Response to Referee 2

First, we would like to thank Referee 2 for his or her thoughtful consideration of our discussion paper. We also appreciate the suggestions provided for cutting down on the level of speculation in the paper. We employed many of these suggestions and the manuscript has been improved as a result.

Responses to Major Comments

1. In the introduction, you mention the shortcomings of field campaigns in studying the spatial scales of the global population of tropical oceanic convective systems, but seem to assume that CloudSat is the best tool for this type of study. This ignores the contributions and/or abilities of geostationary satellites, long-term ground radar datasets (e.g., Darwin, Guam, Kwajalein, etc.) in the tropics, and polar-orbiting satellites such as TRMM to studying tropical, oceanic, deep convective morphology. The real advantage of CloudSat is its ability to observe anvil vertical structure connected to convective regions or the upper portions of deep convection. Other tools such as TRMM are better tasked with studying the spatial scales of convective cores or any moderate or intense low level rainfall in a deep convective system because the TRMM PR is far less attenuated in moderate to intense precipitation and it has a wider swath, which gives cross-track context. TRMM or ground radar observations combined with infrared satellite imagery also allow correlation of convective core/pedestal width and anvil horizontal spatial scales. Therefore, I think you need to better describe why CloudSat is the best tool for the questions that you are trying to answer and why other tools are neglected.

We have added a brief description of why using CloudSat presents a unique combination of benefits for this study toward the end of section 2. We also believe that the shortcomings of other platforms as they relate to our goals and the benefits of CloudSat are already presented in the introduction and broadly throughout the manuscript with the exception of TRMM. Collocated TRMM and IR could present a future method of corroboration of our study insofar as measurement characteristics will allow. Certainly the existing TRMM "cloud object" methods have proven extremely useful across a wide range of convective studies. However, one of our concerns was with the PR footprint size. Since we really wanted to study the entire population of deep convective objects from the largest to the smallest, CloudSat's higher horizontal resolution presented an attractive alternative. Also, by using a single instrument with a consistent "curtain" of data, we are able to ensure connectedness of pedestal and anvil (if not a causal connection). Ground-based data have proven extremely useful in the past, but as we discuss in the manuscript our goal is to examine results from a variety of spatial maritime locations across the tropics.

2. It seems that you are equating pedestal width with convective core width, but pedestals also contain stratiform rainfall. Even with relatively small, isolated convective systems, it is common for convective cores to decay to stratiform rain with anvil attached. These systems can still easily produce peak reflectivities that may not look much different than active convection by CloudSat since W-band frequencies experience Mie scattering for typical raindrop sizes. Furthermore, some people might interpret convective core to mean convective updraft whereas others might interpret it to mean convective precipitation, which itself has many definitions in the literature. Even with reflectivity detected below 1 km and above 11 km in a feature, the entire feature could be stratiform with no convective cores because you are only looking at a curtain (vertical cross-section) view through systems, but the way you define cores would almost certainly assign some cores to these systems.

Furthermore, I would argue that you are not seeing a significant portion of convective rainfall, even for tropical oceanic systems, because the CloudSat signal becomes nearly completely attenuated at low-mid levels in true deep convective cores, even in systems without significant graupel or lightning that are typical of the pure oceanic tropics. This can clearly be seen in the attached Figure 1 showing an MCS from the central Pacific ITCZ observed by CloudSat where low level reflectivity is anti-correlated with upper level reflectivity. When this happens, you've essentially split one convective core into two cores, neither of which is actually the convective core. Even if the signal is only partially attenuated, how can you be confident that you are differentiating between actual cores with so much attenuation? Even deep stratiform regions in tropical, oceanic MCSs exhibit significant attenuation (or example, see Figure 2 which has a large stratiform region with significant attenuation and no discernable convection embedded in the stratiform precipitation). Unless you can show using TRMM (where TRMM and CloudSat observations are co-located) that CloudSat actually differentiates between different convective cores (or even rainfall cores) at low-mid levels, which I don't think it can, I would remove the analysis related to number of convective cores. I don't think that it would affect the primary conclusions of this manuscript relating various anvil and pedestal scales, and simulation output would be much more valuable in showing relationships between number of convective cores, updraft mass flux, pedestal width, and anvil scales that support some of your hypotheses concerning scale relationships.

I know you published information about the convective cores in Igel et al. (2014) already, but I have the same issues with that publication. Even your example in Figure 5a in Igel et al. (2014) looks like it is suffering from significant attenuation. I have no confidence that the separated low level echoes in that Figure 5a in Igel et al. (2014) are separate convective cores or even separate rainfall cores. An upward looking W-band radar at the surface would likely show much more continuous low level echoes for that entire system with maybe a convective core or two where low level echoes are most strongly attenuated.

The referee is correct in stating that for the purposes of our flux arguments we equate the pedestal width to the width relevant for an assessment of the broadness of dynamical ascent within the cloud. This does not equate precisely as we have now noted in the manuscript at the end of the section regarding the 2/3 relationship in the manner the referee suggests.

Regarding the referee's concern about our ability to count cores, we cannot disagree in principle. Attenuation is undoubtedly a concern, a point we clearly stated in Igel et al (2014). However, we feel that attenuation's significance to precisely what we attempt to do (suggest a likely approximate number of cores) should not be overstated. We did attempt, as the reviewer suggested, to use collocated TRMM and CloudSat data to perform some validation of our approach, but given our detection strategy which is highly restrictive, as well as the orbital characteristics of both instruments, collocated data were extremely rare. When collocated objects were found, the lower resolution of TRMM tended to smear out CloudSat "cores" and counting them objectively would have required the development of a whole new methodology. So, given the referee's concerns, we have limited the discussion of cores significantly in the new manuscript. Cores are no longer a basis of analysis; they have been limited to secondary results that aid discussion. Cores are no longer used to define the "average cloud." In our rewrite we have limited the discussion of cores to those included in the analysis of what was originally Table II in which we show the mean size of clouds based on pedestal width thresholds and to define our sub-population of cloud objects in Fig. 4 which is only used as a simple check of the robustness of Fig. 3 and not to introduce new science. That being said, the authors would

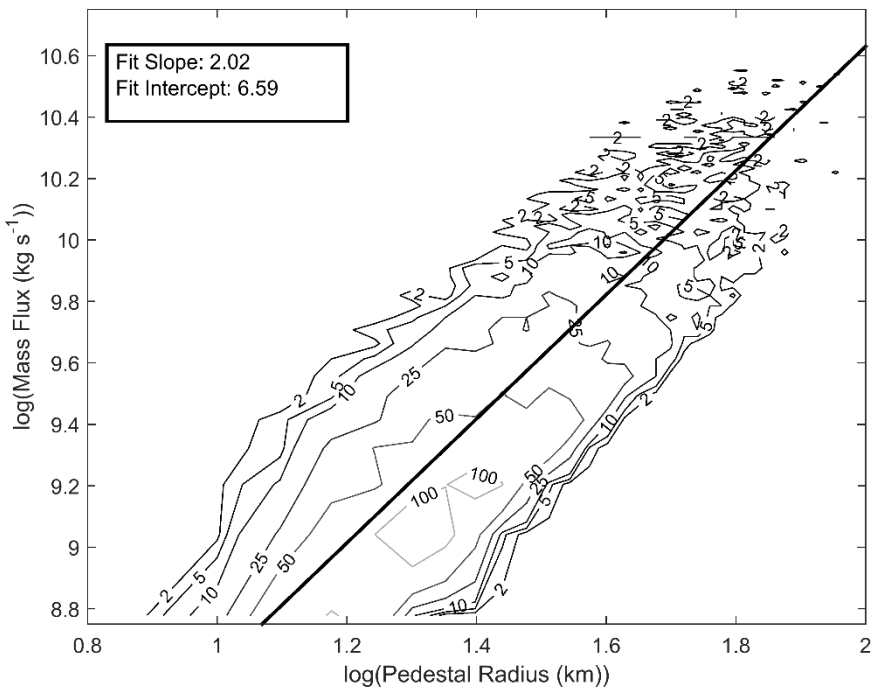
like to express that they still believe core counting, in the manner conducted, to be a useful exercise in general. Core counting in IDV14 represents a general attempt to better understand the in-cloud properties of deep convection from global observations which we believe should be a focus of more specific future research.

3. At the end of Section 3.1, I don't completely follow your argument regarding single cell preconditioning for multi-cell systems through moistening. Again, your pedestals are not necessarily convective cores, your convective cores are not necessarily convective updrafts or convective precipitation, and as system size increases, the fraction of your pedestal that is stratiform rain also likely increases, so it is not surprising that the ratio of anvil width to pedestal width decreases with increasing pedestal width and core number. In fact, stratiform rain can be thought of as "anvil" since well-developed stratiform regions commonly have cloud bases around the melting level. Furthermore, regimes are commonly observed across the tropics with single cell deep convection that never transitions to multi-cell convective systems because many other factors such as large-scale forcing, cold pools, vertical wind shear, and more also matter. I'm sure you know this, but my point is that you need to present much more evidence than you do to support your speculation of single cell moistening that supports multi-cell formation. An equally valid speculative explanation would be that larger systems, however they are forced, tend to develop in moister environments, which can moisten by several mechanisms that are not necessarily related to convection (e.g., large-scale upward motion, advection), that allow stratiform precipitation to more easily grow in scale.

This discussion has been eliminated in keeping with convective core concern as well as the more general concern regarding some of the speculative elements of the paper.

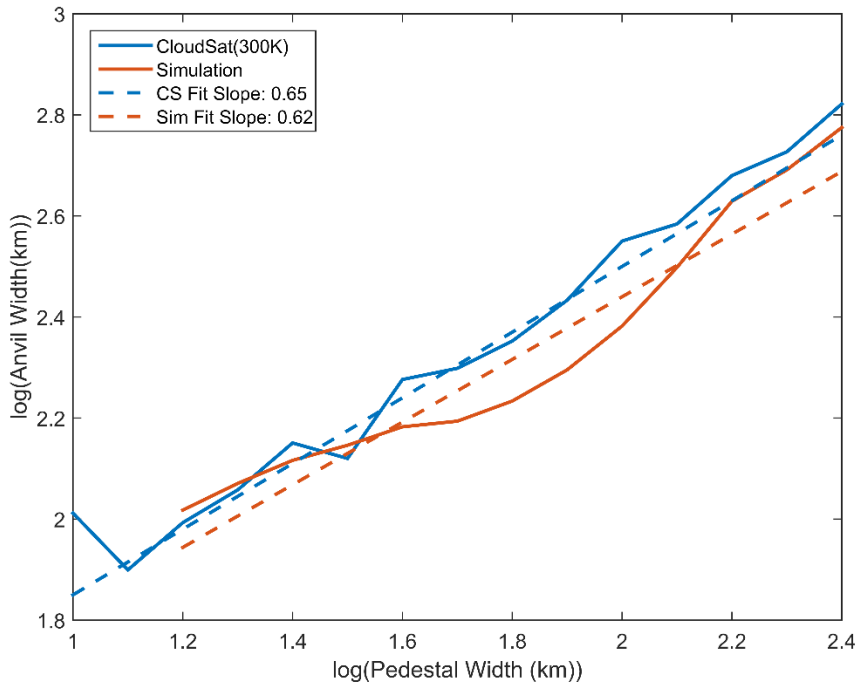
4. There are other considerations for anvil width to pedestal width ratio as well. As an anvil grows away from the convective core, ice will either grow and sediment or sublimate, so the farther it is removed from the convective core, the more likely that it is to have sedimented out or sublimated. Furthermore, anvils can advect over large distances without necessarily being forced by divergence associated with convective mass fluxes. For broad statistics, these may cancel out and leave you with a relation between the updraft mass flux and the anvil width as you claim, but you should discuss these other considerations. You can also provide more evidence for your hypotheses with the simulation output. Why not calculate the approximate mass fluxes into simulated anvils and relate them to simulated pedestal and anvil widths?

The referee presents a good point. We have added discussion of other physical phenomena that are likely to affect the measured anvil width. The referee makes another interesting request – to use the model cloud objects to calculate fluxes into the anvil and relate them to anvil and pedestal width. We have done this by taking the total connective mass flux (calculated as the implied surface area of the pedestal times the mean convective mass flux per unit length/area) at the anvil cutoff height for each object and plotting them as a function of pedestal width (see below). A best-fit line to the scatter of data provides a very nearly quadratic relationship (linear in log-log space to be consistent with the other new figures) which serves as a simple validation of equation (3) although one we admit in the manuscript is not entirely independent of the assumptions that go into (3). This figure is now included in the manuscript.



5. The 2/3 scaling is apparent in the simulation output in Figure 6, but the slope of the line is substantially different from the slope produced in the CloudSat data, so the relationship between anvil width and pedestal width is different in the simulation and observations. The possible reasons for this should be discussed in the text, especially given your suggestion that an anvil width parameterization scheme may be possible from predicted pedestal width.

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. We have also 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, although anvil widths still tend to be narrower in the model cloud objects than those from CloudSat. Our improved computation of the simulation-mean has shown the model results to be somewhat 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.



6. On page 15993, lines 6-11, I don't follow your argument about higher clouds contributing more mass to large-scale circulations. In fact, this entire argument seems to be conjecture with essentially no evidence shown to support it.

MCSs with a significant stratiform rainfall component have heating that peaks higher in the troposphere than more convective systems, as you state, but more than latent heating impacts large-scale circulations. More important near anvil tops would seem to be forced divergence and radiative heating. Latent heating is quite small in the upper troposphere because there is very little water vapor available for condensation there, so a cloud top of 13 km or a cloud top of 16 km is really irrelevant to the impact of latent heating on large-scale circulations because both systems could have equally developed stratiform precipitation between 5 and 10 km where the stratiform latent heating really matters. Stratiform precipitation that is developed enough to significantly contribute to the latent heating of the system would be part of the pedestal, so from a latent heating affecting large-scale circulations perspective, if would be the pedestal width that matters more than the anvil width or height. The anvil latent heating is definitely secondary in magnitude.

The authors agree that there are many complicating factors that will determine the actual horizontal influence of any individual cloud. All we say in the manuscript is that it is possible that the combination of higher and wider anvils is indicative of a greater horizontal influence (i.e. circulation). It is also important to remember the logical order of our argument. We postulate that cloud anvil width is at least partially a signature of the forced divergence and/or large-scale horizontal influence of clouds (as the referee alludes). Mean cloud top height is then regressed as a function of anvil width (Fig. 5). At the end of section 3.3 (at the page and line referenced by the referee), we then try to provide some context for why higher and wider clouds would seem to be the logical combination for more influential clouds. We have added a note at the end of the section to remind readers that this section is simply

another logical conclusion drawn from the cloud object database from which we can only diagnose physical relationships. We see the benefit of using clouds as a diagnostic as being that these conclusions do not rely on any mathematical or model assumptions that might otherwise bias the results from other methods.

7. On page 15993, you state that higher average cloud top heights indicate that wider, more organized systems are better able to produce strong updrafts but increased cloud top height does not necessarily mean that updrafts are dynamically stronger. That is one possible factor, but another could also be a moister environment around organized systems that limits effects of entrainment, and such an environment might also have lesser instability (from the system latent heating) with weaker updraft vertical velocities despite convection reaching higher altitudes. Yet another could be the cooling, moistening, and raising of the tropopause through detrainment, so that by the time the system has matured into an organized system observed by CloudSat, the cloud tops are higher simply because the upper troposphere has been modified. This is something that you could look into using the simulation output if you wanted.

Because we do not have the ability to track cloud objects in time (although this is a future goal), it would be difficult to determine from the model if stronger updrafts, or tropopause cooling are a cause of higher cloud tops. Mean, instantaneous pedestal-top convective velocity does correlate positively with top height (R^2=0.4) but it correlates less well with anvil width. However, it is unclear how well instantaneous measures of velocity at a particular level relate to the process suggestions we make in the manuscript which rely on parcel history.

We have changed the wording in the manuscript to reflect the fact that all we can say regarding higher tops is that wider clouds are somehow correlated with processes that convect cloud mass to higher heights. This leaves us agnostic to the cause since, as the referee points out, we cannot pinpoint it.

8. Sections 3.4.2 and 3.4.3 do not seem to be related to the rest of the paper, and are thus confusing. They would fit well in Igel et al. (2014), but I don't see how they address the results of the previous sections such as the average cloud scales, the ratio of anvil width to pedestal width with a 2/3 scaling, or the change in anvil top height with anvil width, which is what I thought you were going to use the simulations to do. You partially do this in Section 3.4.1, but I would leave 3.4.2 and 3.4.3 for another paper and go further with 3.4.1 by using the simulations to test some of your hypotheses in Sections 3.1-3.3 about the reasons for the 2/3 scaling and relationship between anvil top height and width.

Sections 3.4.2 and 3.4.3 have been removed. This has resulted in a much more focused manuscript overall with the more basic length-scale results and the 2/3 scaling becoming more prominent. At the referee's suggestion in his or her point 4, we have added a brief discussion of the functional form of convective mass flux at pedestal top on pedestal radius in an attempt to validate our equation (3). This has become a new section 3.4.2.

9. In the last paragraph of the conclusion, you state that as the pedestal grows, the anvil widens but at a rate slower than the pedestal. This is not true. The anvil still grows at a rate faster than the pedestal (as clearly shown by Tables 1 and 2), but the ratio of the width of the anvil to the width of the pedestal continuously decreases.

The referee is correct. The wording was ambiguous and has been changed to read: The anvil widens but the anvil-to-pedestal width ratio decreses.

Minor Comments

1. On page 15978, lines 25-26, you state that tropical meteorology is primarily composed of unremarkable oceanic, deep convection that is only unremarkable because of its high frequency. You also mention benign, ordinary deep convection in the previous sentence. Tropical meteorology consists of much more than deep convection, which I am sure you know, but the way that the sentence is written says otherwise, so I would rewrite it to say what you really mean. I also think the adjectives here could be clearer and less subjective. Are you trying to say that most tropical, oceanic convective systems are dynamically weaker, shorter lived, and more isolated than the squall lines and clusters that are more commonly studied? If so, I would say that instead.

Yes, the referee deciphered what we intended to say. This has been made clearer in the manuscript: The tropical deep convective population is primarily composed of weak, isolated convectie cells.

2. On page 15981, lines 18-20, you state that the goal of the paper is to gain simple, theoretical insight into the nature of tropical deep convection. It is unclear to me what "nature" means here. Can you be more specific than "nature"?

Our use of the word "nature" was ambiguous. We have changed it to "behavior" which is more specific.

3. I'm sure this is stated in Igel et al. (2014) somewhere and it seems to be the case from your text in the methodology section, but anvil width includes pedestal width, correct? This should perhaps be more clearly stated to avoid confusion.

Yes, anvil width includes the portion underlain by pedestal. We have made this clearer by including, "[f]urther, IDV14 require than the anvil be at least 50% wider than the pedestal in order to ensure cloud objects are mature. This requirement implies that at least 33% of the anvil width is underlain by clear air."

4. In Equation 6, shouldn't D be equal to one half of the anvil depth rather than the anvil depth? I would also remove the conversation about spherical anvils and just go with ellipsoids since almost no anvils look spherical and ellipsoids are more intuitive to someone imagining an anvil anyway.

The referee is correct that D should be half the depth. This has been rectified. Referee 1 also suggested that we remove the discussion of the sphere. This is something the authors have previously considered. Our concern with removing the discussion of the sphere is that it will be harder for future readers to appreciate that the 2/3 relationship comes from the powers on the flux area/volume since the explicit power is lost in (6). The authors agree, and now state in the manuscript, that a sphere is unlikely for the vast majority of clouds, but we believe that it is the only way to introduce the concept in

a simplified enough way to be easily comprehended. To begin the discussion of the ellipsoid, we now state, "(5) relies on the highly simplified assumption that anvils are spherical. In the mean (see Table I), anvils are much wider than they are deep and are decidedly non-spherical."

5. What do you mean by "deposition has a temperature dependence"? Do you mean that water vapor decreases with temperature, which controls the amount of deposition?

This sections has been removed at the referee's request in point 8 above.

6. Your comparison of simulated composite vertical velocity to maximum updraft vertical velocities in Heymsfield et al. (2010) is not a fair comparison because you are not examining the same thing. Heymsfield et al. (2010) examine peak upward vertical velocities in a set of convective updrafts. You are compositing vertical velocity everywhere (not just peaks in updrafts). Thus, your peak should be (and is) lower in altitude and strength than the peak in Heymsfield et al. (2010).

This section has been removed at the referee's request 8 above.

7. On Page 15996, lines 12-16, I am not sure what you are trying to say. By mass building down, do you mean sedimentation of condensate? If so, say that because condensate is a small fraction of the total air mass, and with air density increasing downward, it is difficult to build total mass downward. Second, why would mass converge at the mid-level velocity maximum? You actually show in Figure 7 that it converges below this level instead. I'm not sure how the anvil can be a bottom up process. You simply have divergence above the level of maximum velocity. Therefore, your anvil base should be located where horizontal divergence of condensate and moisture begin (as controlled by the level of peak vertical velocity, which is controlled by deposition) and the anvil top should be where this divergence stops (as controlled by parcels losing buoyancy). This is clearly shown in your Figure 7. Why does it need to be any more complicated than that?

This section has been removed at the referee's request.