

Response to Referee 1

The authors would like to thank the referee for his or her suggestions that resulted in a much shorter and more logically written manuscript. The manuscript is now more succinct and better focused. We also believe the main messages come across more easily as a result. There is a better focus on the $2/3$ scaling which arises more naturally from the log-log plots the referee suggested.

Summary: The authors present length scales of deep convective clouds and their anvils, including relationships between cloud components, derived from cloud objects identified in CloudSat observations from a database developed in Igel et al. (2014). Anvil width is found to be related to pedestal width to the power $2/3$. The authors argue that this relationship can be founded on simple geometric considerations. The authors relate their results to theoretical considerations of deep convective clouds and support their claims with simulations of tropical convection with RAMS under Radiative Convective Equilibrium (RCE).

General comments: The paper is generally in good shape, with an interesting result ($2/3$ power law) and novel use of CloudSat data to investigate deep convective anvils, which is an area of long-standing and active research. However, the science and the paper suffer from lengthy excursions into hypothetical situations and repetitive statements, which confuse the key messages of this paper. I therefore recommend this paper to be reconsidered after major revisions.

Major Comments

1. Choice of data — By using an existing database (IDV14), the authors are restricted to CloudSat data only. Nevertheless, collocated radar-lidar data are available, and would provide a more complete overview of the anvil characteristics. In particular, higher cloud-tops are observed by the CALIPSO lidar and anvil might also extend further horizontally than observed by CloudSat. The authors should consider the limitations of the CloudSat radar in detecting thin anvil and explain how their results may be affected if lidar data were to be included. For instance, Machado and Rossow (1993) mention differences in optical thickness of anvils between MCSs and more "typical" deep convection.

When generating the database, we had initially included data from the CALIPSO lidar. However, after experimenting with that data, it became apparent that the lidar introduced several practical problems. The lidar often successfully recognized very optically thin upper level clouds (many times in a layer above what we would ultimately deem "cloud top") that CloudSat did not. Since we rely on pixel-connectedness to computationally define individual clouds, oftentimes this resulted in obviously discontinuous objects being aggregated by the computer through artificial connections between optically thin upper level clouds. Ultimately, we decided this was undesirable and not in keeping with our intentions for the database. So the referee is correct in assuming precisely what we measure as "cloud" is limited by the exclusion of the lidar, but we would argue that this results in a logical consistency in our database that would not be attained with its inclusion. We have added a statement to the manuscript to remind readers that we are limited by the sensitivity of CloudSat but that this is logically consistent with our intention that reads, "CloudSat is sensitive to cloud droplet-sized and larger hydrometeors, and as such, in this paper, "cloud" should be taken to mean the subset of cloudiness that is observable by CloudSat."

2. Mass flux considerations — Scattered throughout the paper are hypotheses and thought experiments on how mass flux considerations may lead to the $2/3$ power law relationship. In particular p.15986, line 20 onwards; and p. 15989, line 14 onwards, should be combined. It would be beneficial to the reader to present these as a coherent discussion, possibly after the RCE simulations, which contain actual results on dynamics of convection. Furthermore, the authors should remove the consideration of a "spherical" anvil, as this is impossible by the statistics presented previously: even at the small pedestal end of 6km, minimum anvil width has to be 9km by definition, meaning that a spherical anvil already reaches typical convective storm height presented in the paper and the anvil would have no room to grow spherically. A cylindrical anvil (with fixed depth) is an easier geometric shape to consider. Finally, pedestals contain cores and stratiform precipitating regions, the latter less likely to contribute to the upward mass flux. Although pedestal width appears related to the number of convective cores, in the mass flux consideration, the authors should investigate relationships between convective cores and anvil width or at least explain why they don't consider this.

At referee 2's suggestion, much of the simple discussion occurring on p. 15986 has been eliminated. Because other discussion has also been eliminated, the authors feel that the concern the referee expresses here of too-frequent verbal excursions into thought experiments should now be mostly satisfied without our moving to a Results-then-Discussion format.

The authors agree with the referee that the spherical anvil is somewhat unphysical. We have opted to include the discussion of a spherical anvil because we believe it helps the reader make the association between the observed statistical $2/3$ relationship and the mathematical one which involves length scales to various powers. In Equation (5) which conceptually uses the sphere, the $2/3$ power naturally arises. This is easy for the reader to see. In Equation (6) which uses the ellipsoid, the $2/3$ power is somewhat obscure. We also appreciate the referee's request for a simple use of a cylindrical anvil. This is something we have previously investigated. However, the cylindrical anvil with fixed depth introduces some logical inconsistencies at the genesis phase of the anvil that the ellipsoid does not. The sphere (ellipsoid) allows for growth from zero-anvil-volume to arise naturally as all length scales can mutually grow from zero. The cylindrical anvil does not. And, our data does suggest a dependence of anvil depth on object width (section 3.2).

The authors also agree in principle with the referee on his or her final point. However, at referee 2's request, we have removed much of the high-level discussion of convective core number. The discussion of convective cores is now limited to secondary discussion. Therefore, it would be inconsistent with the rest of the manuscript to add discussion of the relationship of convective cores and anvil width. That being said, we examined the relationship between core number and anvil width for the purposes on this response. In the mean, core number depends almost linearly on pedestal width. So, the relationship between core number and anvil width tends to scale with the same $2/3$ power discussed in the manuscript.

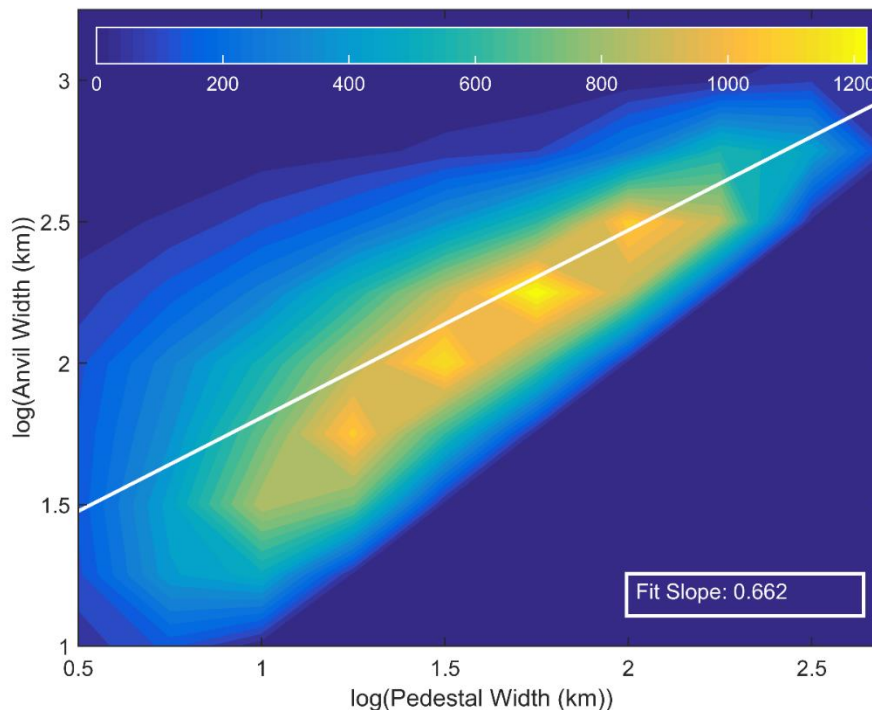
3. Superfluous figures/results — Figures 2, 3, and 4 contain the same information, with no significant new insight offered. If Figure 2 were presented on $\log(\text{pedestal width})$ versus $\log(\text{anvil width})$, the $2/3$ power law should be immediately apparent. The authors should thus ignore the linear fit, which bears no significance for the rest of the paper. Scatter of data points is obvious from the 2-D histogram as well, so Figures 2a, 3a, and 3b should be reduced to a single panel. Although Figure 2b contains the same data points, the representation is useful and the authors should again consider a $\log(\text{pedestal width})$ scale and include the fit discussed in the text (equation 2). The authors should consider showing 1-core, 2-

core, 3-core objects separately in Figure 3b. Following the values in the table, the 2/3 power law should not be obvious from 1-core objects, as these reach only just beyond 100km anvil width. This should not affect the results, but will be easier to interpret in relation to Table 1 and Table 2. This Figure would also be easier to interpret on log(pedestal width) versus log(anvil width), as the values (in km) can be related to physical values in the Tables, rather than mental arithmetic. With these changes, Section 3.2 will be a lot more focussed and the key message (2/3 power law, monotonic decreasing width ratio) will be clear.

All of the figures used to discuss the 2/3 scaling have been changed to log-log scales. The discussion relating to these figures has been changed to reflect the new figures and their implications. An example figure is below.

While the authors continue to believe that for many readers there is some value in the development of the 2/3 relationship as it was previously laid out, we have done our best to implement the suggestion of the referee. Figures 2a and 3a have now been eliminated as has the discussion of the linear fit to the scattered data (except for a brief mention when we discuss fit statistics). We agree with the referee's assessment that this provides a much more focused analysis within this discussion.

We appreciate the referee's suggestion of adding specific figures to show trends with 1, 2, and 3 cores. Given Referee 2's concerns about the accuracy of counting cores, we cannot add more discussion of core number. Since Table 1 has now been eliminated as a result of those concerns and the manuscript has been significantly restructured, referee 1's suggestion is no longer applicable.



This figure replaces the original Fig. 3. The axes are now in log-log space and the best fit in log-log space is in the white line with the slope (approximately 2/3) included in the box.

Minor Comments

1. 15980, line 18-22: Repeats p.15979, line 16-29.

These paragraphs have been changed to emphasize their different purposes in the paper. We intend for the first paragraph to introduce the history of these campaigns broadly and for the second paragraph to introduce the specific limitations (and benefits) of ground-based field campaigns that do not pertain to CloudSat.

2. 15980, line 26-15981, line 5: "The result of Igel et al... (Zhang et al. 2013)."Unnecessary detail.

This has been shortened to eliminate the superfluous discussion.

3. 15982, line 6-7: What granule issues? Why throw out half the data if only a few granules are affected? Or is this a widely reported issue with CloudSat data? If so, please provide a reference.

We deleted the mention of "granule issues" as they are not contextualized in this manuscript. The daytime only data is in keeping with the goals of IDV14. The "granule issues" are rare and insignificant to most analysis methods. They crop up as missing columns at the end of CloudSat data granules. These missing columns could severely bias our dataset by artificially shrinking cloud objects that occur near the equator at night.

4. 15982, line 24: Instead of "accuracy", perhaps "consistency with their results".

This line has shortened to read, "[i]n reality, these are just local maxima-minima pairs in reflectivity that may or may not have an active updraft" so as not to overstate our ability to count cores.

5. 15982, line 24: "Reflectivity maxima": Important nuance here, as ground-based (rainfall) radar detect far higher reflectivity values than CloudSat's cloud radar. Is there any evidence that W-band radar reflectivity maxima can mark the position of updrafts?

The authors feel they did not sufficiently explain how convective cores are counted in this manuscript. While a full description is available in IDV14, the information provided in this manuscript is obviously insufficient to explain our method. We have improved our description of our attempts to count "cores." We should also note here that in the paper we now mention that we only attempt to count the number of cores in the simplest sense; we do not attempt to truly locate the position of cores, only their number. But these concerns should now be limited by our significant reduction to the content concerning cores in the new version of the manuscript.

6. 15983, line 13-24: Unnecessary detail. This is obvious when the results are being discussed.

This paragraph has been eliminated.

7. 15984, line 11: "Large-scale structures", but these are still limited in dimension to 200km?

The referee asks a somewhat philosophical question we cannot answer quantitatively. Yes, any symmetric structure will necessarily be less than 200km. But the 3000km length scale also allows for

asymmetric flow than is larger than 200km. We try to make sure we focus our analysis on scales along the long dimension. But how much the long-dimension structures are affected by the “smallness” of the short dimension, we cannot say.

8. 15984, line 20: "Figure 2" Figure 1?

Yes, the referee is correct. This has been changed.

9. 15984, line 22-24: "Fig 1 illustrates... reaching a certain size." Three interpretations are confusing the message. "Fig 1 illustrates the cloud fraction as a function of distance to object center." Or something similarly concise would be easier to interpret.

We have removed the redundancy.

10. 15985, line 8-9: "although this value... order of 1km". Comparing two very different concepts, pedestal and updraft. Either remove this or provide more nuance to this statement.

We have added the suggested nuance such that the clause now reads, “although this value dwarfs the measured updraft widths of LeMone and Zipser (1980) which tended to be on the order of 1km -- reminding us that the updraft is just part of the pedestal.”

11. 15985, line 11-13: "given that... 67% of the domain." Unnecessary detail.

The authors disagree that this detail is unnecessary. It is not our intention to belittle ground-based radar studies of clouds – these are incredibly valuable studies – but rather we intend for this to serve as motivation for why our results add something substantive to the discussion of cloud size.

12. 15986, line 9-10: "mid-troposphere", "lower troposphere". This is not obvious from the Figures at all. Is there a chance that this difference is simply due to more variability in pedestal characteristics for the 2+ core objects?

The referee provides a logical interpretation. We have edited this paragraph to tone down the implications of such a subtle difference in the composite structure. It now reads: The composite image of cloud objects with pedestal widths larger than 50km (Fig. 1c) is much wider than either of the composites above in panels a and b and appears to exhibit structural characteristics consistent with MCSs (Houze, 2004). It has a wide and deep anvil across a broad range of percentiles unlike the composites of the clouds from the smaller categories. The highest overlap percent occurs higher in the wider-cloud composite than in the narrower-cloud composites but with a lesser magnitude. This result shows, perhaps unsurprisingly, that there is higher variability in the pedestal structure for wider clouds.

13. 15986, line 17: What is this difference in the population? It is not obvious from the Figures or the Tables.

We eliminated mention of the different population of anvil cutoffs. It was inconsequential to the discussion. The mean and median statistics for the 3+ core cutoff height were similar to the other clouds, but some of the higher order statistics were different enough (and there is a hint of that in the

percentile values shown in Table 1) and the population was large enough that a t-test could indicate statistical separation. Regardless, this was unnecessary discussion.

14. 15986, line 28-29: "clouds flux mass" - "flux" as a verb has a different meaning, please replace it throughout the text. Use "transport" or another synonym.

"Flux" has been replaced where it was previously used a verb.

15. 15991, line 15: "exhibit weak gradients" What does this mean?

This discussion has been eliminated thanks to the new version of Fig. 4.

16. 15991, line 20: "These results" Which results?

This, too, has been made clearer. "These results..." has been replaced by "The implied non-linear relationship..."

17. 15992, line 10-11: EarthCARE could be mentioned here for reference to future missions.

The relevant sentence now reads: There is currently no way to examine explicitly such responses to convection from any current satellite since no earth observation platform provides any direct measure of air velocities above the surface level either in cloud or clear air, although the promise of future satellite missions, such as EarthCARE (Illingsworth et al., 2015), suggest this may one day be a capability.

18. 15992, line 27: "higher topped anvils are horizontally wider" There are many results in literature that confirm this, e.g. Machado and Rossow (1993). Some context is warranted.

We have added to the discussion of previous similar results following the discussion of what we see.

19. 15993, line 13-18: Any comments on how model configuration (microphysics, mixing, biases) could affect anvil characteristics?

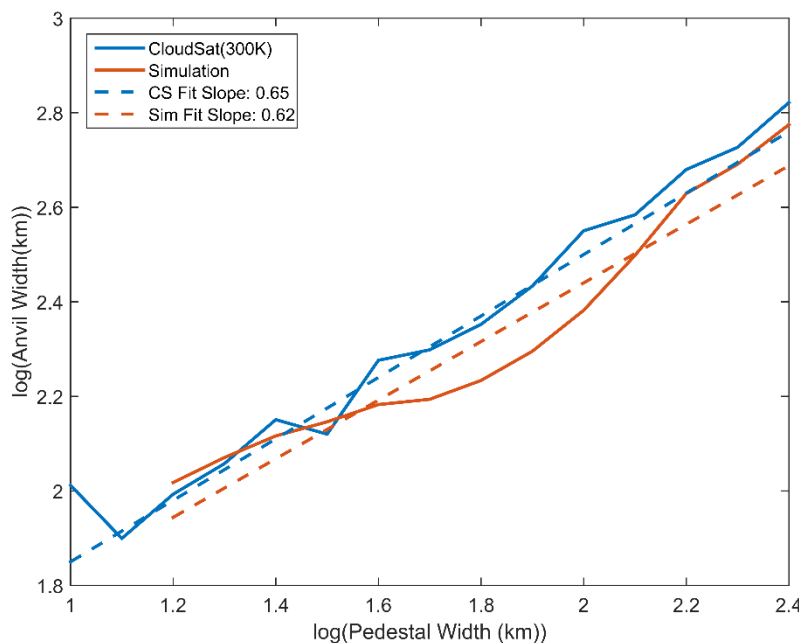
We cannot do more than postulate how our setup affected anvils. It has been shown that anvil size can depend on microphysical scheme (e.g. Adams-Selin et al 2013, Varble et al 2014). Generally cloud resolving models have a hard time accurately simulating some situations resulting in moderate to light rain rates from tropical systems. This bias could be resulting from biases in anvil microphysical characteristics, there might be evidence for poorly constrained simulation of anvil clouds. RCE simulations tend to be rather dry outside of actively convecting regions which could limit anvil width. These general biases are only consequential if they depends on pedestal width in some nonlinear way. We do not see how this would be the case *per se*. A note has been added regarding the possibility of too-narrow anvils as seen in Varble et al (2014) from two-moment microphysics simulations.

20. 15993: How many objects were identified in the RCM simulation?

We have ~10,000 objects in the database from the simulation. This number is difficult to contextualize, though, in comparison to the number of CloudSat objects since we likely to be observing the same cloud in the simulation multiple times which is essentially impossible from CloudSat. We have noted this in section 3.4.

21. 15994, line 10: Could the observed trend be shown in Figure 6? It would be useful to have Figure 6 on the same scales as Figure 4 for comparison.

We have added a limited subset of CloudSat results to Fig. 6 by including the near-300K SST CloudSat data in the figure to aid the kind of comparison the referee points out and better homogenized the binning strategy between the CloudSat data and the simulation-derived cloud objects which had previously been slightly differently defined. Data bins are now exactly the same between the cloud object and the simulation data. It also became apparent when reanalyzing the data that previously, the detrainment index requirement had inadvertently been applied inconsistently to the model data for this figure. Because the detrainment index threshold had not been applied uniformly, the largest objects often had anvils that were too narrow to be allowed. We have rectified this minor coding issue. It brings the slope of the simulation cloud objects into better agreement with the limited subset of CloudSat data. Anvil widths still tend to be narrower in the model cloud objects than those from CloudSat. Also, our improved computation of the simulation-mean has shown the model results to be slightly less linear than in the original submission. We have added a brief discussion of why this might be to the manuscript: often, models have a difficult time simulating expansive enough upper level ice clouds (e.g. Varble et al 2014). The new version of Fig. 6 is included below.



22. 15995, line 15: "accumulated acceleration" What does this mean?

The referenced line has been removed at referee 2's request.

23. 15997, line 19: "cloud base height drops" This result is not presented in this paper.

The referee is correct. We had failed to remove this phrase after removing a results section from the manuscript.

24. (general) This may be my printed version, but several equations are not typeset correctly, namely 4 and 5, as well as the equations in the appendices.

We appreciate the referee's noting this. They appear correctly typeset in the official version we have from ACPD. We will ensure that any final typesetting is appropriate. We did change how the final equation in Appendix B is presented so it is clearer, but we doubt this is the issue the referee was raising.