Reviewer's comments in black, replies in blue.

I always like to see an in-depth study of vertical motions in the atmosphere because, as the authors point out, understanding these is vital to improving our understanding of (and hence modeling capabilities) many processes influenced by vertical motions. First, before I get to the science, this document was not ready for submission in any form. It is riddled with typographical errors making it very difficult to get to the science. I started to list them but, frankly, this is the job of an editorial service, something I recommend the author take advantage of. For example: "The COPE project was conducted from 03 July to 21 August, 2013". This is not English.. "The COPE project was conducted from the 3rd of July to the 21st August, 2013".. Write in English not in code. I have two broad areas of concern with this manuscript:

Answer:

We appreciate the reviewer's comment and sorry for the typographical errors. 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 raised by Reviewer 1, we found that many typographical errors pointed out by the Reviewer 1 exit 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 . In this round of revision, we have corrected a few more typographical errors.

I have two broad areas of concern with this manuscript:

1) The authors do not address the idea of sample size or sample bias OR more importantly geometric issues of sampling, in a line, a 2/3D object (being an updraft core). See Giangrande et al 2013 for a discussion of issues with profiler systems and angle of attack. Basically if you dissect an updraft core how do you know if you hit the strongest part of the updraft? Furthermore, up until the end, the idea of selection bias is not addressed. Even the C-130 will avoid the strongest cores. You can not build a PDF out to the tail from aircraft measurements. You can, as the paper did somewhat, look at intrinsic updraft properties. But you can not look at the distribution. I am somewhat disappointed , given the brief reference to microphysical measurements, that the authors did not relate vertical motions to microphysical properties of the updraft cores. This is something in-situ platforms are uniquely capable of doing. Also, in the literature review of methodologies for measuring vertical motions the authors neglect scanning radar measurements such as those shown in Collis et al 2013 and Nicol et al 2015 (not to mention a raft of airborne radar measurements from the NOAA p3 (look for papers from Jorgensen) and other aircraft that use the vertical plus 45 degree tilt methods.

Answer:

We totally agree with the reviewer that there are many limitations in aircraft measurements. First, aircraft might not penetrate through the strongest part of drafts due to safety issues. In addition, aircraft cannot provide 3-D information of the cloud, and the air mass flux is derived from measurements in single-line penetrations. Moreover, this study only deals with isolated convective clouds. Only three field campaigns are analyzed and MCSs are excluded in this study. The results cannot be generalized globally. We have pointed out these weaknesses 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 (MCSs).

For the PDF distributions, we think it will be good to keep them the paper even though there are potential sampling issues. First, modelers do need the aircraft measurements to provide PDF distributions of vertical velocities (personal communications: Guangjun Zhang, Xiaohong Liu and Sungsu Park). Second, due to the relative small sizes of isolated convective clouds, the sampling bias associated with where to penetrate clouds is not as large as sampling MCSs. During the sampling of isolated convective clouds, we typically aligned the central part of cloud to penetrate at the flight height. During ICE-T and COPE, we have penetrations in updrafts stronger than 20 m/s (please note this is just for isolated convections, in which the updrafts are weaker than MCSs), and previous studies based on in-situ data rarely reported such relatively strong updrafts. Actually, this is one of our motivations to make this study. The PDFs can also be used to evaluate and improve remote sensing retrievals because in-situ measurements are more accurate than remote sensing, especially in mixed-phase convective clouds. Then remote sensing can provide PDFs out to the tail. Therefore, the PDFs in the paper still provide valuable information, but readers do need to be aware of the weaknesses and limitations of aircraft measurements.

We tried to explore the interactions between microphysics and vertical velocity, but the physical processes are very complicated, and there are many limitation of aircraft instruments in measuring the microphysics in mixed-phase convective clouds. For example, FSSP has the shattering issue, hot-wire probes often underestimates the LWC because there are many large drops which cannot be directly sampled by these probes. 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 partition in the mixed-phase cloud region within convective clouds (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. R1, 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 is more significant in weaker updraft (Heymsfield and Willis 2014), results in relatively larger fraction of IWC. Such in-depth analyses only can be applied to ICE-T measurements in that paper because in COPE and HiCu we do not have the appropriate instruments to provide sufficient measurements.

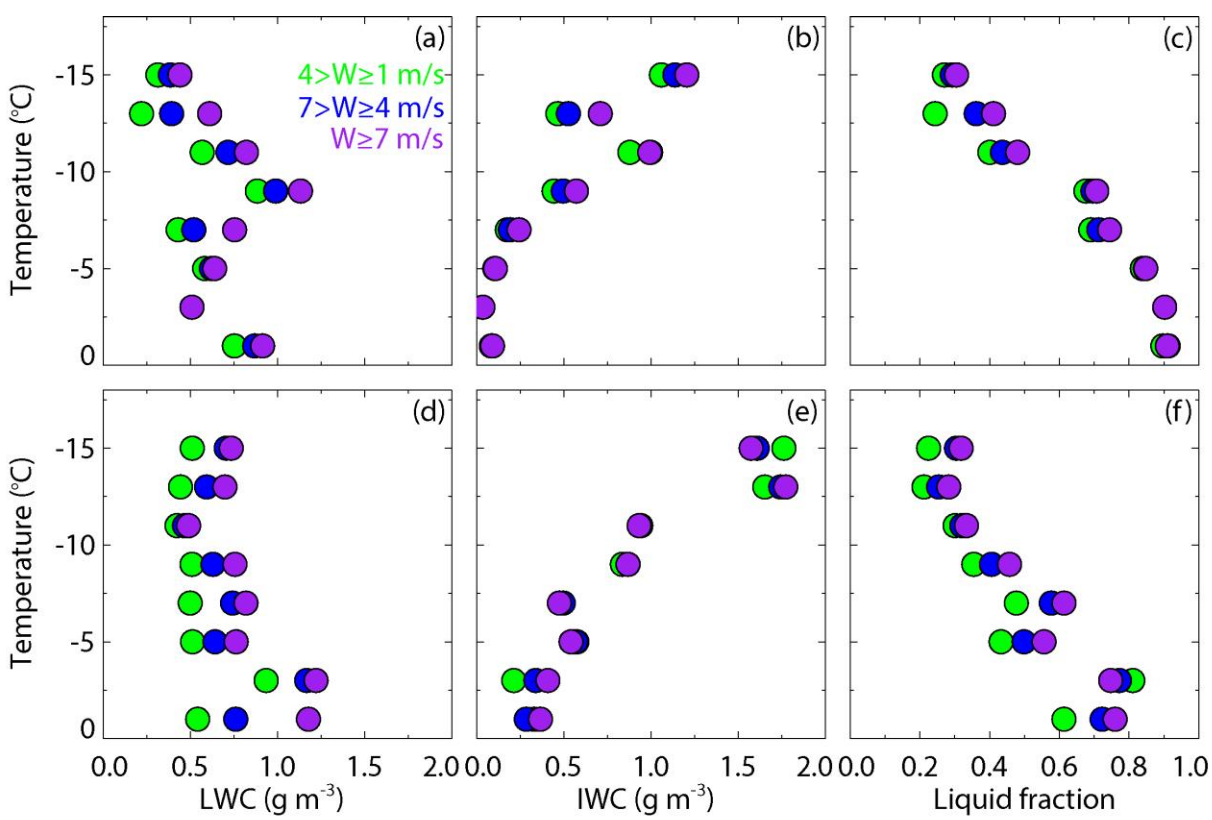

Fig. R1: The mean profiles of LWC, the IWC, and the liquid fraction as a function of temperature for the (a-c) young turrets and (d-f) mature turrets with vertical velocities of 1 m s^{-1} – 4 m s^{-1} (green), 4 m s^{-1} – 7 m s^{-1} (blue) and greater than 7 m s^{-1} (purple).

Other than the interactions between vertical velocity and microphysics, entrainment/detrainment mixing also have impact on vertical velocity. But due to the complexity of the physical processes and the limitations of aircraft instruments, we think it is better to address this problem in detail in separated paper as well. (Please see the reply to Reviewer 1's comments).

In the revised manuscript, we 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.

In the revised manuscript, we have added the literatures about ground-based and airborne volumetric radar measurements in Introduction. For example, "*Collis et al. (2013) provides statistics of updraft velocities for difference convective cases near Darwin, Australia using retrievals from ground-based scanning Doppler radars and a multifrequency profiler". "Airborne volumetric Doppler radars have also been used to study the dynamic structure of convective clouds (e.g. Jorgensen and Smull 1993; Hildebrand et al. 1996; Jorgensen et al. 2000)". "Remote sensing has the advantage of being able to measure the vertically velocity at different heights simultaneously (Tonttila et al., 2011), and some of the techniques can detect the strongest updraft cores in convective clouds (Heymsfield et al. 2010; Collis et al. 2013)". "Volumetric radars can provide three-dimensional (3D) structure of air motion in convective clouds (Collis et al. 2013; Nicol et al. 2015; Jorgensen et al. 2000)".*

2) This comment relates to a specific question asked by the Journal in its review criteria "Are substantial conclusions reached?". I am deeply concerned by the authors attempt to relate the three field programs and say something about maritime versus continental convection. For one, the author did not put the cases into context. What was the CAPE for various cases? etc.. A selection of clouds at each campaign a climatology does not make. While the author caveats his comparison even the attempt to contrast the different regime is dangerous. For one, as mentioned, the strongest cores in the region of HiCu would all but destroy even the C-130 (See the various photos associated with the Byers et al study of hail damage). To attempt to make a comparison, then state it goes contrary to common conception (Continental » Maritime) and then turn around and say "we did not sample the strongest updrafts in the continental case" is disingenuous.

So negatives out of the way, one of the things that redeem the paper is the focus on updraft shape and how that varies with height. Personally I find this very interesting as not only does the mass flux of a plume influence transport but the vertical velocity within determines many microphysical aspects. ie a plume that starts thin and then expand for the same mass flux would have lower vertical velocities aloft influencing processes like Hallett-Mossop splintering etc.. (and associated latent feedbacks).. The paper should focus more on this and the *intrinsic* differences. Things that are co-varying and less susceptible to sampling and decision bias.

Answer:

We appreciate the reviewer’s comments. We have pointed out the weaknesses of aircraft measurements in the revised manuscript, including abstract, introduction, datasets description and conclusion, as well as the title.

Due to the limitation of aircraft measurement, we have deleted some results which are sensitive to the sampling issue. For example, “the vertical velocity in HiCu is weaker than that in COPE and ICE-T”. In addition, in this paper we plot the vertical velocity PDFs and profiles as a function of height MSL (Fig. 8 and 10), so at the same height, the vertical velocity maybe weaker in HiCu. However, the updrafts were strengthening with height, and some updrafts could be close to 20 m/s at > 6 km MSL (Fig. 8) in HiCu. Maybe at higher levels the updrafts in HiCu were stronger than COPE and ICE-T, but we do not have more data. If we plot the updraft PDFs and profiles as a function of height above cloud base, the results in HiCu maybe closer to that in COPE and ICE-T. However, cloud base heights are variable and we do not have data to calculate the cloud base heights.

In the revised paper, we have added some text to describe the ambient conditions which many affect the vertical air motion. For example, *“the convective available potential energy (CAPE) in ICE-T is greater than 2000 J kg^{-1}. The CAPE in COPE is typically a few hundred J kg^{-1}. No soundings are available for HiCu, so we have to use aircraft measurements to estimate the CAPE. In some cases, the full CAPE cannot be calculated since the aircraft only flew at low levels (< 10 km MSL). The aircraft measurements suggest the CAPE in HiCu ranges from less than 100 J kg^{-1} to more than 500 J kg^{-1}”.*

As suggested by the reviewer, we have added more discussion about the *intrinsic* differences among the three field campaigns. For example, the downdrafts in HiCu and COPE are obviously stronger than that in ICE-T, maybe partly due to the evaporation-cooling effect induced by entrainment (please see the reply to Reviewer 1). We also changed Fig. 11 to Fig. R2 as follows to show how the draft shape changes with height. Actually, the evolution of draft with height is very complicated. Based on our datasets, there could be different possibilities: 1) an updraft expands and the vertical velocity weakens with height, 2) an updraft expands and the vertical velocity strengthens with height, 3) an updraft splits to multiple updrafts and downdrafts, 4) two updrafts merged and become one updrafts. Since we do not have continuous penetrations in a single cloud, we have to statistically analyze the evolution of draft shape. In Fig. R2, we can see that the normalized shape do not have significantly change with height, the peak vertical velocity is strengthening with height. Connecting this figure to diameter (Fig. 4), vertical velocity (Fig. 8) and air mass flux (Fig. 9), the results show statistically, the drafts were expanding (Fig. 4) and the vertical velocity was strengthening (Fig. R2 and 8), but the air mass flux was not increasing (Fig. 9). This reveals the complicated physical processes (e.g. entrainment, water loading and the possibilities described above). The interaction between vertical velocity evolution and microphysics is even more complicated and needs to be analyzed in detail in separated papers (please see the reply to the first comment above).

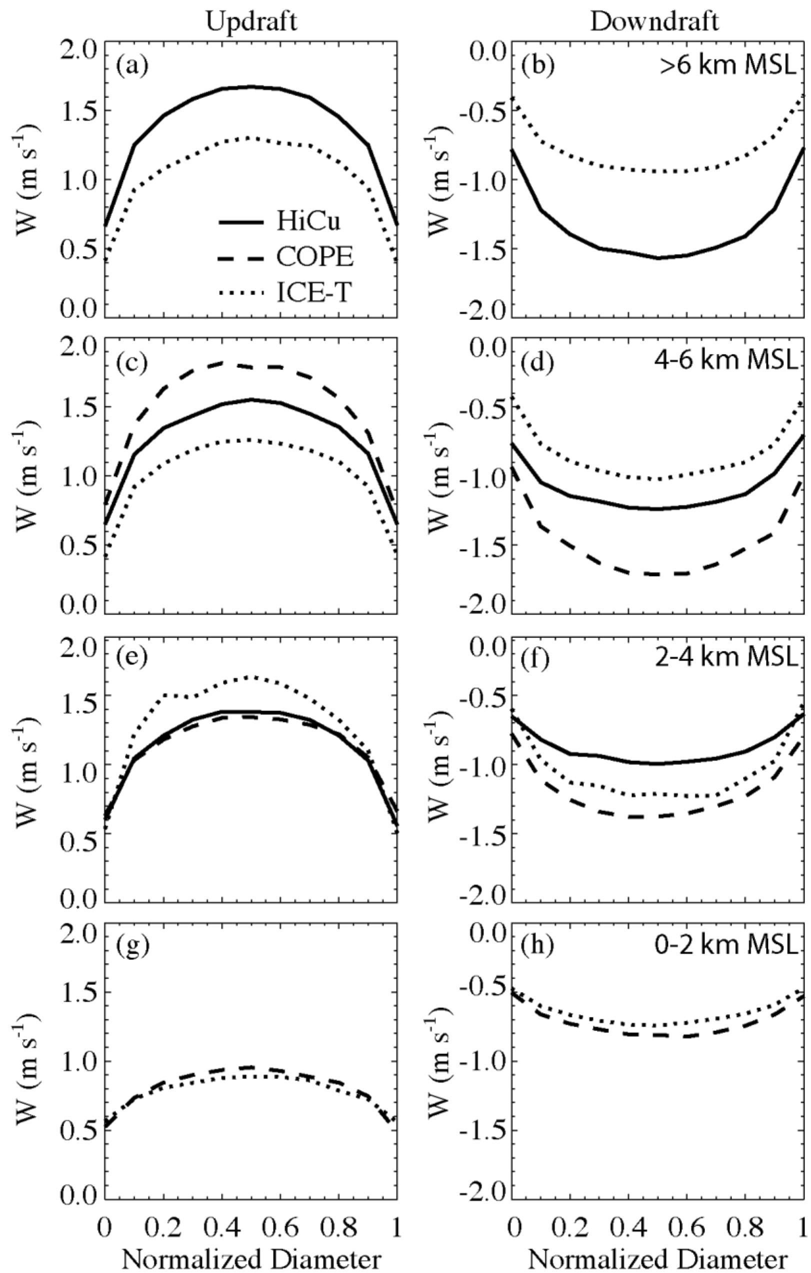

Fig. R2: Composite structure of the vertical velocity as a function of the normalized diameter for the updrafts and downdrafts with air mass flux ≥ 10 kg m^{-1} s^{-1} in magnitude. The 0 and 1 coordinates on the x-axis indicate the upwind and downwind sides of the draft.

Finally, we want to say this paper is just a part of the whole picture. The physical processes in mixed-phase convective clouds (e.g. interaction between dynamics and microphysics) are very complicated, and need to be further explored in the future with more experimental data, especially with more advanced measurements. The contributions of this paper are 1) provides statistical results of vertical air motion in isolated convective clouds using in-situ data in recent field campaigns, which could be used to evaluate remote sensing retrievals and model simulations. 2) In-situ measurements of vertical velocity stronger than 20 m/s in isolated convective clouds are provided. Previous studies using in-situ measurement rarely had penetrations in such relatively strong updrafts. 3) This paper highlights the importance of small drafts using high-resolution in-situ data, which is not shown in previous studies. 4) Some 'intrinsic' differences and similarities of vertical air motions among the three field campaigns are discussed. Aircraft measurements do have many limitations and this paper only deals with isolated convections, we have highlighted them in the revised paper.