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Interactive comment on "Upper Tropospheric Cloud Systems Derived from IR Sounders: Properties of Cirrus Anvils in the Tropics" *by* Sofia E. Protopapadaki et al.

Anonymous Referee #2

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Overview

This manuscript connects properties of tropical cirrus anvils to properties of "convective cores" producing them using AIRS observations. Convective cores are defined as having emissivity values greater than 0.98 based on correlations with AMSR-E rain rate retrievals. Systems with convective cores cover 15% of the area between 30°S and 30°N, while isolated cirrus without cores cover another 5%. Multi-core systems account for 1% of all cirrus systems, but account for 65% of cirrus coverage. Single core system life cycle is estimated using convective area fraction as a proxy for system age. Although land systems produce colder cloud tops and higher rain rates than ocean systems, system size and average emissivity increase similarly as the system

ages. Thin cirrus coverage increases as a fraction of total cirrus coverage (thick + thin) as the system ages. Some differences are apparent for early afternoon and early morning satellite overpasses, presumably because of differences in the probability of life cycle stages at these discrete times. Convective intensity is defined using the minimum retrieved cloud top temperature. As it increases (cloud tops become colder) in mature systems (convective core area fraction between 0.1 and 0.3), system size increases and the thin cirrus area fraction increases as well.

This manuscript presents interesting findings that are worthy of publication, but many of the findings rely on key assumptions that bypass deficiencies of the observations used without exploring their impact on the results and conclusions. The potential impact of these assumptions and deficiencies need to be better assessed, as described further in the major comments below. Satisfactorily addressing the comments will require major revisions, but most of them should be straightforward and hopefully clarify interpretation of the results.

Major Comments

1. Some of the dataset and methodology deficiencies and caveats of the results need to be explained in more detail. For example:

a. A major deficiency of AIRS and AMSR-E is that they only make observations at 2 times of day (~0130 and 1330 LT). This particularly biases results over land as the average tropical diurnal cycle in deep convection has a strong peak in the late afternoon (~1600 LT), which is well known from TRMM observations. Furthermore, this diurnal cycle varies by geographical location, so more intense or larger systems are likely favored more in some regions at 0130 or 1330 LT and in others at different times that are not captured by AIRS and AMSR-E.

b. That convective core area fraction is correlated with system life cycle stages is a major assumption. Although single core systems are isolated for analysis, there is no reason to think that some of these systems did not evolve from or into multi-core systems. If this is the case for a significant fraction of convective systems, then the life cycle stages shown are not representative of typical system evolution, which should not be constrained to single core or multi-core categories for the entire life cycle. It may be the case that this does not heavily impact the statistics shown in Figure 7, but this should be proven to be the case, for example by examining actual life cycles using geostationary satellite data.

2. It is difficult to formulate physical interpretations of “thin” and “thick” anvil cirrus. The distinction seems fairly arbitrary (emissivity greater than or less than 0.5). Since CloudSat and Calipso are also flown in the A-Train satellite constellation, why not show a CloudSat/Calipso cross-section that shows how a typical convective system would be split up into convective core, thick cirrus, and thin cirrus so that the readers can better understand the physical differences between these cloud categories?

3. The definition of “convective core” is different than in most studies of deep convective systems. Typically, this refers to the region with buoyancy driven vertical motions over a relatively deep layer that produces net latent heating throughout most of the troposphere or the region with denser hydrometeors and higher condensates produced via convective motions. The definition used in this manuscript is really just a deep raining region that could be either convective or stratiform. The average AMSR-E rain rates for emissivities between 0.98 and 1 that define convective cores are $\sim$1-3.5 mm/h, which are more consistent with stratiform rain rates than convective rain rates. At the very least, a significant fraction of convective core areas, as they are defined in this study, likely contain stratiform rather than convective precipitation and vertical motions. This should be clarified in the revised manuscript perhaps by renaming the convective cores as deep precipitation cores.

4. In addition to convective intensity, the level of neutral buoyancy for ascending, buoyant air strongly impacts the minimum cloud top temperature. The level of neutral buoyancy is likely to be related to the properties of the tropical tropopause transition layer and the cold point tropopause. The tropical tropopause temperature varies

significantly by latitude and season (e.g., Seidel et al. 2010, JGR-Atmospheres, http://onlinelibrary.wiley.com/doi/10.1029/2000JD900837/full; Fueglistaler et al. 2009, Rev. Geophys., http://onlinelibrary.wiley.com/doi/10.1029/2008RG000267/full), which means that using minimum cloud top temperature as a proxy for convective intensity across the entire tropics may introduce time and location biases. In other words, different convective intensities as they are defined in this manuscript may be correlated with specific geographical locations and seasons. This should be fairly straightforward to explore by comparing different latitude bands and seasons with one another.

5. Many of the large cirrus systems will cover a piece of land and ocean. How are these systems assigned to land or ocean categories?

6. The reason that AIRS and AMSR-E are used over other satellite datasets needs to be better explained. How does AIRS improve on what can be retrieved by geostationary satellites regarding convective system cloud properties? It is suggested that it can better distinguish optically thin cirrus clouds from warmer, mid level clouds by decoupling cloud altitude and emissivity, but how is this done? Are the results of this manuscript any different than what has already been learned from geostationary datasets such as ISCCP and active sensors such as CloudSat and Calipso? Briefly putting the results of this manuscript in the context of previous studies in the conclusions would be useful.

7. Just because the cloud top temperature is correlated with the rain rate does not mean that it is a good proxy for convective strength or intensity. In fact, a recent study has shown that the highest rain rates in the tropics may not be associated with the most intense convection based on radar reflectivity echo tops (Hamada et al. 2015, Nature Communications, http://www.nature.com/articles/ncomms7213?WT.ec_id=NCOMMS-20150225). Of course, this is using the traditional definition of convective intensity, which typically refers to the updraft vertical velocity magnitude. It has been known for some time though that convection need not be intense to reach the tropopause (e.g., Zipser 2003, Meteorological Monographs, http://link.springer.com/chapter/10.1007/978-1-878220-63-9_5). Therefore, I recom-

mend removal of the “convective intensity” terminology because it is being very loosely, which leads to confusion. Why not simply refer to the “convective depth” instead? It could still be pointed out that it is positively correlated with rain rate.

8. On page 8, line 1-2, it is stated that minimum brightness temperature has been shown to be a more skillful proxy to describe convective intensity compared to the radar echo height based on Jiang (2012), but this is not what is concluded in Jiang (2012). Jiang (2012) states that minimum infrared brightness temperature in the inner core of tropical cyclones is a better indicator of tropical cyclone rapid intensification than other proxies for convective intensity. It is also problematic that this study, which focuses on tropical cyclones, is being used in the manuscript as representative of all tropical MCSs. In fact, it is well known that for a given minimum infrared brightness temperature, convective intensity is far stronger over land than ocean, which is reflected in far different reflectivity profiles, microwave brightness temperatures, and lightning flash rates, which are the traditional measures of convective intensity (e.g., Zipser et al. 2006, BAMS, http://journals.ametsoc.org/doi/pdf/10.1175/BAMS-87-8-1057; Liu et al. 2007, J. Climate, http://journals.ametsoc.org/doi/full/10.1175/JCLI4023.1; many others). Minimum infrared brightness temperature is assuredly not the most skillful proxy for convective intensity when used across the entire tropics.

9. Is Figure 1 just one example of many possibilities or is it an average relationship? Does a pair of given brightness temperature and emissivity values always produce the same retrieved cloud top temperature? For example, will a brightness temperature of 260 K and emissivity of 0.6 always produce a cloud top temperature of 230 K? More information on how cloud top temperature is retrieved and limitations of the retrieval would be helpful.

10. Possible inconsistencies between Figure 7 and Figures 10 and 12 need to be explained. For example, in Figure 7a, cloud system size increases from stage 6 to stage 9, but in Figure 7e, the minimum cloud top temperature increases from stage 6 to stage 9 (during system maturity when convective area fraction is between 0.1

and 0.3), so as minimum cloud top temperature increases, system size increases. However, Figure 10 shows system size decreasing with increasing minimum cloud top temperature, which is the opposite relationship. Similarly, Figure 12 shows that the thin cirrus anvil area fraction decreases with increasing minimum cloud top temperature, but Figure 7c and 7e show that thin cirrus anvil area fraction increases with increasing minimum cloud top temperature, which is the opposite relationship.

11. Are the results in Figure 14 a result of different life cycle stages of the single core systems or do these differences also exist for a given life cycle stage (indicating differences in the life cycles of systems of varying convective depth)? Clarification here would provide valuable insight into the results.

Minor Comments

1. I suggest changing “build” on page 1, line 23 to “are part of” since the clouds are primarily a function of the convection rather than the other way around.

2. On page 2, line 5, there appears to be a missing word after “MCS’s”.

3. The data is gridded at a resolution of 0.5°, but it seems that the distribution of cloud types defined at the native measurement resolution in each grid box is use for all of the figures. Is this correct?

4. The gap between orbits is largest at the equator. Please state the width of the gap and scan at the equator on page 4, line 19.

5. Which latitude band is used for most of the figures? This should be mentioned in Section 2.

6. Please capitalize “south” on page 5, line 26.

7. Remove “s” from “includes” on page 6, line 9.

8. Insert “do” after “dissipation” on page 6, line 24.

9. Change “is” to “are” on page 7, line 18.

10. Change “on” to “in” on page 7, line 22.

11. Remove “or” on page 7, lines 24 and 25.

12. Change “signal” to “reflectivity” and “or brightness temperature” to “and microwave or infrared temperatures” on page 7, line 25.

13. Insert “cloud top” before “temperature” on page 7, line 31.

14. Does the resolution of the minimum retrieved cloud top temperature used in analyses change based on distance from nadir?

15. Please clarify what is meant by “rain detection offset over land” on page 8, line 8.

16. Change “is” to “it” on page 8, line 31.

17. A citation is needed for the statement that convective intensity will increase in a warming climate on page 8, lines 32-34.

18. Insert “of” after “years” on page 9, line 2.

19. Why do the distributions in Figure 6 go less than 0 and greater than 1 when convective fraction cannot be less than 0 or greater than 1?

20. Are the bars in Figures 7 and 9-13 standard errors of the mean? If so, please state that and include the sample sizes used to make the figures.

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