Review: ACP-2018-408-R1

Title: A 17 year climatology of the macrophysical properties of convection in Darwin

Recommendation: Minor Revision.

Summary

The authors have done extensive revision to address many of my comments. I particularly commend the authors to extend their analysis beyond just using the vertical height of convection, but also look into the horizontal dimension of convection, including identifying MCSs explicitly. The added results on convective cell sizes and MCSs nicely complement the ETH analysis of the study. As a result, I think the paper has improved significantly compared to the original manuscript.

There are a few places with some relatively minor remaining issues that should be revised/clarified before the paper is published. Please see my comments in orange below. I recommend minor revision.

Comments

Page 6 line 9-12, actually the daytime satellite retrieval algorithm is referred to as VISST, and the nighttime algorithm is referred to as SIST: https://cloudsway2.larc.nasa.gov (click the VISST/SIST link).

7. There is also the issue of daytime vs. nighttime retrieval differences in the MTSAT data. …

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.

Thank you for attempting to separately compare CPOL and MTSAT echo/cloud-top height retrievals between day vs. night. It's good to know that the results are insensitive to the two different algorithms, suggesting at least that the VISST/SIST provide statistically consistent convective cloud-top height retrievals. More importantly, comparing your original Figure 3 with the new one where the CPOL ETH is now defined as the lowest precipitating convective echo-top, the mean differences as well as the spread is now significantly larger (i.e., CPOL ETH is now significantly lower than MTSAT for cloud-tops above 7.5 km. That means the occurrence of multi-layer clouds above precipitating convective cells are indeed quite frequent, obviously the passive satellite only "sees" the highest layer cloud tops for these optically thick clouds, while the CPOL could detect distinct layers. I think this is a useful result to point out, perhaps you could add a couple sentences describing the difference when comparing CPOL lowest layer ETH vs. max ETH.

Figure 4. I don't think the temperature profile (Fig. 4b) provides much useful information. Similarly, dew point temperature and specific humidity is also somewhat duplicative. You also did not specifically discuss these quantities in the text. Relative

humidity is a more useful quantity that differentiates probability of transitioning to deep convection. I suggest you could simplify Fig. 4, 5 with just 4 panels, theta E, RH, U, V.

Page 7 line 33-34, “This shows that there are a greater number of cases with westerly flow advecting moisture from the Indian Ocean when the MJO is active over Australia.” I think the 95th percentile U wind being larger when MJO is over Australia in Fig. 4e, 5e only means the tail of the zonal wind is stronger. That does not necessarily mean moisture advection is larger. You could have stronger zonal wind with drier air. What would support your claim is to compare moisture flux (U * qv) profiles.

Figure 7d, what concerns me is the complete reverse between 100% deep convection in MJO phase 1 to 100% congestus in MJO phase 2. Is it reasonable to believe that: 1) 100% of convective clouds during when MJO is away from Australia are all deep convection (or congestus)? 2) All of the deep convective clouds suddenly all changed to congestus in the next MJO phase? You also ignore this figure in the text when discussing Fig. 7 on page 10. I don’t think I understand what u1 and u2 mean, and how are the fractional contribution of modes in Fig. 7b,d are calculated.

I find the new Figure 8 quite interesting. Thank you for adding this analysis. The result suggests that when MJO is over Australia, monsoon/break has no effect for the population of convective cells. But when MJO is away from Australia, monsoon periods have narrower distribution of convective cell sizes, and also less frequent large convective cells than break period. I think this could be one of the highlighted results of the paper.

Table 1 caption is misleading, the numbers in the table are frequencies, not “average number of MCSs”. The caption also has some missing information: “… using the criteria of (?)”.

Page 11 line 15-16, Table 1 only provides MCS frequencies separately for break/monsoon, and MJO away/over Australia (unless I misunderstood). But it does not provide frequencies during break+MJO away from Australia, break+MJO over Australia, monsoon+MJO away, monsoon+MJO over. So it is inconsistent with the discussions in Fig. 7 in this paragraph, which combines the monsoon and MJO conditions. How about adding the MCS frequency calculations for these combined conditions in the table to align better with the 4 key large-scale conditions of the paper?

Page 13 line 12-14, “Since MCSs are larger …, the reduced frequency of MCSs in the inactive phase of the MJO in Table 1.” The last part of the sentence seems to have some missing words.