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Interactive Comment

Interactive comment on "Tropical, oceanic, deep convective cloud morphology as observed by CloudSat" *by* M. R. Igel and S. C. van den Heever

Anonymous Referee #2

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Overview

This manuscript uses CloudSat data to define convective systems based on the methodology of Igel et al. (2014) to relate pedestal (essentially raining area) widths to anvil height and width scales of tropical, oceanic, convective systems. The approach of Igel et al. (2014) is a good one, and much has been learned via similar approaches of isolating individual convective systems using satellite data. Learning more about the relation between convective, stratiform, and anvil coverage is also important because of the crucial role of MCSs in hydrology and their impact on large-scale circulations, especially considering that organized convective systems are not represented in climate models and have some important aspects that are notoriously difficult to accurately represent in high-resolution models. There are several interesting and unique findings

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in this manuscript that are worthy of publication: an average tropical, oceanic deep convective cloud morphology, a 2/3 scaling between anvil width and pedestal (raining area) width in observations and a long-term RCE simulation, and a weak positive correlation between observed anvil top height and anvil width.

However, despite a lot of interesting results, they are connected via quite a bit of speculation without much evidence to support the speculation. The easiest way to cut down on the speculation is to simply remove most of it. It is okay to speculate a little bit, but large portions of this manuscript are dedicated to thought experiments that are not worthy of publication. I point out several of the most problematic spots of speculation in the manuscript in my numbered comments below. Alternatively, more work using the simulation output or TRMM data can be done to support the presented hypotheses, as also highlighted in the comments.

Furthermore, Sections 3.4.2 and 3.4.3 don't seem related to the focus of the rest of the manuscript and would seem to fit better in a separate publication. Finally, the definition and usage of "convective cores" in this manuscript and Igel et al. (2014) is problematic. With the problems of detecting low level rainfall variability in deep convective systems with a spaceborne W-band radar, evidence needs to be presented that shows that these are indeed convective cores. I have no confidence that they are convective cores or even rainfall cores, so I recommend removing any analysis related to convective cores and replacing it with further simulation analysis in support of some of the hypotheses presented in Sections 3.2 and 3.3, as described further in comments below. This would tighten the focus of the manuscript and provide stronger evidence for your hypotheses.

Following responses by the authors to the following comments and major revisions to the manuscript, it will be ready for publication in ACP.

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.

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

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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.

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

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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.

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?

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.

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.

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.

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.

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.

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.

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”?

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.

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.

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?

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).

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

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that?

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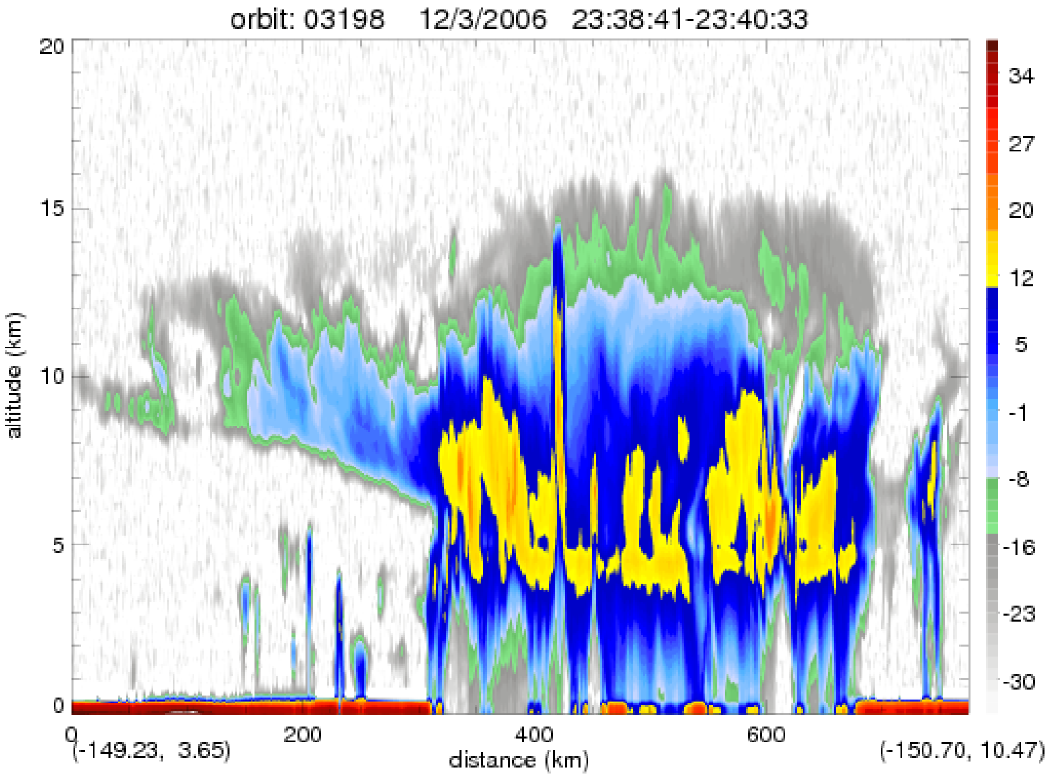

Fig. 1.

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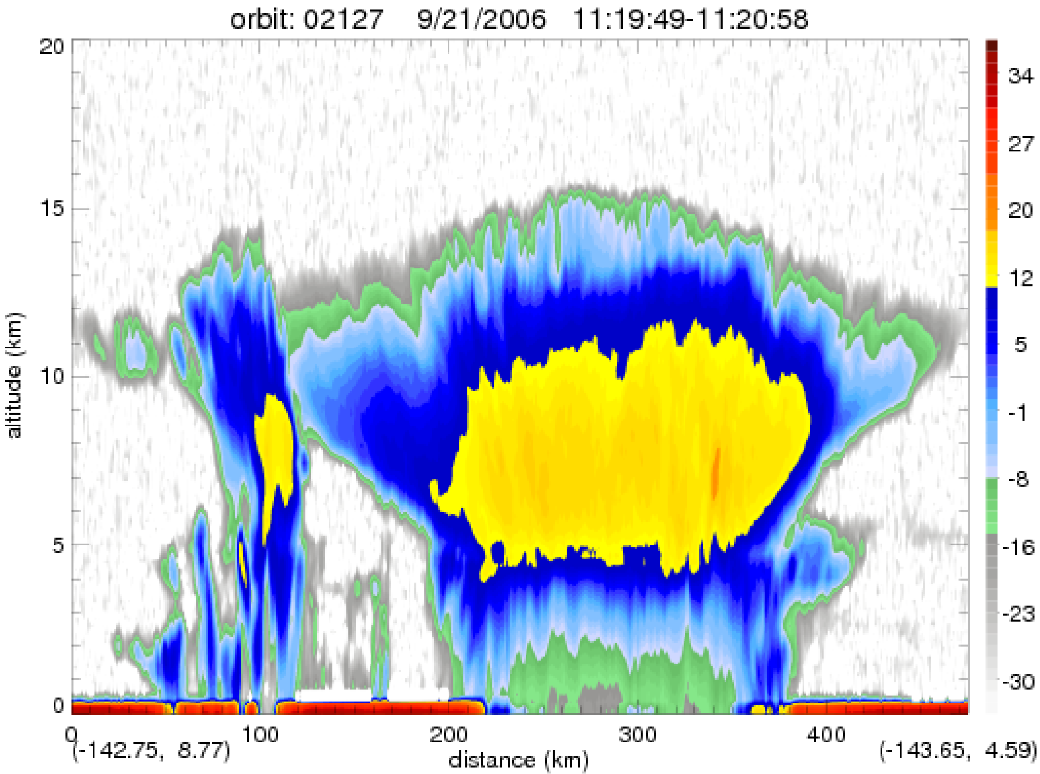

Fig. 2.

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