Response to the Reviewer #1

We really appreciate your time and effort taken to review our manuscript again. We have revised our manuscript according to your comments as explained below. Please see the following for our point-by-point responses shown in red just below the corresponding comments shown in black.

The authors have successfully addressed the majority of my comments. The paper now clarify much better what the CPR can do in convection.
I have one additional comment: it seems that the authors have excluded MS-contaminated profiles (if i understand correctly they do not go into the EU class). Well since MS contaminated are certainly deep convective signatures it would be good to know what is their fraction compared to the other classes. In principle I would use them also as an additional term in the denominator for computing fractions. This is important because we need to know in which fraction of convection CPR can actually quantify something and in which it cannot.

We thank the reviewer for this suggestion. We agree that multiple-scattering (MS)-contaminated profiles provide valuable information, and that the reader should be able to assess what fraction of the selected convective sample can be quantified by CPR Doppler measurements. We have therefore clarified the treatment of MS-contaminated gates/columns in the Doppler-velocity quality-control section and in the definition of the occurrence ratios.

The MS screen is applied at the range-gate and averaging-window level, rather than as a rejection of the entire profile. Thus, when MS-contaminated lower gates are screened out but valid MS-free subfreezing gates remain in the upper part of the same column, those remaining gates are retained and the column can still be assigned to NonSU, SU, or EU according to the same criteria. In our sample, the MS screen removes 1.9% of otherwise valid subfreezing gates.

We did not include MS-limited cases as an additional term in the denominator of the SU and EU ratios, because the MS screen is applied at the range-bin level and is a data-availability/quality-control limitation rather than a mutually exclusive convective-intensity category.

Modified/Added text:

Line 168: To mitigate ε_MS, we follow Battaglia et al. (2011) and exclude range gates for which the cumulative reflectivity from the top of the atmosphere down to that gate exceeds a prescribed threshold. This MS screen is applied at the range-gate level rather than a rejection of the entire profile; therefore, MS-free subfreezing gates that remain in the same column are retained for Doppler-velocity statistics. This MS screen removes 1.9% of the otherwise valid subfreezing gates.

Line 232: The total count denotes the number of columns for which Doppler-based classification remains possible after the quality-control procedures, including the multiple-scattering screen.

Minor comments:

1) The text could benefit from some sharpening and shortening (I found it repetitive in some parts)

We agree and have revised the manuscript to sharpen and shorten the presentation. In particular, we reduced repetitive wording in the regional-analysis discussion (Sect. 3.2) and the Summary and Discussion (Sect. 4) , and we rephrased several paragraphs to emphasize the central message.

2) It would be good to point out in the conclusions/abstract what is the fraction of tropical deep clouds that are EU and SU (just to let the reader understand in which fraction of the cases we do have a quantification of the updraft strengths)

Thank you for this suggestion. We have added explicit statements of the SU and EU fractions in both the Abstract and the Summary and Discussion.

Added text:

Line 14: SU and EU columns account for 4.88 % and 7.42 % of the Doppler-classifiable convective sample, respectively.

Revised text:

Line 597: Using the common denominator of all the selected convective columns, SU columns account for 4.88% of the tropical sample, whereas EU columns account for 7.42%.

3) Line 74 . They are (unfortunately) not simple snapshots of w (but of w+Z-weighted sedimentation velocity)

We agree. The revised text now states that surface-based and airborne radars provide snapshots of hydrometeor Doppler velocities, from which air vertical velocity can be inferred only with additional assumptions.

Revised text:

Line 36: Surface-based and airborne radars have provided invaluable snapshots of hydrometeor Doppler velocities, from which air vertical velocity w can be inferred with assumptions (Heymsfield et al., 2010; Collis et al., 2013; Giangrande et al., 2016; Schiro and Neelin 2018; Schumacher et al., 2015), but these measurements are episodic, geographically restricted, and heavily concentrated over land.

4) Line 111: 240 is not the vertical resolution of Cloudsat but its sampling!

Thank you for catching this. We corrected the CloudSat description. The revised manuscript now states that CloudSat/CPR has a horizontal footprint of approximately 1 km and a vertical sampling of 240 m, rather than describing 240 m as the vertical resolution.

Revised text:

Line 73: The A-Train constellation further advanced vertical profiling through CloudSat's 94 GHz Cloud Profiling Radar (CPR), which is sensitive to clouds and light-to-moderate precipitation and resolves the vertical structure of clouds with horizontal footprint of approximately 1 km and vertical sampling of 240 m, respectively (Stephens et al., 2002; Tanelli et al., 2008; Stephens et al., 2008).

5) Line 191: at this stage I would have extended the analysis to the full 2025 year

We followed the reviewer's suggestion and extended the analysis period from January–September 2025 to January–December 2025. The corresponding statistics, figures, tables, and related numerical values were updated consistently. The extension increased the sample size but did not alter the qualitative results.

6)Line 220: going to large Z does not necessarily reduce e_NUBF. You need to exclude regions of strong along-track reflectivity gradients but I would not do that because this would exclude a lot of convective regions.

We agree with the reviewer. We revised Sect. 2.3 to clarify that the reflectivity threshold is used primarily to reduce low-SNR random errors, not to guarantee removal of non-uniform beam-filling effects. We also added text noting that residual ε_NUBF may remain in convective cores with strong along-track reflectivity gradients. As the reviewer suggested, applying a gradient-based filter would remove a large fraction of convective scenes, so we chose not to apply such a filter and now state this limitation explicitly.

7) Line 458: dBZ==> dB (a difference of dBZ is dB not dBZ)

Corrected. In the CFAD caption, we changed the reflectivity-bin interval from "2 dBZ intervals" to "2 dB intervals."

8) Line 1397: I disagree on this. At the end the selection criteria for the profiles in this paper tend to exclude in my opinion many lower-topped clouds. But this can be left as mentioned afterwards to future work.

We appreciate this comment and have moderated the wording accordingly. The revised Summary and Discussion now states that our selection criteria focus on cold-topped, vertically developed convection, while the analysis samples a range of cloud-top and echo-top structures and is not limited only to the tallest echo-top cases. We also emphasize in the limitations that the temperature and cloud-selection criteria exclude some lower-topped or warmer cloud systems, and that broader sampling of such clouds should be addressed in future work.

9) Fig.

The ninth minor comment appears to have been truncated after "Fig." in the report available to us, so we could not identify the specific concern. We nevertheless rechecked all figures, legends, captions, and symbol definitions.

Response to the Reviewer #2

We really appreciate your time and effort taken to review our manuscript again. We are grateful for your positive evaluation and for pointing out the remaining technical inconsistency in the schematic figure.

I appreciate the authors' effort to address my previous comments in the revision. The paper has been well improved and is now ready for publication except for a minor technical issue that may need attention before acceptance. In Figure 12, the legend says that areas with Ze > 10 dBZ are filled with big black dots, bu the schematic plot is filled entirely with small grey dots within 0-dBZ contours without any black dot for Ze > 10 dBZ. It's a bit bizarre because black dots were drawn in the original manuscript before revision. I would suggest that the authors bring back big black dots inside the 10-dBZ contours or otherwise modify the legend so it matches the plot.

Thank you for identifying this issue. Although the black dots appeared to be visible in our working version of the revised manuscript, we recognize that the rendered version available to the reviewer may not have displayed them clearly or consistently. We have therefore regenerated the schematic figure so that the regions with Ze > 10 dBZ are filled with large black dots, and we have revised the caption to ensure that the plotted symbols and the legend are fully consistent. The caption has also been revised to clearly describe the regions corresponding to the −30, 0, and 10 dBZ thresholds. In the revised manuscript, this conceptual schematic is shown as Fig. 11.

Response to the Reviewer #3

We thank the reviewer for evaluating the revised manuscript. No additional specific suggestions for revision or reasons for rejection were provided in the review report. We have nevertheless carefully checked the manuscript and incorporated the additional technical corrections requested by the other reviewers.