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.

We would like to thank the reviewer for their insightful comments on this manuscript. In a phase randomized radar significant returns have a “Smooth” radial velocity while regions of no return or multi-path have radial velocity that varies randomly from -nyquist to +nyquist. This allows texture of radial velocity to provide a very “clean” determination of a significant return. Most importantly it provides a consistent definition of echo top height rather than relying on an arbitrary threshold. Texture allows the echo top height to match the height of minimum detectable signal.
We have rephrased the “novel new technique” wording in the abstract to simply state that we are assessing the applicability of such a methodology. This therefore reduces the scope of this section from attempting to state which methodology better estimates the cloud top height to simply a comparison that assesses the sensitivity of our results to the ETH retrieval technique used. We still compare the retrieved ETHs against cloud top heights estimated from satellites in order to assess whether or not the retrieval can capture the statistical variability in ETHs without necessarily knowing the uncertainty. Given a dataset as large as this, this approach is feasible as this tells us that we can observe the relative interseasonal variability in ETHs. Furthermore, we feel that it is

important to show the sensitivity of the ETHs to the retrieval technique used, which few studies do, even if a null result is shown.

Given the revision in scope of Section 3, the new wording in the abstract rephases the testing to demonstrate that there is little sensitivity to the ETH with retrieval technique and that our retrieval captures the relative seasonal variability in cloud top height:

"Retrieved ETHs are correlated with those from MTSAT retrieved cloud top heights, showing that the ETHs capture the relative variability in cloud top heights over seasonal scales. "

The wording in Section 3 regarding the methodology comparison already stated that both methods were roughly equivalent, so no changes were made there. However, since both methodologies give similar echo top heights as shown in the comparison in Section 3, we still elect to use the velocity texture methodology since it provides comparable results to using reflectivity.

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.

We have added 5th and 95th percentiles to Figures 4 and 5 to represent the variability seen in each of the large scale forcing regimes and also now add specific humidity. We also now better connect the differences between the large scale forcings in the discussion of Figures 4 and 5 by examining the differences in the distributions of winds and specific humidities, with two paragraphs in Section 3.3 instead of one demonstrating the relative differences between the regimes. In it is shown in this section now that:

1. **There is a greater variability in the Surface to 500 hPa winds during an inactive MJO, which contributes to fewer cases where stronger flow of moisture from the west are occurring.**
2. **There is reduced mid-level moisture (4 to 8 km) during inactive MJO/break conditions, as well as wider variability of such moisture as indicated by the specific humidity profiles. This suggests that the large scale environment during an active MJO is more favorable for the transition of congestus to deep convection.**

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.

We have added an analysis of both how the cell sizes vary and now objectively quantify the number of MCSes using Rowe and Houze (2014)'s methodology to justify our claims about MCS coverage. In addition we have added material to the introduction now introducing why cell size is important and what past studies have concluded about how the cell sizes in Darwin vary in differing large scale forcings in the introduction. We thank the reviewer for this suggestion!

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?

We have replaced this sentence to be more specific by what we mean by "convective parameterizations are poor:"

Also, convective parameterizations in GCMs do not account for mesoscale organization resulting in insufficient sensitivity to upper tropospheric humidity (Del Genio, 2012).

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?

The great advantage of the use of velocity texture compared to using reflectivity is that the noise floor can be detected automatically since we expect the velocity field in regions of noise and second trip echoes to be random, resulting in higher velocity textures. We are also doing the following steps:

- **Excluding gates with differential reflectivity < -3 and > 7 dB.**
- **Excluding gates with < 0.45 cross-correlation coefficient.**
- **Excluding gates with differential phase texture > 20**
- **Excluding gates with reflectivity less than -20 dBZ and greater than 80 dBZ.**

We have added these steps into a bulleted list into Section 3.1

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.

We chose a "box" since we interpolated the radial data onto a Cartesian grid for easier spatial analysis and using boxes is easier with data in Cartesian coordinates. While creating the Cartesian grid, we used Barnes (1964)'s weighting function with a radius of influence that increases with distance from the radar in order to account for the decreased sampling and resolution as a function of distance from the radar. We conducted visual analyses of such grids and had determined that using a 100 km by 100 km box gave reasonable coverage, even at ranges of 140 km.

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?

Previously, the ETH was determined by looking at the gate above the highest valid gate after the σ **technique was applied. The ETH is now determined by looking for the first echo in the column that is masked using the** σ **technique. We have added some clarifying wording in this paragraph to explain how we retrieve the ETHs from the masked data:**
"*The ETHs are then determined by looking at the lowest gate in the column that is masked. We use the lowest gate in the column in order to ensure that we are capturing the ETHs of the precipitating convection, and not that of detrained anvils and cirrus that can lie above the precipitating convection.***"**

This is different from using Z > 5 dBZ as we are not using reflectivity at all.

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).

We had originally used the topmost point in the column where Z > 5 dBZ, or the second approach. However, in order to account for cases of multi-layer clouds, we have switched to the first approach as it is more representative of the precipitating convection. We now also make it more clear how we are deriving the ETH from Z in Section 3.

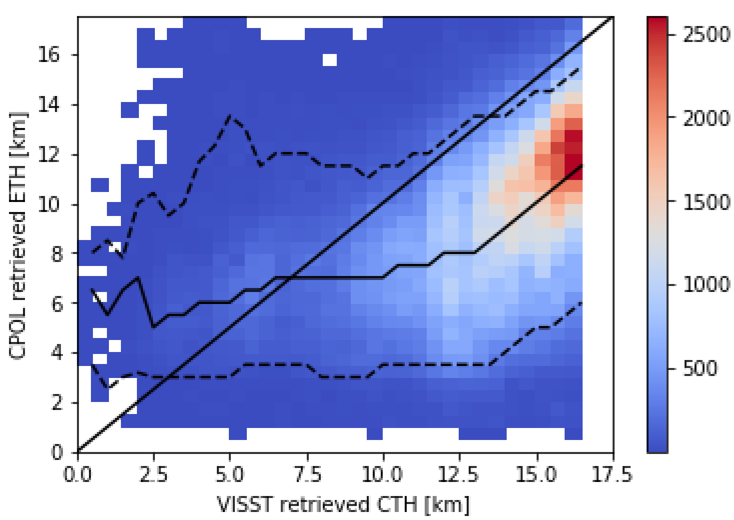
6. Figure 3, the authors did not provide enough detail about how the CPOL data and MTSAT data are matched for the comparison. Given the two datasets have different spatial resolution, do you match them by interpolating one to another? Do you only compare grid points that both CPOL and MTSAT identified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

We interpolated the MTSAT data to CPOL's grid and now say so in Section 3.2. In addition we also state that we only compare points that were identified as in cloud by the VISST product and as convective by the Steiner et al. (1995) algorithm in CPOL.

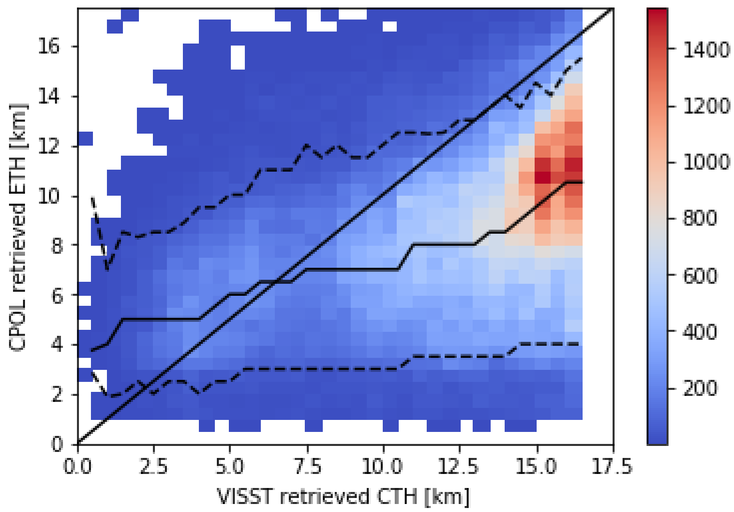
"Since the two datasets are at differing resolution, the MTSAT data are interpolated onto the same grid as the CPOL data for the comparison. Furthermore, to ensure that we are comparing points that are in precipitating convection we both only include points from MTSAT where the VISST product identified cloud and where the convective classification algorithm, detailed in Section 3.4, classified the grid points as precipitating convection."

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

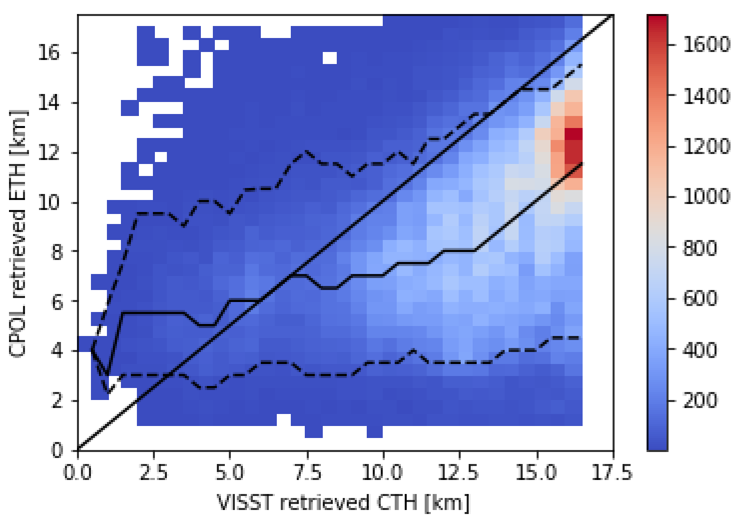
We had not originally done these separations. We went ahead and separated Figure 3c by retrievals made in the daytime and excluded twilight and placed them in the figures below. Our results are insensitive to the time of day.



Frequency histogram of VISST retrieved cloud top heights versus CPOL retrieved ETHs using the σ during the daytime (600 to 1700 local time).



As above, but during night time



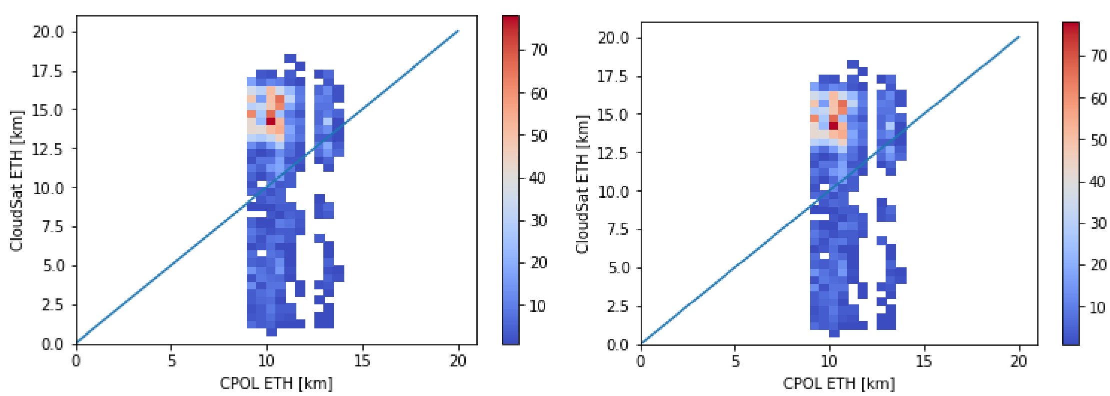
As above, but with twilight hours excluded

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.

We had chosen the two month period during TWP-ICE because the MTSAT data in the ARM archive initially because we, although now we have extended the comparison from 2006 until 2010 where the version 4 MTSAT data are available. We decided to choose this time period because as same version of the VISST product was available to ensure that differences in processing between the differing versions of the VISST product did not interfere with the comparison. Therefore, we now have done a more comprehensive comparison of the ETHs with those from MTSAT than what was done before.

Using CloudSat data instead of MTSAT data for the comparison provides us with even fewer data points. Figures R1 and R2 show the derived from the CloudSat calculated using the highest point where the echoes are classified as "good" in the Level 2 compared to those derived using Z < 5 dBZ (Figure R1) and $\sigma < 3$. The gate from CloudSat was compared to the nearest grid point in the CPOL data for the comparison. While there is CloudSat data present from the years 2006 to 2017, since the comparisons are limited to the areas scanned by the CloudSat granules, we only have 2,387 datapoints for comparison compared to having . Therefore, the issue of there being relatively few samples also applies to the CloudSat data. Therefore, we elect to use the MTSAT data in the comparison, as there is actually more data in the two month period we analyzed than in all of the CloudSat granules.



(left) The 2D frequency distribution of ETH derived from CPOL using the highest gate where $\sigma < 3$. compared against ETH derived from CloudSat. (right) as left, but using the highest gate where Z > 5 dBZ.

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?

This new approach has the major advantage in that, one the noise floor is automatically detected, and, two, second trip echoes are removed. Also, the approach that uses velocity texture is immune to radar miscalibration and therefore can be applied more readily to radar datasets than techniques that use reflectivity. Further, since 5 dBZ can be well above the noise floor of the

In this particular case, we have shown that there is little sensitivity in the retrieved ETHs to whether or not one uses reflectivity or velocity texture. Therefore, we have reframed the comparison as a sensitivity test and removed claims in the paper about the novelty of the approach. Rather, we now claim that our ETH retrieval is robust given how little sensitivity there is.

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.

We thank the reviewer for this very useful comment. We have added specific humidity profiles to Figures 4 and 5 as well as added the amount of soundings that we derived the profiles from in the figure captions. Given concerns from the other reviewer regarding there being too many variables to look at in the paper, we did not add relative humidity to Figures 4 and 5.

The specific humidity at the mid levels is about 1 g/kg higher when the MJO is active and during monsoon conditions. The Hagos et al. (2014) study would suggest that enhanced mid level moisture facilitates the transition from shallow to deep convection, which is consistent with the relative unimodality and the relative unimodality we see in MJO-active/monsoon conditions. Furthermore, the new analysis now shows a greater variability in the winds and specific humidity during MJO inactive/break conditions, which makes for

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?

With the new processing as suggested by previous comments from this reviewer, the 14 km peak has disappeared. However, we have added discussion on the presence of ETH greater than 15 km, which are only present during break conditions.

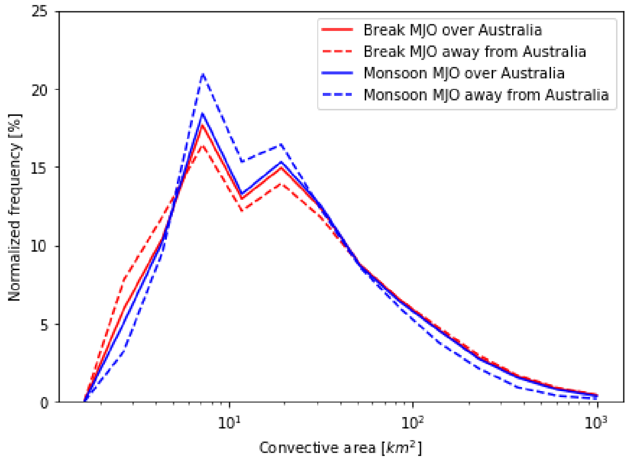
12. The congestus mode in Kumar et al. (2013) is defined as ETH < 6.5 km, where in this study it is 8 km. That is not a small difference. 8 km is also significantly higher than the 0C (4.5-5.0 km) level where above which freezing and additional latent heating acceleration of vertical motion can occur.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

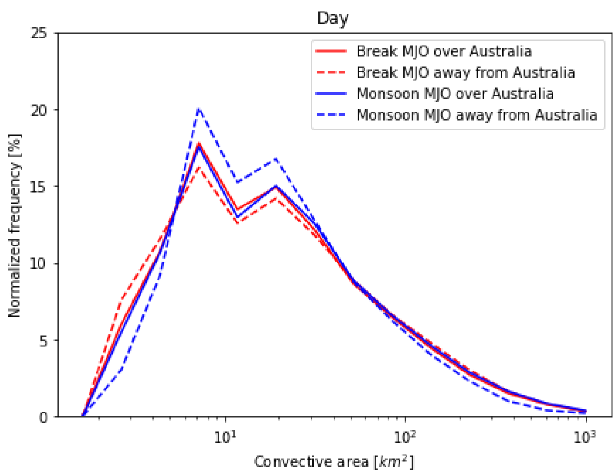
We had originally defined the congestus and deep modes corresponding to the μ_1 and μ_2 derived from the bimodal Gaussian fits which resulted in larger values than what is typically considered congestus. In order to be more consistent with the past literature, we still base our classification of congestus and deep convection from the fits, but now place stricter criteria on what is classified as "congestus" versus "deep convective:"

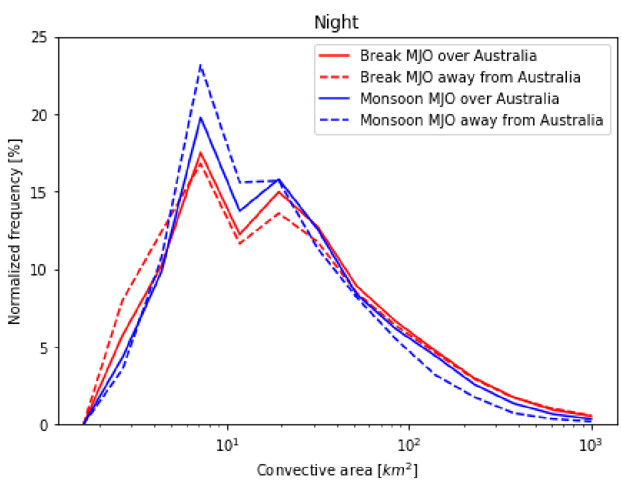
1. **If the ETH distribution is bimodal (0.1 < A < 0.9) then mode 1 is the congestus mode and, mode 2 is the deep convective mode**
2. **If the ETH distribution is unimodal (A < 0.1) and μ_2 < 6.5 km then the single mode is the congestus mode, otherwise the single mode is the deep convective mode**
3. **If the ETH distribution is unimodal (A > 0.9) and μ_1 < 6.5 km then the single mode is the congestus mode, otherwise the single mode is the deep convective mode**

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.

We thank the reviewer for this helpful comment. We have taken care to more carefully define congestus and deep convection in response to this reviewer's major comment number 12. Doing this has improved the presentation of the results in this section and reduces confusion.







We now discuss the convective areas as a function of large scale forcing, noting that there are generally lower convective areas in monsoon or active MJO conditions, which would be consistent with generally weaker convection being present during these conditions. We use this analysis, in combination with the quantitative MCS analysis

suggested by the reviewer to show that monsoonal conditions are characterized by weaker, but more frequent MCSes.

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).

Thank you. We have added a couple of sentences of discussion here on how the differences in the environmental profiles observed between MJO phases could be contributing to the differences in the ETHs seen:
"*Considering that, in Figures 4a and 5a show greater increases in equivalent potential temperature with height above the 5 km stable layer during MJO inactive conditions, this suggests that the midlevel thermodynamic profiles support greater inhibition of the convection in the deep convective mode when the active phase of the MJO is away from Australia."*

With the new ETH processing and more careful definition of the modes, we also now observe that the congestus mode is more sensitive to the presence of the monsoon while the deep convective mode is more sensitive to the presence of the MJO

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.

We attempted to add a difference panel to Figure 9, but this made the figure too busy to be readable. Given that the other reviewer commented that there were too many variables in the paper, we did not add a difference panel here.

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?

We have quantified the radar coverage of MCSes using the methodology of Rowe and Houze (2014) for each scan as the reviewer suggested and have added the average number of MCSes in the radar domain per scan for a given large scale forcing regime and time of day in Table 1. The normalization was done to ensure that differences in the number of MCSes identified was not due to the differing lengths of time spent in each regime.

The results in Table 1 clearly show that, during both an active MJO and active monsoon, that on average more MCSes are present in the radar domain during these conditions. On average, there is also increased presence of MCSes during the daytime compared to night time. Therefore, doing a quantitative analysis suggests that, in most conditions except active monsoon, there are more MCSes at night than during the day. Therefore, most of the conclusions we had before still hold, and where any changes had to be made the discussion was changed accordingly. We also now refer to the frequencies in Table 1 in addition to the already provided references to justify our claims of MCS frequency in Section 4.

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.

We did not initially separate the diurnal cycle figures by land and ocean. At the suggestion of the other reviewer who was concerned about too many variables being presented in this section we have not added an extra figure separating the diurnal cycle by land and ocean.
Figures 10 and 11 are now plotted as a function of local time.

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.

We have modified this conclusion to be more consistent with the analysis in Figure 6, which has changed with the ETH reprocessing.

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

We have changed the wording in this sentence to the suggested wording from the reviewer.

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.

We were focusing on E3SM as for that model there is an undergoing development to have the model compute results at a 12 km resolution that is high enough such that both MCSes are resolves and the assumptions made in convective parameterizations may not apply. However, since we agree that this is really useful for any GCM, we have changed this sentence to state that this dataset is useful for the validation of convective

processes in any GCM.

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.

We have corrected this statement.

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

We have removed the extra "of."

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

We have completed this sentence

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

Due to a recent change in the name of the ARM Climate Research Facility (ACRF) to ARM Facility, we have changed this to say "ARM Facility."

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 geo-stationary 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.

We now mention that the VISST technique uses these two techniques in this sentence. Also, according to the ARM archive, the resolution is 4 km, which we now mention here.

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.

We have removed the word "normalized" from this sentence.

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.

We have changed this phrase to say "echo top height."

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

The vertical scales are all now the same between Figures 3 and 4.

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

We have corrected this to say "normalized frequency distributions" which is actually what is being plotted.

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

We meant to say Figure 7. It says so now.

References for Authors' Response
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