Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-408-RC1, 2018

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A 17 year climatology of convective cloud top heights in Darwin" *by* Robert C. Jackson et al.

Anonymous Referee #1

Received and published: 4 August 2018

Review: ACP-2018-408

Title: A 17 year climatology of convective cloud top heights in Darwin

Recommendation: Major Revision.

Summary

This study examines the variability of convective echo-top heights (ETH) observed by long-term CPOL radar in Darwin Australia as functions of large-scale conditions during active/suppressed MJO and monsoon/break periods. A new technique to estimate ETH is described and compared to a traditional reflectivity threshold based technique, and to a short period of geostationary satellite retrieved cloud-top heights. The study then continues to partition ETH distributions by combining MJO phases with monsoon

indices, and concludes that MJO has relatively stronger influence to convective ETH than monsoon.

In general, I think there are valuable results presented in this paper, particularly illustrating the MJO activity over Australia are relatively more important in regulating convective ETH compared to monsoon vs. break conditions, which previous studies have not investigated. However, there are many aspects of the study that need improvement to make it a significant contribution worthy of publication in ACP.

First, the "novel new technique" described in this study does not prove to be any better (or even different) than simpler existing method in calculating ETH, at least based on the short and in my opinion problematic comparison with a passive satellite cloud-top height retrieval dataset. Then why make a big deal about it? There is nothing wrong with using existing ETH technique, especially if you can't show that this new method works better than previous ones.

Second, I think the author need to connect the relative difference in large-scale conditions between active/suppressed MJO and monsoon/break to help explain why MJO has stronger modulation to convective ETH. Looking more at the difference in variability of the sounding profiles (rather than just the mean) between those conditions may be useful.

Third, the study misses an opportunity to examine how do these different large-scale regimes modulate an important aspect of convection in Darwin: the variability of convective cell sizes. Several recent studies have pointed out the importance of convective cell sizes to mass flux, a critical aspect to cumulus parameterizations. Also, analyses of spatial scales of convection would provide more concrete conclusions on changes of MCS activities with large-scale regimes, which the paper makes many reference but did not show supporting evidence.

I provided more detail comments below on various places in the paper that need improvement. Because I do see the value of the long-term tropical radar observations, I recommend major revision of the paper before it can be accepted for publication.

Major Comments

1. Page 2 line 20, please be more specific on what aspects of convective parameterizations are “poor”. Poor in doing what?

2. Section 3.1, what kind of quality control procedures were applied to the raw radial radar data? How do you handle ground clutter, AP, and noise that are particularly prevalent in CPOL data at lower level, which could affect your ETH estimates?

3. Page 4 line 18, why choose a “box” when the radar scans are circular? The diagonal corners of the box are 140 km away from CPOL, which is much further than 100 km radius where sampling and resolution are better.

4. Page 5 paragraph 2 and 3, I do not understand how is ETH estimated in CPOL using the Doppler velocity standard deviation (σ) technique. Figure 2 shows example of using σ > 3 to remove non-precipitating radar echoes, but then how is ETH determined from the remaining echoes? How is it different from simply just using Z threshold > 5 dBZ?

5. Please also clarify how is ETH estimated using the Z > 5 dBZ threshold method. Do you go from surface upwards and find the first height level where Z drops below 5 dBZ? Or do you go from top downwards to find first level where Z exceeds 5 dBZ (i.e. max height of 5 dBZ in a column)? They could give very different results because the second approach would get cirrus/anvil clouds that are above precipitating convective cloud-tops (particularly for existence of multi-layer clouds).

6. Figure 3, the authors did not provide enough detail about how the CPOL data and MTSAT data are matched for the comparison. Given the two datasets have different spatial resolution, do you match them by interpolating one to another? Do you only compare grid points that both CPOL and MTSAT identified as echo/cloud?

7. There is also the issue of daytime vs. nighttime retrieval differences in the MTSAT data. As I mentioned in minor comment 7, there are two satellite retrieval algo-

rithms separately for daytime and nighttime. My previous experience working with these datasets are they do not necessarily provide consistent cloud-top height retrievals when switching from one to another. Cloud-top heights from the same cloud systems can differ as much as several kilometers between the two estimates, and during twilight hours (+/- 1-2 hour) when the solar zenith angle is high, the retrievals uncertainties are very large. Did you 1) compare daytime vs. nighttime separately? 2) exclude twilight hours?

8. Why do you choose such a short period of only two months during the peak monsoon TWP-ICE period for a comparison? Particularly when most of the precipitating clouds are deeper than 7-8 km. MTSAT retrievals at this location are available for multiple years in the ARM data archive. Further, wouldn’t a more direct comparison be made between CloudSat measured cloud-top heights as it is active remote sensing? For such a long CPOL record and decent spatial coverage, there should be plenty of samples to compare. One of the coauthors of the study (Alain Protat) has made much harder comparisons between CloudSat and ARM cloud radars before (Protat et al. 2014), what stop you from doing that? I understand it takes some effort to do that, but if the paper wants to claim that this new method of estimate ETH from CPOL is closer to actual cloud-top height, then a more stringent evaluation is needed than what is presented here.

9. Page 5 lines 26-29, if the new technique using σ to calculate ETH (which I do not understand, see major comment 4) gives a similar result with a much simpler Z threshold approach, what is so unique about this new approach then if it does not perform better than existing ones? Why make a big deal about the novelty?

10. Page 6 line 30, I cannot tell if there is a significant difference in dew point temperature between monsoon break and active period from Fig. 3-4 as they look very similar. You should also compare specific and relative humidity profiles. Mid-level humidity in the tropics is particularly important for supporting deep convection (e.g. Hagos et al. 2014). Showing the difference between the profiles may be useful. Also, given the

small difference in the mean thermodynamic profiles between MJO phases, showing a difference in the mean and the variability is useful as well. Please also include the number of soundings that go into the composite.

11. Figure 6, can you comment on why in this study the "overshooting" mode found in Kumar et al. (2013) is not visible in the much longer dataset? Their study showed that the overshooting mode correspond to intense low-level reflectivity (and inferred larger and more numerous raindrop particles), which tend to occur more during the monsoon break period. Does the 14 km peak in break period (Fig. 6 MJO=1,2) correspond to that mode?

12. The congestus mode in Kumar et al. (2013) is defined as ETH < 6.5 km, where in this study it is 8 km. That is not a small difference. 8 km is also significantly higher than the 0C (4.5-5.0 km) level where above which freezing and additional latent heating acceleration of vertical motion can occur. Can you comment on why you choose a larger ETH value for congestus?

13. Page 8 line 16-18, I thought A is the contribution of mode 1 (congestus), when A increase to 0.9 during active phases of the MJO under monsoon conditions, doesn't that mean most of the convection are congestus, as opposed to deep convection stated in this sentence? That is contrary to the statement of mostly widespread MCSs during active MJO and monsoon. I think the issue here is using only echo-top height to indicate congestus vs. deep convection is too simplistic. The wide spread MCSs are likely associated with much larger but not as deep convective cells compare to more isolated but deep convection during break period. One very important indicator for organized convection, i.e., the size of convective cells is ignored in this study. Larger convective cells and larger updrafts (i.e. in MCSs) carry a majority of the mass flux as reported in both observational analyses (Kumar et al. 2015, Masunaga and Luo 2016) and high-resolution model results (Hagos et al. 2018). The size of convective cells is just as important (if not more) as the depth of the convective cells. It can be easily quantified with the CPOL data and I think would be a useful quantity to investigate as functions of

large-scale regimes along with the ETH analyses.

14. Page 8 line 29-30, I think this is an interesting and important finding. It would be useful to discuss what aspects of the large-scale conditions can help explain larger difference under active MJO (i.e. going back to Fig. 4-5, see major comment 10 about quantifying their relative environment profile difference between active/suppressed MJO, monsoon/break).

15. Figure 9, other than the more obvious enhanced frequency of deep convective during daytime, the difference for the rest of the panels between MJO phases are difficult to see. Perhaps adding a difference panel would help.

16. Page 9 line 26-29, why do you have to guess that the enhanced nighttime peak over the ocean during monsoon period is due to MJO? It is relatively easy to identify MCSs in CPOL data (e.g., Rowe et al. 2014 used a simple criteria of precipitation feature major axis length > 100 km to identify MCS in ground-based radar observations), why not actually quantify MCS frequency changes to better support your claim?

17. Figures 10-11, given the strong diurnal cycle between land vs. oceanic area shown in Figures 8-9, did you separate land area and ocean area when calculating their ETH occurrence? I also suggest plotting Figures 10-11 in local time to make it easier for readers.

18. Page 11 line 13-14, which figure shows a peak of ETH around 5-6 km during break conditions? Figure 6 when MJO is away from Australia (phases 1-2) generally show a peak between 6-8 km, but drops to 4 km in phases 3-4.

Minor Comments

1. Page 1 line 2, technically validation of convective processes in GCMs do not "require" such statistics, perhaps it's better to say "could benefit from" such statistics.

2. Page 1 line 5, why does it have to be for a specific model? These observations can be useful for any large-scale model validations.

3. Page 1 line 22, Jensen et al. (1994) stated the 100 W/m2 is solar forcing, not net radiative forcing, please clarify that in the statement.

4. Page 2 line 19, "... of of an intense ...".

5. Page 2 line 27, "... for one wet season in and found ..." in what?

6. Page 4 line 1, spell out the acronym "ACRF".

7. Page 4 line 4-6, I believe the MTSAT data, at least the inferred channel spatial resolution should be 5 km, not 1 km. Also, the VISST technique uses all available geostationary satellite channels, including visible, water vapor, near IR, and IR channels to retrieve cloud properties during day time. For nighttime, a different technique SIST is used for the lack of visible channel.

8. Page 5 line 14, you mentioned "normalized frequency distribution", what is the unit of the shading in Figure 3? The large numbers appears to be just a count, if so it is not normalized frequency.

9. Page 5 line 31, do not use "cloud top height" and ETH interchangeably. You have not established in this study that CPOL ETH is equivalent to cloud top height.

10. Figs. 3-4, why is the vertical scale different between the two figures?

11. Page 7 line 12, spell out p.d.f. in section headings.

12. Page 8 line 8, "In 7, ..." do you mean in "Eq. (1)"?

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