Review of egusphere-2025-2839

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.

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.

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.

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 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?

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

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?

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.

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

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

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.

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

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).

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?)
- 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?
- Annotations a, b, c are missing.
- 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.

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?

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 sub-conclusion 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.

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?

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?

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.

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?

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?

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?

Section 7.3 (building towards conclusion 4) presents an intriguing framework diagnosing cloud fractions at both cloud base and cloud top from beta_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 beta_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.

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.

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?
- Line 625: Does this assume that the tansit/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?
- 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.
- 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.

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?

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.

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).

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?

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

Figure 4: there are no figures c and d. The caption and line 161 refer to 4c-d.
Figure 4a: it seems like p2 should be bigger than 0.1
Figure 4b: Are the annotated proportions correct? There does not appear to be a the second population.

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 25-106 for the second cloud population.

Line 251: "pre-specified value". And line 253: "pre specifeid value f_TH". It would be nice if the pre-specified value would be defined when first mentioning it.

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

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

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.

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

Spelling mistakes:
a*n* updraft instead of a updraft
48. George et al. (2023) should be placed in brackets.
101. Ascending anomalies *which* are called thermals
119. No brackets around Lenschow and Stephens (1980)
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.
284: Nevertheless instead of Neverthess.
448: Feb instead of Fev.
601 and 644: Figure instead of figure.

Supplement: homothety instead of homothethy.
Supplement: a*n* updraft