig. 1c" rather than 1a?
Corrected.

Line 204 and elsewhere: suggest saying "is written as" instead of "writes" for describing these equations.
Done.

Line 256-258: Suggest splitting into two sentences.
Done.

Figure 7: colors appear different between the caption and figure: blue ¿ purple and red ¿ pink
Done.
Figure 12: consider reordering panels so they are discussed in order in the text. Also, is the dashed line in b the critical merging efficiency?
Unchanged.
Line 448: “Feb”
Corrected.

Appendix A
Line 629: “its”
Done.
Line 648: “as is the”
Done.

Supplemental Material
Above S5: “is” instead of “writes”
Done.
Above S57: only one “that”
Done.
Above S58: “updraft sizes to the”
Done.
Above S64: “to an”
Done.
Above S67: “L2 is not”
Done.
Below S67: “strong constraint”
Done.