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Atmospheric Chemistry and Physics Discussions

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 #1

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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.

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.

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(pedestal width) versus log(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(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.

Minor comments:

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

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

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.

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

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?

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

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

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

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.

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.

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

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?

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

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.

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

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

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

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.

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

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

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.

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

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

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 15977, 2015.