

Reviewer's comments in black, replies in blue.

Comments from reviewer

This paper provides a valuable contribution since there is not a huge amount of updraft information of this type in the literature. The results are valuable for a variety of reasons such as for providing better realism for numerical models on convection vertical motion scales and magnitudes. The results are also very useful for microphysical studies such as rain/snow growth mechanisms that require a vertical motions as a key input. The authors provide a good summary of past work and the manuscript in general is well written except for numerous typographical errors and some poorly worded sentences. The technical details are sufficient for the material presented. There is too much detail in some sections, and only the key points should be included (e.g., section 4.3).

Answer:

We appreciate the reviewer's comment. Actually, the Editor had pointed out the typographical errors after we submitted the original manuscript, then we sent the manuscript out for editorial service and submitted a revised version. However, when dealing with the technical comments, we found many typographical errors do exist in the old version, but have been corrected in the revised version. Maybe the reviewers were reading the old version. The revised version can be downloaded on <http://www.atmos-chem-phys-discuss.net/acp-2015-1021/#discussion> . Nevertheless, the sciences are the same. We have addressed the comments raised by the reviewer, the sample issue and limitations of aircraft measurements have been highlighted. Sections with too much detail are simplified. A discussion section has been added to show the complicated interactions among

vertical velocity, entrainment/detrainment and microphysics. We have changed the manuscript title to “Characteristics of Vertical Air Motion in Isolated Convective Clouds” to highlight that this study deals with isolated convections rather than mesoscale convective systems.

The paper deals with what shallow to moderate convection. The authors need to add some discussion in the abstract and conclusions on the fact that the measurements presented are still a biased sample of convection. Are the measurements truly representative of all convection in the three regions presented, or did for example the planes used stay away from stronger, and/or deeper convection, or ones with higher reflectivity. What the paper points out is that there are some similarities between the regions, but that there is really a wide variety of convective types over the globe. This is a good point to make in the paper that there are few measurements of this sort so they are greatly needed, but they represent specific regions and types of convection, more from other regions are needed, and one should not interpret the results that these results can be generalized globally. A few summary sentences (abstract, intro, and conclusions) on this point would make the paper better.

Answer:

We appreciate the comment. We totally agree that this study only deals with a biased sample of convective clouds. Only three field campaigns are analyzed and MCSs were not sampled. The results cannot be generalized globally. We have pointed this out in the revised manuscript, included in abstract, introduction, datasets description and conclusion. We also changed the manuscript title to “Characteristics of Vertical Air Motion in Isolated Convective Clouds” to highlight that this study deals with isolated convections rather than mesoscale convective systems.

In addition, we have added more text to point out the limitations of aircraft measurements. First, aircraft cannot provide 3-D information of the cloud, so the air mass flux is derived from measurements in single-line penetrations. Second, aircraft might not penetrate through the strongest part of drafts due to safety issues. Moreover, in-situ measurements only provide data from single-line penetrations, but the vertical velocities are very different at different heights in a cloud. For example, many penetrations in COPE are near cloud top, while in HiCu and ICE-T there are many penetrations far below cloud top. Therefore, readers need to be aware of the limitations of aircraft measurements when using the results in this study.

While I find the paper quite interesting, there could be more connection between the convection dynamics and microphysics. Processes such as mixing are barely mentioned in the text. It would be interesting for example to make connections between the updraft characteristics such as mass fluxes, diameters, and entrainment.

Answer:

We appreciate the comment. We have tried to explore the interactions between dynamics and microphysics, but the physical processes are very complicated and there are many limitations of aircraft instruments (e.g. resolution, time response and uncertainty) and sample issues. An example is given in Fig. R1. In the figure, we plot the mean vertical velocity (a and b), normalized relative humidity (c and d), normalized FSSP concentration and normalized King LWC (e and f) as a function of normalized scale from cloud edge to location of the maximum vertical velocity in the updraft closest to the cloud edge. On the x-axis, 0 indicates the cloud edge, 1 indicates the location of maximum vertical velocity in the updraft closest to the cloud edge, where is less affected by entrainment. As shown in the figure, weaker updraft associates with

lower relative humidity, droplet concentration and LWC, and stronger updraft associates with higher relative humidity, droplet concentration and LWC. This maybe partly due to entrainment/detrainment mixing. This figure is from ICE-T only because in HiCu and COPE we do not have fast-response instrument to measure RH. The droplet concentration and LWC may have large uncertainty because FSSP often has shattering issues and King probe cannot detect large drops ($> 50\mu\text{m}$).

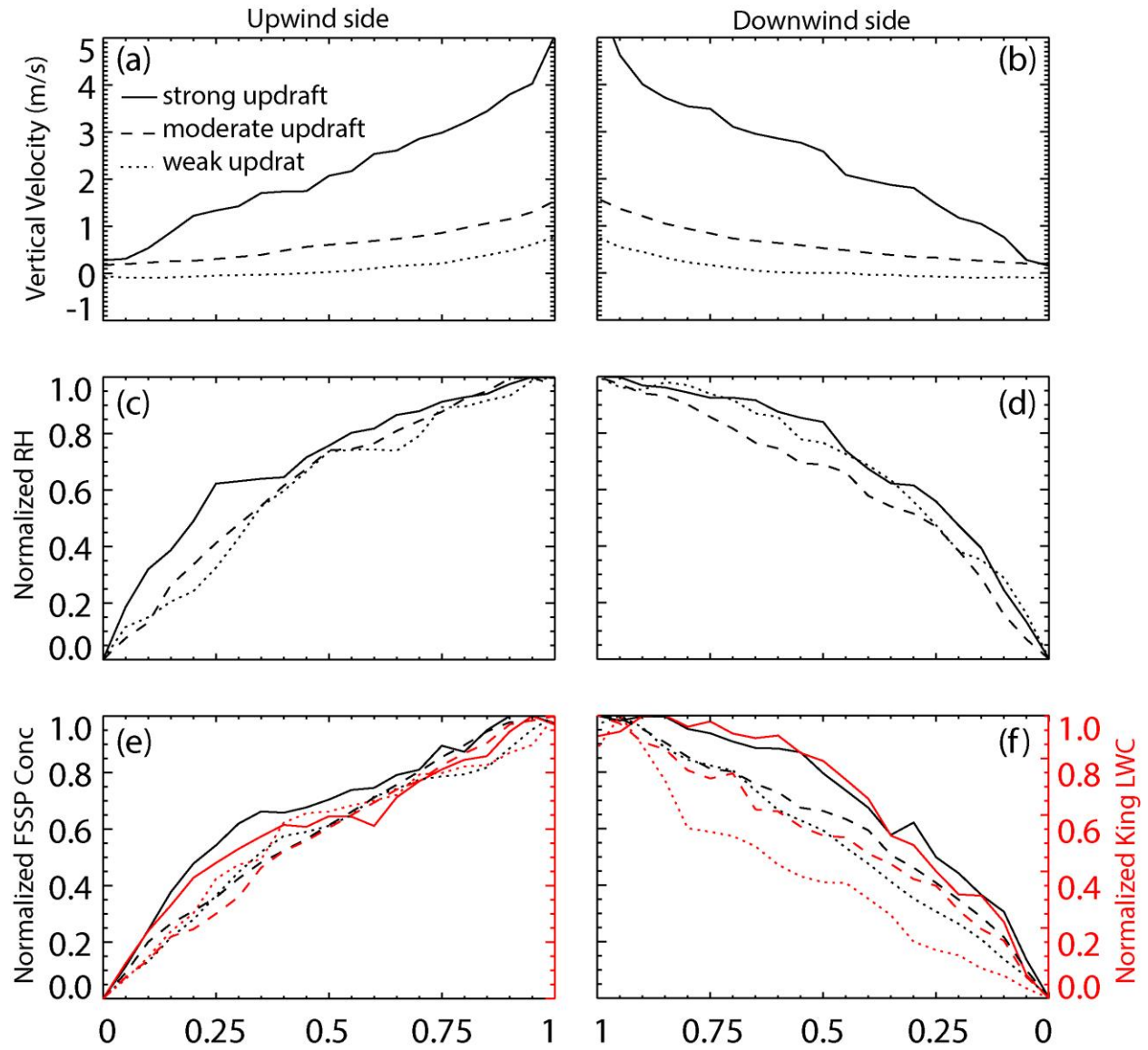


Fig. R1: Mean vertical velocity (a and b), normalized relative humidity (c and d), normalized FSSP concentration and normalized King LWC (e and f) as a function of normalized scale from cloud edge to updraft closest to the edge. 0 on the x-axis indicates the cloud edge, 1 on the x-axis indicates the location of maximum vertical velocity in the updraft closest to the cloud edge.

We also tried to use indirect ways to explore the impacts of entrainment on vertical velocity. Fig. R2 shows the PDF of vertical velocity in downdrafts near cloud edge and inside cloud. In HiCu and COPE the downdrafts near cloud edge are stronger than those inside clouds, maybe because of the strong evaporation-cooling effect induced by entrainment, while in ICE-T the downdrafts are similar near cloud edge and inside cloud. This only partly explains the stronger downdraft in HiCu and COPE than ICE-T, because the downdrafts inside clouds are also stronger in HiCu and COPE than ICE-T.

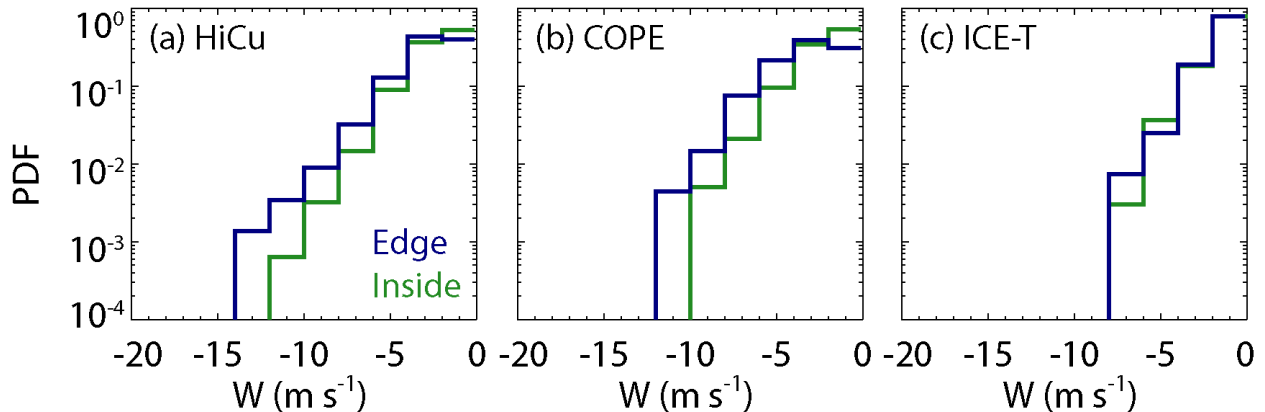


Fig. R2: PDFs of vertical velocity in downdrafts near cloud edge and inside cloud.

Due to the complexity of dynamics-microphysics interactions and the limitations of aircraft measurements, it is better to address this problem in detail in other papers. We have written a separated paper and discussed the interaction between vertical velocity and liquid-ice mass

partitioning (Yang et al. manuscript submitted to JAS), in which an algorithms is developed to partitioning liquid and ice mass using multiple in-situ instruments. An example is given in Fig. R3, the figure shows in developing cloud the LWC and IWC are higher in stronger updraft, but the liquid fraction has no obvious correlation with vertical velocity. In mature clouds, LWC is higher in stronger updrafts, but IWC is similar in weak and strong updrafts. Between -3 C and -8 C, the liquid fraction is smaller in weaker updrafts, maybe because secondary ice production (e.g. H-M process) is more significant in weaker updraft (Heymsfield and Willis 2014). Only ICE-T is used in that paper because in COPE and HiCu we do not have the appropriate instruments to provide sufficient measurements.

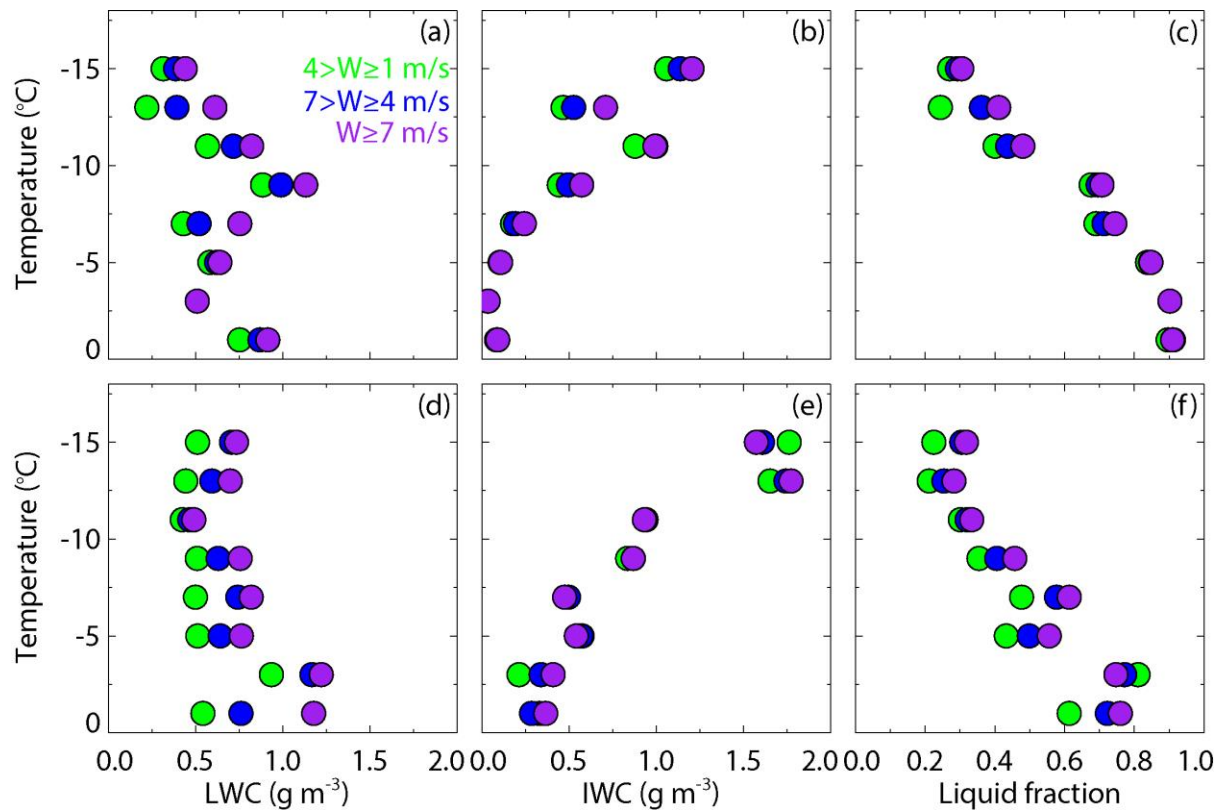


Fig. R3: The mean profiles of LWC, the IWC, and the liquid fraction as a function of temperature for the (a-c) young turrets and (d-f) mature turrets with vertical velocities of $1 \text{ m s}^{-1} - 4 \text{ m s}^{-1}$ (green), $4 \text{ m s}^{-1} - 7 \text{ m s}^{-1}$ (blue) and greater than 7 m s^{-1} (purple).

In the revised manuscript, we decide to add a discussion section to highlight the importance of the interactions between dynamics and microphysics, and discuss the possible impacts of entrainment and microphysics on vertical velocity.

Technical and other details:

Lines 128-138: What are typical reflectivity in the convection. I know this will be from the W-band radar but this information would still be useful.

Answer:

The reflectivity depends on the stage of the clouds. The reflectivity in convective core is typically 10-20 dBZ in ICE-T, 5-20 dBZ in COPE, and 0-15 dBZ in HiCu. These reflectivity values may not reveal the maximum reflectivity in convective cores due to sampling issue. We've added this information in the text.

Lines 171: Define strong updrafts since these are still relatively weak compared to deeper convection.

Answer: We have changed "strong updrafts" to "relatively strong updrafts". And have added *"These drafts maybe strong for isolated convections, but not necessary strong compared to MSCs"*.

Line 189: This accuracy (0.2 m/s) is quite good. Is there any chance there are biases rather than random errors on the vertical velocity?

Answer: There are no other instruments as references to provide systematic errors on vertical velocity. In the three datasets we do not see unrealistic values of vertical velocity (except a few cases in which the instrument was not working, which have been excluded in the study). Generally, the 0.2 m/s could be seen as the systematic error, random error could be larger than 0.2 m/s.

Line 209: Footnote. This is roughly the sensitivity of CloudSat. Is this the reason -30 dBZ was chosen since there will be cloud at lower reflectivities?

Answer: We choose this threshold by plotting the reflectivity near flight level in cloud free air. As shown in Fig. 2 in the manuscript, the reflectivity near flight levels is about -30 dBZ in cloud free air due to WCR signal noise. At levels far above or below flight level, the noise level is higher. In this study we mainly use in-situ measurement, so we only consider the reflectivity near flight level. Clouds with reflectivity lower than the noise level cannot be identified by WCR, and are excluded in this study, most of them maybe not convective clouds.

Lines 233-235: How do you know the 2D symmetry of the updraft since you might not fly through the peak up and downdrafts? Both the W-band radar and in situ measurements will not tell you this.

Answer: Here we want to show whirling penetrations and penetrations with significant turns have been rejected, so the cloud scale will not be significantly overestimated. We have modified this sentence to make it clear.

Line 236: “there is no” should be “there are no”

Answer: The comment has been addressed in the revised manuscript.

Line 238: Excluding MCS biases the results. This comes down to emphasis in this paper on small to moderate convection rather than deep convection in MCSs. Might mention this to keep the scope of your study in perspective.

Answer: We have pointed out the sample issue in the revised manuscript, including abstract, introduction, datasets description and conclusion. We also changed the manuscript title to “Characteristics of Vertical Air Motion in Isolated Convective Clouds” to highlight that this study deals with isolated convections rather than mesoscale convective systems.

Figure 4: need labels for field experiment associated with each color.

Answer: Labels have been added.

Lines 264-268: It might be useful to plot one example of a trace through one of the updraft/downdraft penetrations. This would be helpful to understand some of the averaging performed.

Answer:

Good suggestion. But the clouds were randomly sampled in the three field campaigns, we do not have continuous penetrations in one updraft/downdraft. More data are needed in the future. In addition, in-situ data itself is not enough to resolve the fine structure, in Fig. 2 in the manuscript, we can see many fine structures from the Doppler velocity measured by WCR, in-situ measurements can capture the details at single levels.

Line 268: “turbulences” to “turbulence”

Answer: “turbulences” has been changed to “turbulence” in the revised manuscript.

Line 300: “convections” to “convection”

Answer: “convections” has been changed to “convection” in the revised manuscript.

Line 294: “strong draft” – I would again put this in perspective since it is strong in your study, but not necessarily strong with respect to MCS updrafts for example.

Answer: We have add a sentence to indicate the definition of “strong” is only for this study, but not necessarily strong with respect to other convections (e.g. MCS): *“The definition of “weak”, “moderate” and “strong” only apply for this study. Other convections (e.g. MCS) could have much stronger updrafts.”*

Line 323-324: Again, this point should be more prominent in the paper.

Answer: We have highlighted that the definition of “weak”, “moderate” and “strong” only apply for this study.

Lines 386-387: This statement should be in the summary/conclusions since these measurements are important but we need a lot more.

Answer: We have added this statement in the conclusion.

Line 397: “results” to “result”

Answer: “results” has been changed to “result” in the revised manuscript.

Line 403: “relatively” to “relative”

Answer: “relatively” has been changed to “relative” in the revised manuscript.

Figure 10 and similar plots: I find these plots a little complicated and confusing but probably acceptable for publication. I can't think of an alternative but possibly there is a better way to plot.

Answer: We tried to improve the figures but haven't found a better way, because there are a lot of information in the figure. We have modified the text to better describe this figure.

Line 422: Some of the statements in this section should go in a summary and conclusions section. Points like lines 468-473 are important summary statements. You should consider pulling some of the summary points like this and putting them in the conclusions.

Answer: Statements with key points have been added in the conclusion.

Line 469: “pervious” to “previous”

Answer: “pervious” has been changed to “previous” in the revised manuscript.

Lines 509-510: “While in this study” should be something like “In contrast, this study shows the strongest. . .”

Answer: The sentence has been changed to “*In contrast, this study shows the strongest updrafts and downdrafts were observed at higher levels*”.

Line 522: “When exclude” to “When we exclude” Line 536: “convective cloud” to “convective clouds”

Answer: The comment has been addressed in the revised manuscript.

Lines 539-540: Can you say anything about the two-dimensionality of drafts from the remote sensing data?

Answer: We have added the following sentence in the text: “for example, airborne radar with slant and zenith/nadir viewing beams can provide two-dimensional wind structure in convective clouds”.

Line 553: “with expectation” to “expected”

Answer: “with expectation” has been changed to “expected” in the revised manuscript.

Line 564: “Since . . .to better”. This is an obvious fact. May want to just say that the aircraft just provides a line of data through drafts, and not vertical information unless the plane makes multiple passes through the same cell.

Answer: We have changed to sentence to “*Since the aircraft just provides a line of data through drafts, and not vertical information unless the plane makes multiple passes through the same cell, more data, including remote sensing measurements are needed to better understand the evolution of the vertical velocity in convective clouds at different stages.*”

Line 568: in the Summary section, you should reiterate the criteria for considering up/downdrafts, i.e., >xx m/s.

Answer: The criteria for considering up/downdrafts is reiterated.

Line 596: Flux calculations assume two-dimensionality of drafts and this might not be the case. Should mention this as a weakness in the study, i.e., using a single line penetration through drafts to make flux calculations.

Answer: We have highlighted that due to the limitation of aircraft measurements, the air mass flux is calculated using the data from single line penetrations. This may not fully capture the real air mass flux in the clouds and is a weakness of this study.