Reply to Reviewer #2

We thank the reviewer for providing comments on our manuscript. Following your and the first reviewer's comments, we have revised the manuscript significantly. The introduction has been rewritten completely to provide a more focused and clearer storyline. The results and discussion sections have been separated to improve clarity, providing detailed characterisation in the results section, while presenting respective discussion with more elaborate explanations in the discussion section. Below, we provide answers to the reviewer's comments, with the original comments in red, our clarifications and answers in black, and newly added text in blue. Any reference to lines in our answers is given with respect to the original manuscript (not the revised version).

[…general comment by the reviewer…] The authors argue that strong downdrafts are more common in the case of a supersaturated state (RHi > 110%), and not in a subsaturated state (RHi < 90%), which is expected because a subsaturated state would require more energy (sublimation) and, as a consequence, more cooling.

Moreover, the more intense downdrafts in a supersaturated state are related to high values of droplet number concentration. Strong downdrafts between 5 and 10 km also tend to be related to high concentrations of particles with sizes less than 100 um. The analysis presented by the authors shows what they state, but I find the whole manuscript too simple in the analysis and interpretation of the results. It is difficult to get the idea the authors want to transmit.

Do they want to say that the way we think about downdrafts is wrong?

Or do the characteristics of downdrafts depend on the region or the sampling method? The dynamic of the manuscript is to present a result a then compare it to the other studies. If the comparison goes in the same direction, the authors state that the results are in accordance with the literature. When it is not the case, the authors give different hypotheses to explain the discrepancy, but there is no analysis supporting their hypotheses. This way of interpretation gives the manuscript a direction of too speculative.

We thank the Anonymous reviewer #2 for the detailed remark on the manuscript as a whole. We have now restructured the manuscript in order to improve the clarity of the analysis and, at the same time, bring more substance and explanations to our discussion. We have split the results and discussion into two self-standing sections. This provides a self-contained results section that offers a thorough analysis of the unique observations, including their statistics. The discussion section is used to elaborate on similarities and differences we have found in comparison to previous studies.

Before addressing the specific comments, we would like to clarify that we want to point out the existence of supersaturation in downdrafts. However, we do not want to argue that the downdrafts are more common based on the state they are in (subsaturated vs supersaturated).

Major comments:

In the following lines, I give my major concerns about the manuscript:

1. The objective of the paper is difficult to the asses. The introduction, which in my opinion is unnecessarily long, does not provide the problem that the authors aim to tackle. The motivation presented by the authors is that there is not enough literature about observations of upper-level drafts. […] the object of sampling is restricted to cloud decks around convective systems, avoiding the convective core. This points out that the sampling region could also play a role in the results of the manuscript. I would suggest stating clearly in the introduction what is the scientific questions that the authors are tackling in the manuscript […]

   Thank you for pointing out the difficulty in assessing the objective of this study. The objective of this study is to gain a deeper understanding of the characteristics of deep convective cloud anvils, which cover large areas and thus constitute a significant part of the convective system. To achieve this, we use rare in situ aircraft observations from high altitudes (up to 14 km) during the ACRIDICON-CHUVA campaign over the Amazon. In particular, we provide details on the dynamical, thermodynamic, and microphysical properties of updrafts and downdrafts, which will help to inform numerical model simulations. As previous studies (including modelling) mostly focus on the core region of deep convection, more knowledge about the properties in the anvil parts of the clouds is needed. We will include these objectives clearly in the introduction to enhance the clarity.

   […]Are they stating that not all the downdraft shows the same properties? I also suggest stating clearly what the limitations of their study are. Regarding the limitations of the study, I was wondering if the results would change if upper-level downdrafts inside convective cores were included.

   The inhomogeneities in downdraft characteristics are one of the takeaways of our study. For example, some downdrafts appear in supersaturated regions, and others can be completely subsaturated. Particle size distributions in updrafts and downdrafts show similar characteristics in upper levels. These findings help us to enhance our understanding of draft characteristics and can be used to evaluate model performance.

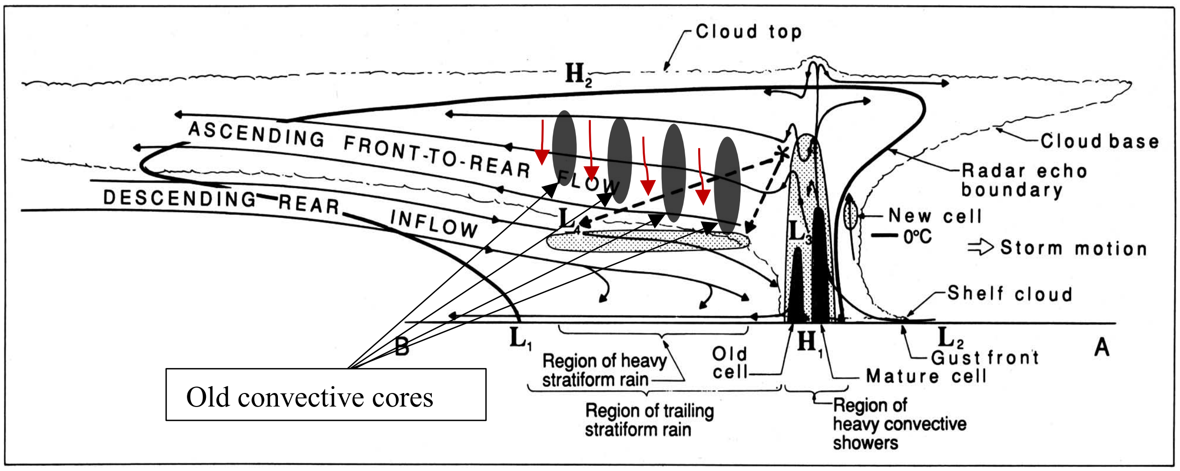
   Certainly, the sampling of different parts of clouds can affect the observed characteristics. For example, the convective cores have stronger updrafts, resulting in higher updraft and downdraft mass fluxes (Wang et al., 2020). Additionally, stronger updrafts can carry larger hydrometeors aloft, and thus, they can affect the particle size distribution.

2. The results section is difficult to read because it is arduous to get a continuous storyline. The authors explain their results with respect to other studies, looking only for similarities or differences, but there is a lack of deepening the analysis to explain the possible hypotheses stated by the authors. If the authors want to compare their results, I suggest opening a Discussion section.

   We thank the reviewer for the suggestion. While we previously thought it might be easier to keep descriptive results of particular aspects and the respective discussion together, we have now come to the conclusion that it is indeed better to separate results and discussion completely, and split the sections up accordingly.

3. In the same direction as point 2, the authors stated that neither the loading nor the evaporative cooling can explain the downdraft velocity. If this is the case, what is the hypothesis that the authors propose, and can they prove it? The manuscript will tremendously benefit from more analysis to prove or disprove those hypotheses.

   Indeed, the vertical velocity depends on several factors that can act simultaneously, such as the amount and the size/weight of hydrometeors, relative humidity, temperature, etc. Previous studies (Jorgensen and LeMone, 1989; Kamburova and Ludlam, 1966; Knupp and Cotton, 1985; Lucas et al., 1994) have identified condensate loading, related to the drag force of the hydrometeors, and evaporative cooling, relying on the downdrafts being subsaturated, as driving factors of the downdrafts. Our first hypothesis was that these drivers would also act in the upper cloud parts. However, our data shows that a significant amount of downdrafts (250 downdrafts) are in fact saturated or even supersaturated with respect to ice; thus, evaporative cooling (or, in ice clouds, sublimative cooling) cannot be the force driving or maintaining the downdrafts. To prove or disprove the condensate loading hypothesis, we looked at the relation between cloud water content, as a representative of condensate loading, versus vertical wind. Also, here, we cannot find a solid relation. This could be due to the fact that in ice clouds, particularly the larger particles are typically not spherical but have very complex shapes, which act very differently than spherical particles in terms of fall velocity. Thus, the effect of the drag force on the particles is significantly different. As the hypothesised drivers were identified in studies looking at warm clouds, we can say that our data indicates that these drivers are not playing (a major) role in ice cloud anvils. To understand what might drive/maintain the downdrafts in the deep convective cloud anvils, we use the schematic drawn by Houze et al. (1989), which in the original version only shows only one older cell, but more older cells follow successively behind. We add these in the original figure below (as Houze et al. state, even though the older cell is weakening, it is characterised by an updraft core, followed by a convective-scale downdraft, depicted here by red arrows). While the older cells seem to be maintained by "inertia", we postulate that the downdrafts between the old updraft cores are basically maintained by the existence of the older updrafts (air goes up, must come down), thus, in a way, also maintained by inertia. We include this discussion in the revised manuscript. With our airborne observations that consist of point measurements, we can, unfortunately, not prove this. However, we would like to employ a Large Eddy Simulation model (MicroHH) in a future study to look more closely at the structures of the anvil up- and downdrafts, investigating the "classical" driving forces (condensate loading and evaporative/sublimative cooling), the role of the older updrafts, and also the saturation sate of the downdrafts.



Adapted from Houze et al. (1989): Conceptual model of a squall line with a trailing stratiform area viewed in a vertical cross section oriented perpendicular to the convective line (i.e., parallel to its motion). Older convective cores are indicated by black ellipses and the associated downdrafts in red arrows.

4. I had difficulties understanding the mass flux discussion. The authors find that upper-level drafts have less mass flux than lower-level drafts. The discussion in the paper suggests that the vertical velocity is the one explaining the decrease in the mass flux with altitude. So, I was wondering if the authors expected that the mass flux of upper-level and lower-level drafts would be equal. If this is the case, please state this clearly in the manuscript.

Mass flux in clouds depends on air density, draft width, and vertical velocity. The vertical profile of draft width shows an increasing trend with altitude (Figure 1b) which would lead to an increase in mass flux. However, the mean mass flux decreases with altitude (Figure 2b), despite the linear relationship between mass flux and draft width (Figure 3). The reason for this is the decrease in mean vertical velocity and air density with altitude.

To clarify, the mass flux profile presented in this study is not entirely dependent on vertical velocity variations. It is also not expected to have similar mass flux values in higher altitudes and lower altitudes as the measured vertical velocities differ at different altitudes.

In order to make it clear, we have added in the line 195 :

The majority of the drafts at higher altitudes show very weak mean vertical velocity and low air density. So, despite the fact that they are wider, the overall mass flux values are *lower. Drafts in lower altitudes correspond to less width but relatively high mean vertical velocity values and higher air density, resulting in higher mass flux.*

Moreover, assuming again that the mass flux should be conserved, are the changes of mass flux and velocity related to entrainment and detrainment? If it is not expected that the mass flux in upper-level and low-level drafts will be equal, what is the reason for the comparison?

We have not come across studies that specifically discuss mass flux conservation using aircraft data. Could you provide more clarification on the assumption of mass flux conservation and its context?

The comparison of the upper and lower levels aims to highlight the effect on average width and intensity of drafts on mass flux values at different altitudes. As previous studies have primarily focused on lower levels, we complement their work by extending our analysis to the upper levels. A key objective here is to establish the characteristics of the upper-level drafts, which remain unknown to date.

To make it clear, we have amended the following in the line 199:

This discussion shows that upper-level drafts are wider in anvil clouds compared to the lower-level drafts observed during the campaign. It also provides an overview of the differences in draft characteristics between higher and lower altitudes, noting that previous studies mainly focused on lower altitudes.

5. The conclusion summarizes the main points of the manuscript, and in the actual structure and storyline of the manuscript, it gives a feel that the document is most like a collection of different analyses rather than addressing a scientific question. I would suggest addressing points 1 to 4 and changing the conclusion section depending on the outcome. Moreover, I find it hard to imagine large eddies communicating dropplets between downdrafts and updrafts without affecting their entropy.

   We have revised the conclusion section to enhance clarity and align it with the scientific question. To avoid the ambiguity regarding the eddy sizes, we have removed the word “large” from the manuscript. Additionally, regarding the comments made by reviewer #1, we clarified the concept of direct mixing between updrafts and downdrafts. The proximity of updrafts and downdrafts is shown to have some effect on properties such as PSDs and RH_{ice}. Please refer to the figure below, which shows two instances where the PSDs in the updraft-downdraft structure are remarkably similar and exhibit higher number concentration than the region which is not part of the draft. For more detailed information, we would like to refer to our answer to Comment 3 of Reviewer #1.

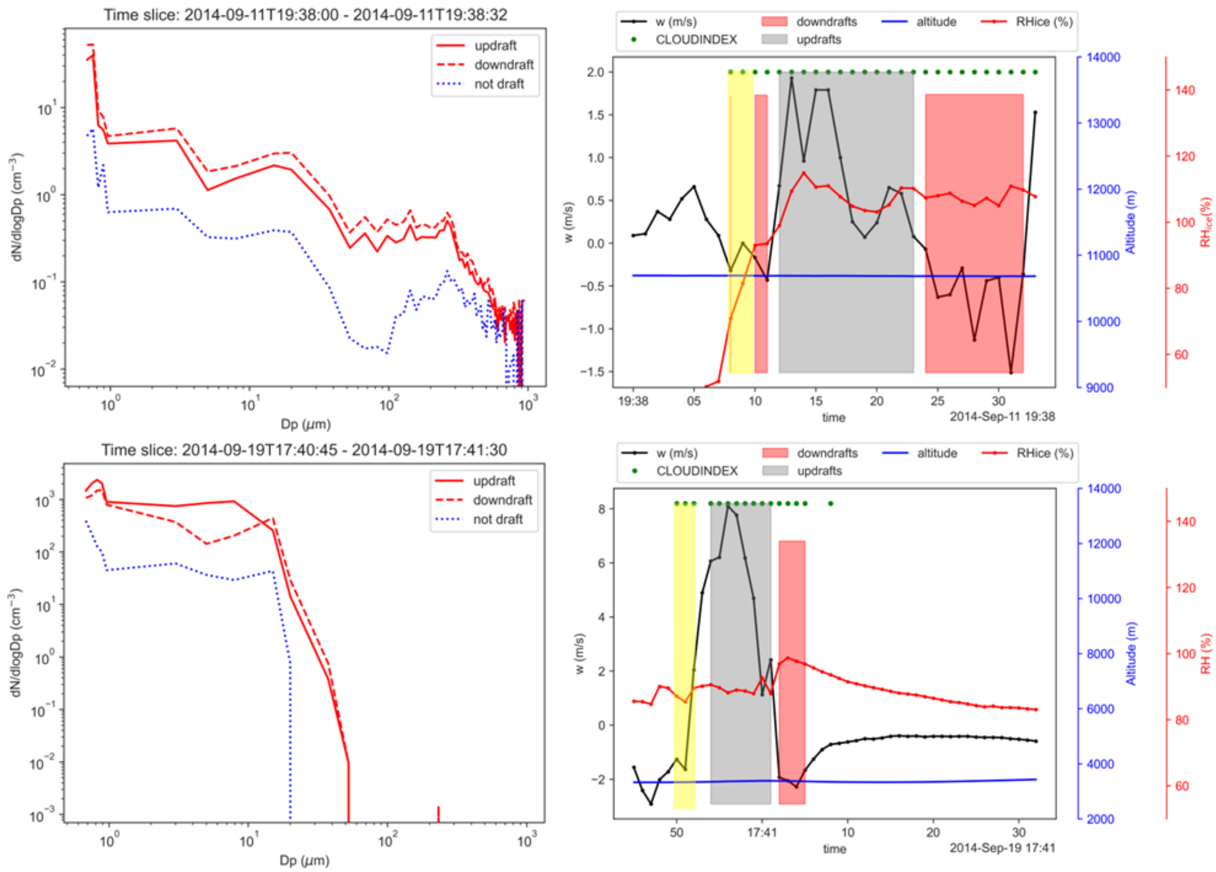


Figure R1: Particle Size distribution and time series of w, altitude, and RH of the corresponding flight segments. Similarities in updraft (grey shading) and downdraft (red shading) PSDs are visible, while those from outside the draft structure (yellow shading on the right) exhibit lower number concentrations.

Specific comments:

Line 11: What is Dp? And is it 100 um?

Dp represents the diameter of the cloud particle with the unit in micrometres (corrected in the revised manuscript).

Lines 11-12: Increases faster than what?

Faster than the weaker drafts. We have rephrased the sentence to

Furthermore, the number concentration of larger particles (Dp > 100 um) increases faster in stronger drafts than that in weaker drafts as altitude increases.

Lines 32-33: "However, the earlier observations…" I agree that this is part of the region analyzed in the manuscript, but as the authors stated, they only analyzed cloud decks.

The introduction has been rewritten completely, and therefore, this statement has been revised.

Lines 101-103: Are you studying the whole spectrum of downdrafts or just a subset of this? What about drafts in convective cores?

The study focuses on ACRIDICON-CHUVA campaign data and downdrafts encountered within. Major part of the data is sampled from higher altitudes which this study utilizes to study the draft characteristics. Since convective cores are not sampled in this campaign, it is not discussed in this study.

In the revised version of the manuscript, specifically in the introduction and discussion, we make this more clear.

Lines 176-178: How many samples do you have in the lower troposphere compared to the upper troposphere?

There is a total of 19722 data points after filtering. In which 16108 data points are from above 10km, and 3614 data points are from below 10km. In terms of drafts, 1478 drafts are from above 10 km, and 557 drafts are from below 10 km.

We include this information in the results section of the revised manuscript.

How much does the PDF of the draft diameter vary when the same number of samples is chosen randomly in the lower and upper troposphere?

We agree that there could be concerns regarding the uncertainty arising from the sample sizes. We have now included the 90% confidence interval estimate for mean and percentile curves in panel b of Figure 1 and 2. Additionally, we have modified the number of drafts in Figure 1b to draft fraction, as suggested by the editor. In a general sense, the uncertainty in the mean is smaller than the percentile curve. We provide this information in the revised manuscript.

Lines 196-197: "Drafts in lower altitudes…" I see your point, but I was wondering whether the decrease in density with altitude also explains the difference in the mass flux between upper-level and lower-level drafts.

We agree with the reviewer that the vertical profile of density would definitely affect the calculations of mass flux values. Please see our answer to comment 4.

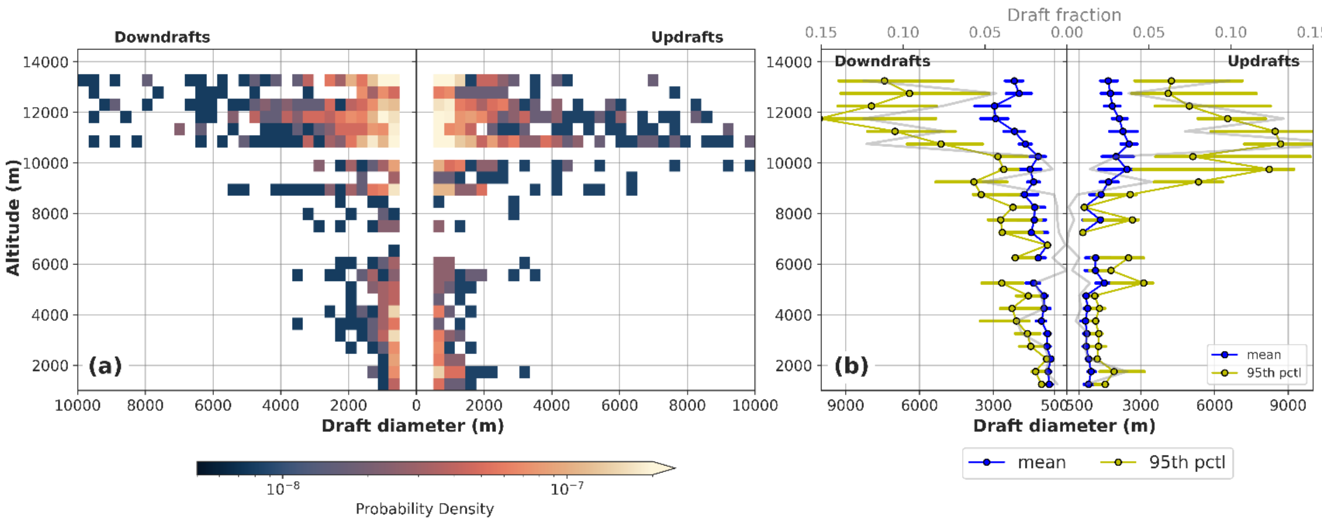


Figure 1 (revised) : Altitude-wise draft diameter statistics of all in-cloud drafts. (a) Joint Probability Density Function of Draft diameter and altitude (b) mean (blue) and 95th percentile (yellow) values of diameter of drafs. 90 Confidence intervals for mean and percentile curves are indicated by the error bars.

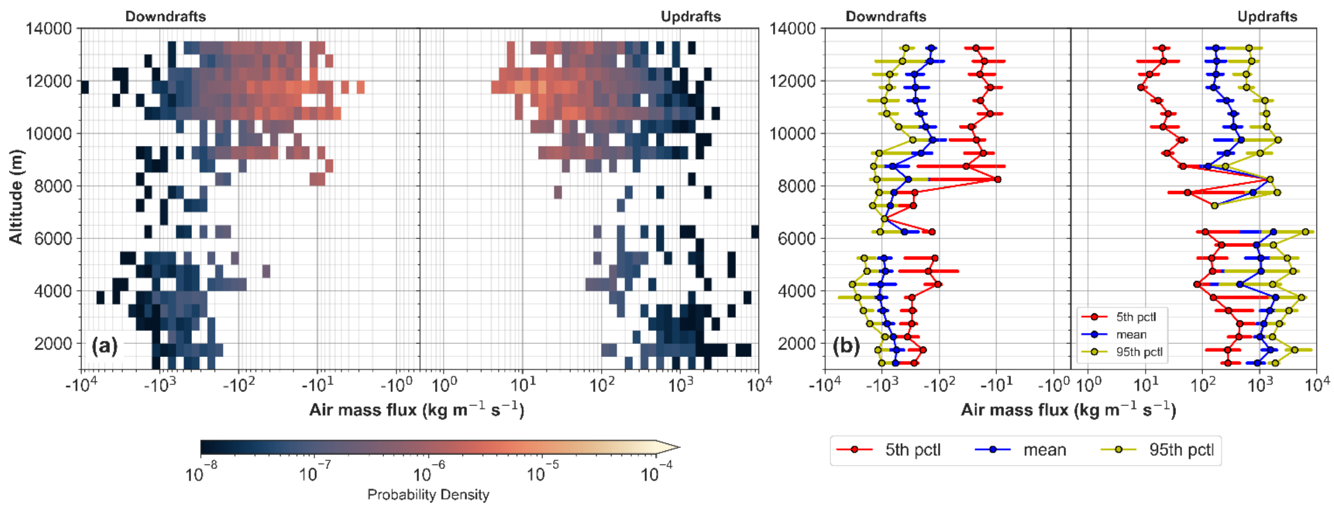


Figure 2 (revised) : Altitude wise air mass flux statistics of all in-cloud drafts (a) Joint Probability Density Function of Air mass flux and altitude (b) mean (blue), 5th percentile (red) and 95th percentile (yellow) of air mass flux. Confidence intervals for mean and percentile curves are indicated by the error bars.

Lines 212-215: What is the point of comparing if this is a different method?

Thank you for the suggestion. We remove these lines in the revised manuscript.

Lines 219-220: Does it mean that the other 70% come from wider drafts?

Yes, the other 70% mass flux is contributed by drafts wider than 500m.

We added:

Thus, the main contribution to up- and downdraft mass flux stems from the wider drafts (width > 500 m).

Lines 226-228: "We would like to ..." How do you think that the sampling method affected the results?

Sampling at different locations of cloud (e.g., convective core) would affect the statistics and particle size distributions. Please see the answer to comment 1 for more detail.

Figure 4: If I understood correctly, every point is a draft. So, what about dividing the cloud water content by the draft diameter? This is to see if the concentration of cloud water content with respect to the diameter shows a relationship with vertical velocity.

Thank you for this suggestion. Unfortunately, we could not observe a conclusive relationship between CWC divided by draft width and vertical velocity (Figure R3). We would also like to clarify that each point in Figure 4 is an individual measurement, as calculating a representative value for each draft could lead to ignoring the fluctuations. This way, we can observe the actual value of the CWC.

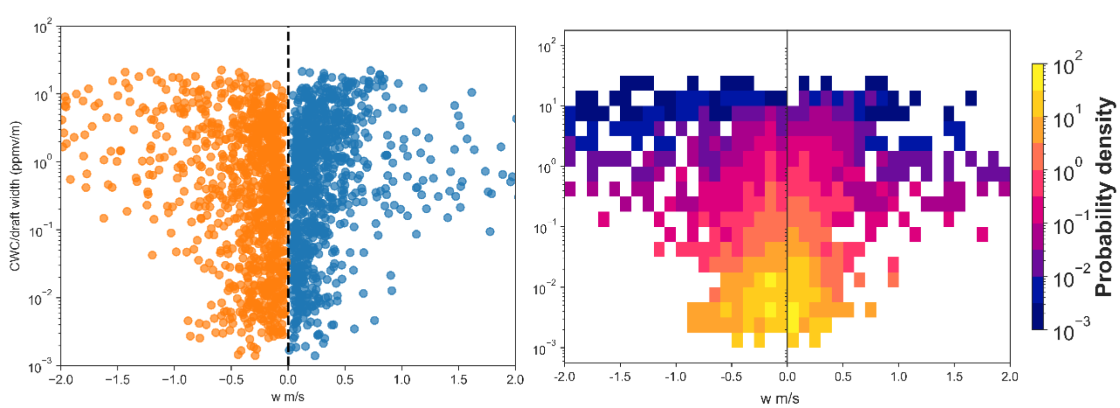


Figure R3: (left) Vertical velocity vs Cloud Water Content (CWC) / draft width in updrafts (blue) and downdrafts (orange) points. (right) Joint Probability Density Function of vertical velocity and CWC/draft width.

Lines 236-237: I did not understand the logic of the two sentences. First, atmospheric motion is influenced by hydrometeors, but in the following sentence, which is an example, updrafts influence supersaturation. So, are hydrometeors affecting atmospheric motion or the other way around?

We thank the reviewer for pointing out the ambiguity in the sentence. We remove the first part of the sentence and start with

The updrafts influence the supersaturation, thus the condensation process….

Lines 255-256: "We observe stronger downdrafts…" Are you sure? What I see is that the subsaturated state has equally strong downward vertical velocity as the supersaturated downdraft (< -1 m s-1).

The sentence in the manuscript reads "We observe strong downdrafts in the supersaturated regime where, due to the supersaturation, sublimation is not possible."(not "stronger downdrafts"). This indicates the measurements with w < -1 m/s.

Lines 273-275: How confident are you that this method removes the influence of mesoscale draft in the vertical velocity?

This method is only to examine the regions in drafts where |w| > 1 m/s and not eliminate the mesoscale drafts. Due to the large length scale, these drafts have prolonged regions of very weak vertical velocities (|w| < 1 m/s) and large fluctuations in variables which introduce uncertainty, along with regions of significant vertical velocity (|w| > 1 m/s). Elimination of weaker vertical velocity points enables us to focus on the more active part of the drafts.

In line 275, we rephrased "higher intensity drafts" to "*more active parts of the drafts*".

Line 302: "Larger particles…", what does it mean larger particles? Dp >100 um , or are you talking about particles close to 100 um.

The term "larger particles" represent the particles with diameter (Dp) > 100 um. We added this information to improve clarity.

References

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