RESPONSES TO REVIEWERS ARE IN BLACK
REVIEWERS COMMENTS ARE IN BLUE

REVIEW A

Review of "Characteristics of Deep Convection from Nadir-Viewing High-Altitude Airborne Doppler Radar" by G. Heymsfield et al.

Summary

This paper uses the EDOP data collected over the years to examine differences in deep convection in the land, ocean, coastal, and tropical cyclone cases collected. The paper investigates differences in echo and vertical velocity characteristics for these cases. For echo characteristics, the authors provide a somewhat inconsistent dataset, with some data that has been corrected for attenuation at X-Band and some that is not. The authors do not address this issue, and unless I am misunderstanding them, this would be a major shortcoming of their methodology.

There are some other conclusions that I believe could greatly enhance the utility of the study that were not addressed adequately. Based on my review, I suggest that the authors need to address the attenuation correction issue with a revised dataset, or place some major caveats on the conclusions. Other than these issues, I find the paper makes progress on attempting to retrieve vertical velocity in mixed phase regions, although uncertainties are not treated. I recommend the paper be accepted with major revisions detailed below.

We appreciate the reviewers comments that point out some analysis questions that were not addressed adequately in the paper. For the major comment on attenuation, we had actually run the attenuation correction on most of the cases except the very oldest data sets. The overall data set has been updated so that all the cases are attenuation corrected. Figures that involved the uncorrected cases were replaced. In addition, we added one more case and fixed some plotting errors in the figures.

Major comments

1. On the bottom of p. 8, the authors write:

"Reflectivities were corrected for attenuation using the surface reference approach (Iguchi and Meneghini 1994). The correction was not always performed since EDOP's nadir "surface" receiver channel was not available for all flights and the "rain" receiver channel saturates at the surface. Reflectivity without this correction would result in lower values in the rain region where most of the attenuation occurs. The attenuation correction over land is of lower accuracy since the background (non-precipitating) surface reflectivity returns are more difficult to estimate (Tian et al. 2002)."

The fact that a consistent methodology for addressing the attenuation correction within the dataset places any conclusions drawn from this paper on shaky ground. Since there is a small sample of observations to begin with, non-uniform application of an attenuation correction to the data could drastically bias not only the reflectivity profile calculations, but the calculations of terminal fall speed due to the use of Z-Vt relations among cases. Particularly troubling is the fact that this correction was not-uniformly applied, and the reader has no idea when the surface reference technique was and was not applied. Since attenuation in mixed phase regions can easily exceed several dB/km (e.g., Battan (1971), *JAM*), attenuation will cause the improper Z-Vt relation to be applied in a given situation, as well as cause a systematic overestimation of vertical velocity and underestimation of echo top heights. If characteristics of the convection vary systematically among land and ocean cases (one could imagine more water coated hail in electrified continental oceanic convection relative to ocean), this could impact conclusions drawn from the results. Also, for completeness, what k-Z relationships did you use in mixed phase regions when applying the SRT (Iguchi and Meneghini (1994)), and how might these assumptions impact the results (e.g., cause systematic biases) in intense systems?

As it turns out, just before submission of the manuscript, we did attenuation corrections on all the cases except the 1995 data sets. In the revision, we have made attenuation corrections on all the flight lines. The figures have been updated appropriately for the 10 cases corrected.

The differences in the figures are insignificant for the following reason. Attenuation at X-band kicks in more significantly at around 40-45 dBZ in convective cores. The majority of the properties we calculate are for 40 dBZ or below. The >40 dBZ echoes are usually at 6 km altitude or less. Since we're looking from top down, the first detection of a 40 dBZ contour will not be attenuated very significantly. The high reflectivities are mostly in the mixed phase or rain regions. Doppler velocities for rain will not be affected more than 1-2 meter per seconds for any reasonable error in reflectivity. If there is hail, we will underestimate the fallspeeds with the current algorithm so the updrafts will be stronger than what we've presented (so we have presented a conservative estimate of updrafts). Discussion is added at the end the Appendix on errors.

We used "hybrid" approach of Iguchi and Meneghini for attenuation correction using k – Z relation from Battan (1973) for rain at X-band. The method adjusts the attenuation so that both the initial and final condition of PIA are satisfied. The initial PIA = 0 and final PIA is determined from the surface return. Of course the mixed phase region does present challenges since we don't understand the scattering physics well. Errors in the reflectivity corrections are hard to quantify but I highly doubt they will impact the results significantly since much of our focus is above the mixed phase region. We did sensitivity tests on fallspeed assumptions (see reviewer C comments) and these did not have a major impact on the plots.

2. The biggest advantage EDOP has over spaceborne radars is the high spatial resolution.

If the results of the study are sound given major point (1) above, then this study may show that the differences shown by TRMM between land and ocean convection may be partially explained by the horizontal size differences of the convection among regions, in addition to echo profile intensity differences. These differences cannot be resolved by TRMM due to its spatial resolution, and just show up as differences in vertical reflectivity profiles. I believe these ideas need to be developed more throughout the results and conclusions sections of the paper. How might your results change if the resolution of the data were degraded to 4-5 km?
We've tried to keep the discussion on the observed properties with EDOP and we use the previous TRMM results as a reference. The paper has already gotten lengthy and we wanted to focus more on the overall measurement conclusions so that they could be used toward our understanding of cloud dynamics and for improving models. We did in fact have a paper (Heymsfield et al. 2000) that did precisely the TRMM comparison for reflectivity-only using some of the cases in this paper. This paper is now referenced. We now discuss resolution differences when referencing previous TRMM statistics.

3. Analysis methodology, p. 10: *"As mentioned previously, intense convection in the current study is defined by either a 20 dBZ echo above 12 km altitude, or by updrafts with magnitudes >10 m s-1 at any altitude."* Why did the authors include both a reflectivity and a vertical velocity (w) threshold in selecting cases? It seems to mix the utility of the results. On one hand, a reflectivity threshold might help examine w distributions in convection as seen by an incoherent radar (such as TRMM). A w threshold might help examine reflectivity differences associated with a given subset of convection with strong updrafts in a consistent manner. Mixing the two thresholds seems to confuse this issue, and I request that the authors clarify this choice that seems to unnecessarily confound these two issues.
We made this choice for the following reason. Our selection of cases originally was based on assembling ALL the strong convection cases that we had available – either high reflectivity cores or strong updrafts. We clearly have a biased sample of cases but we wanted to use all of them to get an estimate on the range of updraft and reflectivity magnitudes. We have added: "The rationale for using either of these parameters is that convection often evolves where updrafts are strongest and reflectivities weakest in the early to mature lifetime, and reflectivities and downdrafts are strongest in the mature and dissipating periods, making it difficult to rely on just one of the parameters. Using both parameters provides an indirect method for handling cell evolution."

Minor comments

1. p. 4: *"The convective storm environment deduced from soundings (e.g. CAPE, vertical shear) and low level forcing, can be drastically different leading to different attributes of convection."* This statement seems to suggest that CAPE is a determiner of convective attributes, but this flies in the face of a lot of work by Earle Williams, Jeff Halverson, and Ed Zipser that says that attributes like CAPE do **not** have much to do with convective characteristics.

This statement is incorrect. Halverson's papers suggest that CAPE provides the primary control of the updraft, but not just its magnitude, but also the "shape of the CAPE", i.e., how the positive area is distributed with height. He also talks about how shear interacts with an updraft to induce non-hydrostatic, vertical pressure gradient forces. Earle Williams believes the microphysics (dirty vs. clean air) has an important role. We would still argue for the diverse data sets in this paper, that the environmental factors are first order determining convective intensity and not the aerosols.

2. p. 7: Unattended Aerial Platforms ≠ acronym UAS?

Fixed.

3. p. 8, 2nd paragraph: *"-10 dBZ at 10 km range (10 km altitude) from 1995– 1997, and -10 dBZ at 10 km range after 1997"* – there must be a typo here because I don't note any change in MDS?

It should be 0 dBZ at 10 km range for the 1995-1997 data. This will be difficult to discern in the data. Fixed.

4. p. 9, 1st paragraph: *"Once the fallspeeds are estimated and added to the hydrometeor motions, a median filter is used to remove spurious values and a 9-point (~338 m) running mean is then applied to provide additional smoothing."* Does the size of this filter impact your results? One could imagine a situation where this filter might amplify signals of certain wavelengths over others, possibly impacting comparisons of updraft sizes and intensity that vary in nature.

We tried various filters on the data but in the end, the code used a 9-point median filter. These filters are used to remove spikes in the data without modifying the data itself. So there is minimal filtering on the data. Keep in mind that one point used in the filter is 100 meter resolution so the beam width is the limiting factor in any width calculations.
The filter discussion was corrected along with Reviewer B comments: "Once the fallspeeds are estimated and added to the hydrometeor motions, a 9-point median filter is used to remove spurious values (spikes) from the data without altering the widths of features. The EDOP antenna side lobes are 56 dB (two-way) down from the main antenna lobe, so these will not appreciably broaden the width of the measurements."

5. p. 12, 2nd paragraph: *"The is the…"* needs revision.

Fixed. Should be "This cell is .."

6. p. 14, 1st paragraph, 1st sentence: Could the lower density aloft also lead to increased updraft speeds in this case?

Are you asking whether the air density that decays exponentially with height would that lead to larger updrafts aloft?

It is the perturbation density that is responsible for buoyancy, not the total density. So just because the environmental air is less dense at 12 km height doesn't mean the updraft should accelerate. Now, if the density of the air parcel relative to the environment was less dense, then this would shoot up the updraft (this is buoyancy).

p. 16, 1st sentence: Large resolution differences hamper comparisons with TRMM: Would this storm look so extreme if the resolution were lowered to TRMM's?

In some cases, the storm would look less extreme in TRMM because of resolution. This is well known. Examples of this are shown in Heymsfield et al. (2000). It is pointless to spend much time on this issue since our main point is to take high resolution data and establish relationships between reflectivity and vertical velocity if they exist. Then if one sees high reflectivities aloft with TRMM, this would provide credibility to the TRMM reflectivity-only statistics inferring that high reflectivities aloft are correlated with strong updrafts. This discussion has been improved in the paper.

"As noted earlier, the resolution of EDOP measurements is a factor of 5 to 10 higher then the satellite TRMM PR measurements. The approach here is to look for physical relations in the higher resolution EDOP measurements, and then use them to validate previous published inferences from TRMM PR reflectivity-only measurements that may have under sampled convection."

8. p. 17, 1st paragraph, last sentence: EDOP beam is filtering, sidelobes also smear the echo, and you are smoothing the data as well. Some discussion of the resolution differences w.r.t. TRMM and it possibly leading to larger differences than observed with EDOP, keeping in mind the small sample you have, is in order.

See previous comment #2 and #4 responses.

9. p. 19, 2nd paragraph, last sentence: Resolution differences are important in this discussion. What would happen to the correlation in Fig. 13 if the resolution were degraded to PR?

We do not feel degrading the data is the correct approach but to use the higher resolution data to obtain physical relationships. "As noted earlier, the resolution of EDOP measurements is a factor of 5 to 10 higher then the satellite TRMM PR measurements. The approach here is to look for physical relations in the higher resolution EDOP measurements, and then use them to validate previous published inferences from TRMM PR-only measurements that may have undersampled convection. " We also referenced an earlier paper as mentioned above.

10. p. 25, 1st line: “Tropical Chantal”

Fixed

11. p. 25, 2nd paragraph, *“Most large drops freeze by -10oC and the cloud drops freeze at the lower temperatures they observed.”* Needs revision.

Sentence was replaced with: “They examined rain drops and the small coexisting cloud drops can freeze at much lower temperatures (e.g. -38^{o}C) by homogeneous nucleation (Heymsfield et al. 2005).”

12. p. 27: There is an orphan paragraph.

This was removed.

REVIEW B

Review of: Characteristics of Deep Convection from Nadir-Viewing High-Altitude Airborne Doppler Radar, by Heymsfield et al.

General comments

We greatly appreciate the these comments and suggestions. Several historical references were from your suggestions. Other suggested changes have been made on the text as detailed below. Perhaps the only suggestion we did not incorporate was the reduction of the Appendix. Reviewer C wanted more justification of the fallspeeds assumed in the calculations, so we feel it is necessary to keep this detail in the Appendix.

This paper characterizes vertical velocities and reflectivity for four categories of strong precipitating convection, presenting information the community has been waiting for. As such, this is an important and exciting paper. The early work based on GATE suggested that vertical velocities in tropical convection were weak, but the aircraft generally flew below the freezing level. While this work proved to be significant from several points of view (including a better understanding of the role of dynamics in electrification), people still didn't know what was going on at higher levels.

The discussion related to previous work is improved in the Introduction and elsewhere in the text.

There were hints from TOGA COARE (references in this review), Anderson et al., and even some work by Peter May, as well as some observational data shown in Fierro et al. (2009). (Indeed, the model data compare well to the strong updraft in this paper). But this is the first paper that presents statistics.

However, there are shortcomings. I found myself editing the paper as I read it. Some of the minor comments below have to do with revisions. Furthermore, the authors get sloppy in several places (comparisons with Szoke et al, wrong model for Fierro et al., etc – pointed out in specific comments. Also, the Appendix needs a rewrite. I really couldn't get through it (See specific comments, below)
The Appendix writing has been improved.
Some points, sometimes repeated in "specific comments"

- Differences in reflectivity from Szoke are undoubtedly due to the selection of stronger cells (p. 9, middle paragraph). There was not sufficient information to separate out stronger systems in GATE, so all deep convective clouds were included in the sample.

A few sentences were added to as suggested on the GATE and Szoke study.

- Regarding bias in diameter due to missing the center of circular cores has been calculated by Jorgensen et al. (1985, JAS). (By the way, there are reflectivity profiles in this paper – have you compared them to your results?)

Added in Fig. 7 discussion: "It should be noted that the peak values derived in the radar measurements here have been filtered by the radar beam over a broader area then previous aircraft flight level vertical velocity measurements such as Jorgensen et al. (1985). They showed that when the aircraft did not pass directly through the updraft maximum, it could be underestimated by a factor of 2.2."

- There are types, variously labeled as "groups", "classes" and "categories" in Table 2, the figures, and the text. It would be clearer if the same word was used, particularly since you have "campaigns" and "cases."

We now use "categories" throughout paper for consistency.

- I am assuming that "altitude" is AGL. Is that correct?

Added: "We note that altitudes in these and other figures throughout the paper are referenced the Global Positioning Altitude (GPS), so they are closest above sea level and not above ground level. The majority of our cases are near sea level, with TRMM LBA cases at ~550m altitude for example. TRMM measurements studied by Zipser et al. (2006) and others are based on GPS altitudes that are referenced to geoids so we are most consistent with these satellite heights."

- Are the "sea breeze" cases all over land? (Again, an assumption on my part – that the systems initiated along sea breezes were over land and stayed there).

Added: "….; they were very close to the coastline and were within ~30 km of it."

- I really think the figures were well-designed. I particularly like Figs. 7-10. However – one thing the figures really tell the reader that isn't really discussed is what they reveal about the relationship of other properties to maximum vertical velocity. (My version of the paper has these figures covered with eyeball "best-fit" lines). The point is brought out, however in Figure 14 for the association of peak updraft speed to peak reflectivity.

This discussion was being saved for Figure 14 discussion but a sentence or two are now added earlier in the paper.

Thus – revisions are needed, particularly editing. There are no major science issues as far as I can see, except that I would have liked to see more comparison to previous data as well as more care in the present comparisons to earlier work. I would be glad to take another look.

Specific Comments

p. 3, line -5. The sampling "bias" is to some degree dependent on the field program described. In GATE, not all the aircraft deviated around strong cores. In TOGA COARE, penetrations were typically done after the storms strongest phase (which was sampled using Doppler radar).

Agreed. Added the sentence: "Many biases in the intensity of convection have often been related to the field experiment, cloud penetration safety issues, the specific aircraft used for the studies, and the type of instrumentation (i.e., *in situ* or radar)." There is also some minor rewording in the paragraph.

p. 4, end of first paragraph. Here, it is recognized that storm *type* is important. Later, on page 18, differences are attributed to other factors.

This sentence was modified to be more consistent with later discussion.
"One would expect that the environmental conditions that are often a function of geographic location,…"

p. 6, line 2. "the closer the radar echo top" rather than "the more likely the radar echo top"
Fixed

p. 6, line 9. "not been measured adequately due to the lack of observations"? Isn't that redundant? How about "have not been sampled adequately."
Fixed.

p. 6, line 12. Don't need "the presence of"
Fixed

p. 6, observations of vertical velocity in the upper part of storms. Also see Peterson et al. (1999, BAMS), and perhaps Hildebrand (1988, MWR, I remember hearing from him about strong vertical velocities in the upper part of convective systems). Other authors that might be checked – Peter May (looking at vertical velocities from ground-based radars) and Brian Mapes (same thing).

We referenced May and Rajopadhyaya (1999) since they show a nice profile that gets close to what we are observing but limited by the maximum height of their observations.

p. 6, line -11: Start new paragraph with "Early"
Fixed.

p. 6, line -3. "tropical oceanic convective cores are dilute or undiluted"
"undiluted". Fixed.

p. 7, first paragraph, last sentence. Since we already know from Jorgensen et al. (1985) that hurricane and MCS hot towers differ – perhaps it would be more appropriate to write something about gathering further statistics about hurricane vs MCS hot towers – especially at higher altitudes, where data are scarce to nonexistent?
This sentence has been modified as per your suggestion.

p. 7, second paragraph, first sentence. Could delete "analysis"
Fixed.
p. 8, line 1. Could delete "width" and add "s" to "beam."
Fixed.
p. 8, line 6-7. If the beam is 1.1 km wide at the surface, how does this result in 0.5-km resolution? Because the signal is Gaussian?

Added: "The main filtering on the data is by the radar beam itself whose width increases from 0 km near the plane to ~ 500 m at 10 km altitude, to ~1km near the surface; data oversampling by a factor of 5 will result in a resolution less than these height dependent beam widths….. Earlier work by Lemone and Zipser (1980), Anderson et al. (2005), etc. defined their updrafts with vertical velocity thresholds over 0.5 km along the flight line, so these differences should be noted in subsequent discussion."

p. 9, line 6. What is a "median filter?"

From a web definition, but this is a standard filter in IDL. We use this for removing noise spikes that occur infrequently. DEF: The median filter is a non-linear digital filtering technique, often used to remove noise from images or other signals. The idea is to examine a sample of the input and decide if it is representative of the signal. This is performed using a window consisting of an odd number of samples. The values in the window are sorted into numerical order; the median value, the sample in the center of the window, is selected as the output. The oldest sample is discarded, a new sample acquired, and the calculation repeats. Median filtering is a common step in image processing. It is particularly useful to reduce speckle noise and salt and pepper noise. Its edge-preserving nature makes it useful in cases where edge blurring is undesirable.

p. 9, paragraph 2, first sentence. Are all the field programs NASA field programs?

Yes. All except the first (HOPEX) were process study types of programs; HOPEX was very small where EDOP piggybacked on another atmospheric radiation campaign.

p. 11, good discussion.
Thanks
p. 12, end of first paragraph. Don't need the last sentence.
Deleted

p. 12, first sentence of second paragraph. Suggest "intense" or "strong" rather than "supercell-like," which connotes a lot more than strength (unicellularity, near-steady state)
Changed. Added: "Note that this was probably a hail storm since a similar large storm in the LBA network that was observed the following day in radar polarimetric observations, had strong suggestion of hail."

p. 12, line -3. "are again…"
Not sure what is meant here but modified the sentence slightly.
p. 12, last line. Delete "contains"
Fixed.

Figure 5. Check the labels; I think they are wrong.
If you are referring to the labels on the panels in the caption, I eliminated confusion by putting the A-D panel number before the description of the panel rather than after it.

p. 13, middle paragraph, line 8: Delete "and"
Fixed.

p. 13, middle paragraph, line -6: Delete "vertical" in "vertical depth."
Fixed.

p. 13, bottom – p. 14, top, Section 3. Do you need the first paragraph? It looks like there are two introductions.
This paragraph has been deleted.

p. 14, first full paragraph, line 3, Suggest, after "convection": "included in Table 2." And then "Within each category, the points are identified by location"
Fixed.

p. 14 – Since you do this sorting, why don't you comment on the association of various properties with the maximum vertical velocity for Figs. 7-10?

p. 14, second full paragraph … range from *6* m/s …?
Yes, this should be 6 m/s. Fixed.

Caption of Fig. 7, line -2. Since you are labeling the four groups as "Categories" you should consistently use the word "Category" rather than "group"
Fixed.

p. 14, second full paragraph, line 2. Suggest "slightly lower average peak vertical velocities … than sea breeze cases (differences 2-5 m/s).
Changed.

p. 14, reference to the data in Anderson et al. Weren't most of the storms in LBA weaker than typical continental storms; in fact I've heard that LBA had a marine-like environment much of the time.
You've probably heard the expression "green ocean" when people talk about LBA convection. The two regimes during LBA were monsoon and easterly, where the easterly regime had the strongest convection. Most of the storms we studied were strong but not exceptional. My impression was that many storms had 20 m/s updrafts but were rather slender and short-lived. But there were a few intense storms in the easterly regime such as the one in this paper (Fig. 2) and the hail storm studied by the CSU group on 26 January 1999. We did not have insitu data from either of the cases. I'm sure there are sampling biases since the Citation avoided these strong cores.

End of same paragraph…

Suggest, "… strong, ranging from a few m/s to more than ~15 m/s; the land and sea breeze convection has significantly stronger average peak downdrafts than the oceanic … (17 m/s versus 11 m/s).
Fixed.

Figure 7. Why aren't you commenting on the association of stronger downdrafts with stronger updrafts, and the correlation of height with stronger vertical velocities for oceanic systems? (but not the other systems?). This informs your summary graph later on.
Added: "). It is interesting to note that the intensity of both peak updrafts and downdrafts are weaker for the oceanic and tropical storm cases than the other convection categories, but there is not a direct correlation between peak updrafts and peak downdrafts for any of the categories."

p. 15, top paragraph. The use of environment as an explanation makes sense. Can you quote references?
Referenced Lucas et al. (1994) comments since they address differences between continental and oceanic convection.

p. 15, middle paragraph, line 4. Suggest deleting "generally" (or "on the average") – don't need both.
Removed generally.

Same paragraph, line 7. Don't need "Figure 8"
Removed Fig. 8

p. 16, line 1. … altitude; these correspond to ?
This should be altitude and it has been fixed.

p. 16, top. Don't V and W correspond to the "easterly" regime mentioned earlier for LBA?
Yes. This has been restated.

Figure 8: Again – it is interesting how much the heights correspond to the maximum vertical velocities. Why not remark on this?
A sentence has been added making note that high reflectivities aloft are strongly correlated with strong vertical velocities in Fig. 7.

p. 16, second paragraph, line 2. "strongest updrafts" rather than "strongest convection," since the former is more specific?
Fixed.

p. 16, middle paragraph, line -3. Panel C not Panel D.
Fixed.

Figure 9. The outliers seem to be from LBA. Question – what height was used? AGL consistently? Again – there are interesting relationships with maximum vertical velocity. Why not point them out?

We used "above sea level" which is not standard (AGL) since the measurements are all referenced to the aircraft and terrain such as mountains are represented at their actual height. Most of the cases were near sea level so it should not make a lot of difference. A sentence was added earlier in the Fig. 2-4 discussion.

p. 16, discussion of Figure 10. Suggest first sentence read:

"It is difficult to obtain full profiles of the core reflectivity, so we have examined widths at the levels (6, 8, 12 km) used in Figs. 1 - 4." (I wonder if you need to include the part about the vertical velocities – it is interesting, but doesn't seem to fit here).
Fixed as suggested and velocity text eliminated.

p. 17, end of top paragraph, two points.

(1) The historic studies of convective cores, being based primarily on data below the freezing level, probably include more "weak" storms than the present paper, which uses criteria based on strength above the freezing level.

Addressed this in introduction.

(2) LeMone et al. (1994, JAS) discuss the effect of filtering (cutoff frequency 1 km) on convective cores and find the effect to be detectable but small.

Added this reference. That paper suggested a cutoff frequency of 0.5 km. The radar beam that provides the main filtering in the current study filters on the 0.5-1 km scale.

Discussion of Figure 11. Isn't this precisely what you said it was difficult to do? I'm wondering if it might be more useful to put the caveats you list on p. 16 into a discussion of the profiles, to remind the reader that the profiles are for separate events, and thus cannot necessarily be interpreted as profiles of a "typical core." Just a suggestion.
Added sentence here: "Each profile is from a different case in Table 2 and the group of profiles does not represent a "typical profile" but rather a variety of different events"

p. 17, line 1. Suggest "Vertical profiles of peak reflectivity, and peak updraft and downdraft magnitudes, sorted by convection category. For example, the rightmost curves …." or something like that.
This was changed along the lines of suggestion.

p. 18, first paragraph, explanation of values being higher. Assume you mean higher than Szoke et al.? If so, in line 5, should insert "than in Szoke et al. (1986)" after "higher reflectivity."
Added this.

Also, there are at least two other important reasons why the Szoke et al. values were lower:

(1) The Szoke et al. study screened storms according to near-surface reflectivity, which probably included several storms that were weaker than those in this dataset.

(2) Fig. 12 in Szoke et al. plots mean reflectivity rather than peak reflectivity (at least this is what is suggested by the caption).

A few sentences added on this since both your suggestions are correct. Forgot to include the second one for sure.

p. 18, bottom line. As pointed out in the introduction (p. 4, top), storms vary considerably in structure, intensity, and longevity, which could lead to different peak updrafts (or different probabilities for sampling strong updrafts). The characteristics of storms are a strong function of the environment (CAPE, vertical shear of the horizontal wind, type of hodograph), as outlined in, going back in time: Johnson et al. (2005, MWR), LeMone et al. (1998, JAS), Alexander and Young (1992, MWR), Barnes and Sieckman (1984, MWR). This could be a source of the variability as well as different parts in the storm's lifetime, which is of course also important.

Absolutely. There are lots of references on this but the cases in this paper are not generally convective lines. Nevertheless, the latest paper by Johnson et al. is referenced in the Introduction.

p. 19, top paragraph. Were the suspicious data included in the averages? If so, did they strongly influence the average?

Suspicious data was averaged but it does not appear to affect the mean very much when the entire profile was removed. A qualifier was added in the text.

p. 19, Section c. Should be Fig. 14 not Fig. 13. I like this figure. But it would have been useful to point out the association with peak updraft speed earlier, which was quite obvious from the graphs.

Figure number fixed. This association was mentioned earlier.

p. 19, Section c, middle. Again, in addition to the age of the cloud, the structure of the cloud is important as well (a function of the environment, as pointed out earlier in this review). The convection in LBA, which had weaker "maritime" convection as well as the stronger convection (V, W, etc.) is an example of this.

Yes, both environmental conditions and life cycle mentioned earlier in the paper. Fixed.

p. 19, bottom. Consistent nomenclature? How about the four "categories" of convection. Also the sentence is redundant. Could just write after Fig. 14 "for the four convection categories."

Sentence was restructured.

p. 20. Fierro et al. (2009, JAS) used a cloud model, which is described in Straka and Mansell (2005), not WRF.

Straka, J.M., and E.R. Mansell, 2005: A Bulk Microphysics Parameterization with Multiple Ice Precipitation Categories. *J. Appl. Meteor.*, **44**, 445–466.

Removed WRF and added cloud model without referencing Straka's paper since the references are getting long.

p. 20, first paragraph, line -2. Latent heat of freezing.
Fixed.

p. 20, lines -5 to -4. Please explain more clearly. How can particles sediment out of large downdrafts.   Are the updrafts tilted? (i.e., do you "count" the edges of the updraft as part of the updraft)? Is there mixing, which reduces vertical momentum so that the particles can fall?
All of these are possible. Tilted updrafts or mixing near cloud edge are both possible. This sentence is improved.

p. 22, line 7. The reflectivity profiles *confirmed* .. I think Szoke et al. were the first to show this, although there could have been earlier results. (Although I don't think so. Prior to GATE, it was thought that the oceanic convection would much stronger than it turned out to be.)
A reference to Szoke et al. was included here and mention is made that the current results confirm some of the earlier results.

p. 22, line -4. Also, as pointed out earlier, the evolution of the convection and hence the strength of the updrafts and downdrafts probably relate to structure as well.
Added "evolution" and cleaned up sentence.

Appendix. This was extremely difficult for me to read. It needs to be shortened, and just deal with how the reflectivity-fallspeed relationship was found. Once this is described, it could be compared to earlier results.
The Appendix writing has been improved.

Other potentially-useful historical references, for TOGA-COARE case studies. These of course represent considerable smoothing, so you will not get the extreme values of updraft speed – however, the maximum vertical velocities are observed well above the freezing level.

Roux, F., 1998: The oceanic mesoscale convective system observed with airborne Doppler radars on 9 February 1998 during TOGA-COARE: Structure, Evolution, and Budgets. *Q. J. Roy. Meteor. Soc,* **124**, 585-614. Figure 8, and possibly elsewhere.

And

Jorgensen, D. P., M. A. LeMone, and S. B. Trier, 1997: Structure and evolution of the 22 February 1993 TOGA-COARE squall line: aircraft observations of structure, circulation, and surface energy fluxes. *J. Atmos. Sci.*, **54**, 1961-1985. Figure 7, and possibly elsewhere.
We looked for a good reference such as Jorgensen et al. observations of a double peaked updraft structure and peak updrafts aloft. This paper is now referenced and used in the Introduction and Conclusions.

REVIEW C

Review of “Characteristics of Deep Convection from Nadir-Viewing High-Altitude Airborne Doppler Radar”

By Heymsfield et al.

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General Comments.

The focus of most of your comments is on the microphysics that we are greatly interested in and appreciative of you comments. We have tried to keep the focus of the paper on a vertical motion discussion but it is certainly difficult not to discuss microphysics. We have addressed your concerns on assumptions of graupel fallspeeds rather than frozen drops by performing a simple sensitivity test using larger fallspeeds then assumed. This did not seem to have much impact on the overall conclusions in the paper. Our estimates of vertical velocity may at times be biased low since our fallspeed corrections may be on the low side.

This is a worthwhile study that is appropriate for this journal. It provides new and unique data on vertical winds in convective storms that will be of interest. I have a few relatively minor, but possibly important, comments that should be considered before publication. I recommend acceptance after minor revisions.

More attention should be paid in the paper to describing the bias in the data. Although this set covers several regions it is heavily weighted towards tropical and subtropical oceanic-style convection. For example the time periods included from the “land” LBA period are during the “green ocean period”, when many of the updraft characteristics were similar to those from oceanic storms (see Anderson et al); this is not surprising because the vertical stability and even the CCN are similar to oceanic cases. If periods from the dry season in Amazonia were included significantly different results might have been obtained. Many of the other data sets are near costal areas and heavily influenced by the ocean (e.g. Florida).

We never claimed in the paper that we are looking at an unbiased sample but rather an assemblage of events from different regions. We are mainly trying to look at the range of convective updraft intensities from the cases we have. I would argue (as well as alluded to in Reviewer B’s comments) that many of the previous studies had biases. We’re trying to make general comparisons between the cases we have to see whether there are systematic differences between convection in different regions. I hope we’ve conveyed this point in the paper.

The major limitation of this paper is that there is considerable scatter in the data (e.g. Figs. 7-13). Although there are differences in mean values, it is not clear that these differences are statistically significant. Thus some of the statements about the differences in updraft characteristics seem out of place unless it can be shown that these differences are not due to chance.

These observed scatter is due to a combination of differences in environmental conditions between the regions, and storm lifetime. We are not as concerned about statistical significance of the comparisons as we are with the magnitudes and heights of updrafts and reflectivity in the different regions. It's remarkable that there are not larger scatter in the observed convective events between the various land and ocean regions. We've trytried to clean up discussions related to this in the paper.

*Specific Comments. *

Title. The title should indicate "Characteristics of Deep /Tropical and Sub-tropical/ Convection from Nadir-viewing…."
Changed. This was in the original title but dropped for the first submission.

Appendix. It would be helpful to point out to the reader the likelihood that particles might be misclassified and how this might affect the results. Graupel, for example may be hard to identify, because it often exists mixed with or in close proximity to unrimed particles. Also, the top of page 26 starts "Calculations were made based upon Mie spheres…" What calculations? Reflectivity? (Later you talk about Radar reflectivity calculated using Bohren and Huffman, are these different calculations? This section is confusing.)

This section is cleaned up. The "Calculations.. " sentence was improved. These calculations and relations for fallspeed are the best we can do with the current observations. They are significantly better than what has previously been used in vertical Doppler measurements. About all we can do is point out error bars in the derived vertical velocities. As we point out, these will not affect the conclusions of the paper.

Page 12. "Figure 12 resembles a supercell-like tower" How so? I don't see a weak echo region. This seems quite different from a supercell, which forms in a heavily sheared environment (which is not likely here). Although there are moderately high reflectivities aloft, the highest reflectivity is in the lower regions of the storm, below the main updraft. This seems different from what would be expected for hail growth. A more likely explanation for these observations may be the presence of large frozen raindrops, which have been found in previous LBA cases (e.g. Stith et al. 2004, Fig. 11) and seem likely to be present in large numbers for this case, due to the active warm rain process that is evident in the lower regions of this storm and the fact that the updrafts are strong enough to loft raindrops to higher altitudes.
We replaced "supercell" with "intense". Frozen drops such as discussed in Stith et al. (2004) are certainly possible. Stith et al. discussed large frozen drops but I found that none of these were larger than 1 mm (Fig. 11 suggests a particle that may be slightly larger than 1 mm). We examined the low level EDOP vertical velocities and deduced

that the low level updrafts (below the melting layer) rarely exceeded 5-7 m/s and would therefore only support 1 mm raindrops. So we would not expect larger frozen drop. More likely, the suggestion in Stith et al. that frozen 1 mm drops act as embryos for graupel formation. It is certainly possible that larger frozen drops do make it aloft, but this is probably not a majority of the cases. If frozen drops were present, the EDOP-derived updrafts would be higher because the fallspeed is underestimated with graupel.

We attempted to examine the uncertainty in the EDOP-derived vertical velocity for the case where we underestimate fallspeeds, such as if frozen drops rather than graupel were present. We assumed rain fallspeeds instead of graupel fallspeeds since these will provide a rough upper bound for 1 mm frozen drops (Fig. A2). The resulting plots were compared to what we have in the paper that were calculated with graupel. As expected: The peak fallspeeds were typically 2-5 m/s higher assuming rain fallspeeds, the height of the updraft maxima were within a few tenths of a kilometer, and the downdrafts were affected less than 1 m/s. We therefore conclude that the larger fallspeeds associated with freezing rain will have a minor effect on the overall results in the paper. Our vertical velocity estimates are on the conservative site. A few sentences are added in main body of the text and the Appendix.

Page 13. Figure 5 appears to me to have a maximum downdraft of 18 m/s, not 20 m/s.
Fixed.

Page 15. Discussion of supercooled raindrops and decrease in reflectivity between the ice and water phase. It would be helpful to expand this section somewhat, as this is an important part of the paper.
We included a reference to Stith et. al. (2004) on the freezing of raindrops and a reference on the reflectivity change from liquid to ice phase Smith (1984). I think we'd be speculating too much if we said a whole lot more on the microphysics without insitu measurements. There are not many in situ observations in intense convective cores.

Page 16. "This level is examined since it is near the -40 C altitude and it is near the base of the strongest convection." It sounds like you mean the cloud base, but that cannot be right. Clarification needed. If there is a strong correlation between the updraft gradient and the location where homogenous freezing takes place that would be important to point out.

This has been reworded. "This altitude is examined since it is near the -40°C level and generally below the strongest updrafts. "

Page 19. Figure 13 should read Figure 14, I think.
Fixed.

Page 20. Discussion. See comment about statistical significance. It would be helpful to discuss possible causes for the observations. It appears that freezing of ice is the major factor causing stronger updrafts in the upper portions of these clouds. Why are the downdrafts strongest there? It is also worth pointing out that the updraft characteristics of these warm-based clouds are likely to be much different than those in mid latitude or continental storms, of which there are few similar observations. I hope the authors have a chance someday to make similar measurements in higher latitude and continental storms, so that a more complete picture of convective drafts can be developed.

We can speculate on why the downdrafts are strongest at upper portions, but a more definitive study would require some numerical modeling to examine dynamical processes. We have referenced possible explanations for upper level downdrafts in previous work (Heymsfield and Schotz 1985, Sun et al. (1994), etc. in the Introduction. Also, a few sentences have been added on downdrafts in Section 3. A few sentences are added in the conclusion about hoping to study higher latitude continental storms - this may be a possibility in 2011.

Page 29. Reference. Bansemer is misspelled in the 2009 reference.
Fixed

Figure 5. Purple is listed twice, and I don't see that color in the figure. Better selection of colors is needed.
The caption should have read turquoise.

Figure 7 The units of height appear to be incorrectly labeled as m/s.
Fixed.