General Comments This study aims to define characteristics of monsoon phase (break vs active) within context of MJO phase over Australia for northern AU region. 17 years of radar data are used to increase sample size and develop statically significant results. Overall I think this is an interesting study that shows the impact of MJO phase on cloud top heights for active and inactive periods drawing on precipitation and thermodynamic characteristics to explain the results.

We would like to give thanks to the reviewer for taking the time to provide careful feedback on the manuscript. We agree that the results from the long term dataset presented in the paper are an interesting addition to the literature. In response to another reviewer we have re-proccessed the ETH to be defined as the lowest gate in the column where the velocity texture is greater than 3, when previously it was the highest gate in the column where it was less than 3 (not mentioned in the draft manuscript). We have also excluded data greater than 100 km in range This was done to account for regions where a detached anvil or cirrus cloud layer was above the precipitating convection and helps to ensure that we are only including the precipitation convection in our analysis instead of remnant anvils. Furthermore, it was found that the year 2013 was missing from the ETH database, so now data from the year 2013 are included. Therefore, the updated processing of the ETHs has resulted in some changes to the conclusions of this study.

In response to another reviewer we have also added an analysis of the convective areas and quantified the number of MCSs using the technique used by Rowe and Houze (2014). We felt that now that this paper covers more than just the ETHs, we have changed the title to "*A 17 year climatology of the macrophysical properties of convection in Darwin.*"

My major criticism is that the description is confusing and hard to follow in parts of the statistical analysis (Sec. 4.1) and diurnal cycle (Sec. 4.2) sections as described in the specific comments below. The confusion is due to 1) combination of too many variables to consider when trying to correlate interpretations stated in the text to results shown in selected figures: active vs inactive monsoon, MJO phase, day vs night, ocean vs land; and 2) the text is not always explicit in terms of which panel of a figure is being used to advance the argument. In consequence, I as the reader, have sometimes come to a different conclusion when interpreting the figures in question compared to the authors. I have also noted these instances below.

We agree that there are quite a few variables that we are stratifying the ETHs and convective areas against and it can be quite overwhelming to the reader. However, the macrophysical properties of convection in Darwin are sensitive to numerous factors including the time of day, the phase of the MJO or the monsoon and even the surface characteristics. We strongly feel that it is important to show how the ETHs can vary as as a function of such characteristics.

Since we are unable to outright eliminate any independent variables from our analysis, we have taken some steps to simplify the presentation of the data to make it easier to interpret:

1. **We now use a more rigorous definition of "congestus" versus "deep convection" that is based off of the threshold used by Kumar et al. (2013), who defined the boundary between congestus and deep convection to be 6.5 km. This, not only being more rigorous and consistent with past literature, also makes the results in Section 4.1 and 4.2 easier to read.**
2. **Every multipanel figure are now labelled by letter and each subpanel is now referred to explicitly.**
3. **The figures showing the ETH distributions as a function of time now just show the frequency distributions with time, reducing the number of figures by 1 and cleaning up clutter. This was done as there was little discussion and few conclusions to be made about how bimodality varies throughout the day.**

Furthermore, the other reviewer requested a quantification of MCSes as well as an an analysis of convective areas in order to more adequately characterize the presence of deep convection than what was done in this manuscript. We have added such analysis to the statistical analysis section.

A more modest critique concerns the spectrum width thresholding technique used to discriminate echo top height as opposed to a minimum reflectivity threshold. On the bright side, the method appears to work reasonably well. However, the end results is that there does not seem to be any real difference in the results when compared to simply applying a minimum reflectivity threshold (which is the traditional approach)
so I am left scratching my head when trying to understand the real advantage of the methodology.

We do not use spectrum width in order to determine the echo top height, but rather, we use the texture of the radial velocity field. This is the standard deviation of the radial velocity of a 3 by 3 gate window surrounding a gate and not the standard deviation of the Doppler spectrum. Radars like CPOL that have a phase that varies randomly from pulse to pulse produce radial velocities that vary randomly gate-to-gate when there is no single scattering returned radiation.

The advantage of such an approach is that (1) the noise floor is automatically detected, (2) we can potentially be able to keep regions in cloud where Z is lower than 5 dBZ, which would be more frequent with 50 km range of CPOL. Finally, this methodology is immune to radar miscalibration and less sensitive to attenuation. This would give us, in theory, more representative of the true cloud top height. While we have removed the "novel" and "new" wording in the abstract, we now more explicitly list these advantages in the introduction and frame the discussion as a sensitivity test. We also have improved the explanation of the velocity texture based ETH retrieval technique so that it is easier to understand both how it works and its potential advantages.

While, for this particular dataset, we arrive at the null conclusion that the ETHs retrieved using the velocity texture methodology and using reflectivity are comparable. We feel that the inclusion of null results in a paper is something that should be done more to guide future research on what methodologies have been tried. This is not done enough in papers in our opinion, and given that this section is short, adding a null result does not significantly lengthen the paper.

Specific Comments 1. P. 4, line 12; please define gate spacing and resolution

We have added the 300 m resolution and gate spacing to this section.

2. P. 5, line 7: What is the spatial resolution of the satellite data? If it's less than radar resolution it's not clear what a relative comparison tells us regarding the performance of the radar-based ETH algorithm.

The spatial resolution of the satellite data at 4 km, so interpolation of the satellite data to the radar's grid was needed.

In response to the other reviewer, we have expanded the analysis in this section to 4 years of MTSAT data from 2006 to 2010, using version 4 of the VISST product. While we acknowledge that the coarser resolution of the satellite introduces uncertainties into the satellite retrieval, over time scales of years the relative seasonal variability in cloud top heights should be captured.

3. P. 5, line 15: similar to previous comment - to understand the differences in cpol vs satellite – what is satellite brightness temp keying off of – what depth of cloud is considered?

The VISST technique uses both the solar and infrared channels at multiple wavelengths to retrieve the cloud properties. According to Cheng et al. (2010), the retrieval is keying off of a height "somewhere

below physical cloud top," but they do not quantify exactly where. We have chosen not to attempt to quantify this since this is extremely difficult to do.

4. P. 5, line 17: cc of 0.49 is not very good
This, while weak, is a statistically significant correlation according to a chi-squared test. The mention of the statistical test has been added to this sentence.

5. P. 5, line 20: this statement assumes the satellite is capturing the variability …

Over timescales of seasons and spatial scales of hundreds of kilometers, we would fully expect this to be the case.

6. Fig. 3 please state in the caption what the color shading represents

We now state that the shading represents the number of counts.

7. P 6. Lines 25-30: In references to Figs 4-5, seems like the big differences are between monsoon phase instead of MJO phase?

For the winds, this is the case. We have noted that there is little difference between the wind speeds between active and inactive MJO conditions.

8. P. 7, line 7 – There are several other older references
that show this behavior: Cifelli and Rutledge 1994 (JAS); 1998 (QJRMS)

We have added these references to this sentence.

9. P. 7, line 27-28: some hint of trade wind layer in MJO=3 for break (Fig. 6)?

We have now changed this sentence to acknowledge that we could be sensing some of the trade wind layer during break conditions, but we ultimately need a radar that is sensitive to cloud particles to characterize it:

"*Also, some evidence of the trade wind mode is visible in Figures 6a-h. However, since the 2 km modes in Johnson et al. (1999) and Kumar et al. (2013) were observed using measurements with a cloud radar that would be more sensitive to liquid cloud droplets than CPOL, more sufficient quantification of this mode would require a radar with a lower minimum discernable signal than CPOL."*

10. P. 8, line 8: This is a minor point but it should be noted that the heights of the different modes that are stated here are approximate. For example, in Fig. 7c the height of mode 2 does not appear to actually reach 15 km
...

We have added the word "approximately" before the quantities in this sentence, and fixed the faulty reference to Figure 7.

11. P. 8, line 12-18: there is some confusion looking
at Fig 7. My read of the red line (A=congestus) in MJO phases 4-7 is
~0.05 – 0.6 for break (Fig 7b) – not 0.8-0.5 as described - and
~0.1-0.4 for monsoon (Fig. 7d) – not > 0.9 as described. Also, the statement on line 14 about unimodality is confusing:

Fig 7b,d show that there is a significant contribution from the congestus mode in break conditions while the MJO is over AU (Fig. 7b, MJO phases 6,7). Similar in monsoon conditions for MJO phase 6 – see Fig 7d. I think the confusion noted above could be avoided by stating more clearly which features in specific figure panels are being referred to.

We have made a more rigorous definition of "congestus" versus "deep" convective modes that makes this section easier to understand. We

determine whether the modes present are congestus or deep convection based on the average location of the modes:

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**

The 6.5 km threshold was chosen based off of Kumar et al. (2013).

We agree, the statement about the unimodality was confusing. We removed it.

12. Fig. 8 – please state in the caption and the figure that this is for break conditions

We have added this information to the caption.

13. P. 9 -please call out panels explicitly in reference to Figs. 8-9

Labels have been added to these panels and now the discussion refers to each relevant panel explicitly.

14. The discussion jumps to Fig 10 before discussing Fig. 9

This is no longer the case.

15. P. 9, line 24: which panel of Fig 10? My read of comparing Fig 10 a and Fig 10b is that during the day there is a higher frequency of deep convection when the MJO is over AU (assume that includes Tiwi islands as well) compared to when MJO is elsewhere.

We have added a reference to Figure 10d.

16. P. 9, 25-26: I don't understand the point about what is being extended in this study vs previous work.

We agree, this sentence was quite confusing. We have rephrased this sentence from:
"*Figure 10d shows a greater frequency of deep convective ETHs over the Tiwi Islands when the MJO is inactive over Australia during the day, which is consistent with increased rainfall over this region.*"

17. P. 10 lines 11-12 – where do the number of days come from?

The number of days comes from the sounding classification in Section 3.3

References:

Chang, F.-L., P. Minnis, J. K. Ayers, M. J. McGill, R. Palikonda, D. A. Spangenberg, W. L. Smith Jr., and C. R. Yost (2010), Evaluation of satellite-based upper troposphere cloud top height retrievals in multilayer cloud conditions during TC4, *J. Geophys. Res.*, 115, D00J05, doi: 10.1029/2009JD013305.