Review of "Tropical cirrus clouds of convective and non-convective origins" by D. Huang and Dinh

The authors use CALIPSO measurements of tropical cirrus, along with ERA5 reanalysis temperature and specific humidity fields, to categorize cirrus as either convective or non-convective. They define convective clouds as those for which the specific humidity is greater than the annual mean at the location where the clouds were observed. As described below, I am skeptical that the paper adds any new understanding of the roles of convection and non-convective processes in governing the occurrence of cirrus clouds, which is the stated goal of the paper. Further, I believe the authors' definition of convective versus non-convective cirrus is misleading because it includes cirrus formed in situ in airmasses with relatively high specific humidity that might (or might not) have been caused by convection somewhere upstream of the observed cirrus.

Major comments:

1. As noted by the authors, their definition of "convective-origin" cirrus includes clouds that form well downstream of the convection in airmasses with relatively high specific humidity. This definition is much broader than the conventional definition of convective cirrus, which is limited to cirrus produced directly by the deep convection. Taken to the extreme, the authors' definition could actually include all cirrus in the tropical upper troposphere since deep convection is the primary source of upper tropospheric water vapor. The abstract states that convective cirrus are three times more common than non-convective cirrus. I fear readers will take this statement as the key message of the paper without understanding that the authors' definition of convective cirrus includes any clouds formed downstream of locations where deep convection hydrated the upper troposphere. I believe the authors should choose different terminology to avoid sending a misleading message.

A related issue is that convection is not the only mechanism that can produce specific humidity at a particular location that is higher than the climatological mean. Transport of water from moist to dry regions can also produce a positive humidity anomaly. Further, the mean values used here are apparently annual means (the authors need to be more explicit on this point), in which case the specific humidity anomalies they are using could be dominated by seasonal variability. An alternate approach would be to use regional, seasonal (or subseasonal) mean temperature and humidity based on averages over some nearby domain and limited time period.

2. As noted above, the authors state in the abstract that most tropical cirrus originates from convection. However, their results show a strong height variation in the relative abundance of convective versus non-convective cirrus, with the former dominating below about 15 km, and the latter dominating in the upper TTL. This result is consistent with other data analysis and modeling studies. For example, Jensen et al. (2017) and Schoeberl et al. (2019) used satellite and airborne measurements of clouds and humidity to show that temperature effects dominate in the upper TTL, whereas deep convection controls clouds and humidity in the lower TTL. A number of modeling studies have shown that the observed occurrence frequency and regional distribution of TTL cirrus can be reproduced with only in situ cloud formation (e.g., Ueyama et al., 2015; Ueyama et al., 2018; Schoeberl et al., 2019). Therefore, the conclusion that convection dominates clouds and humidity in the lower TTL, whereas temperature variability dominates cirrus formation in the upper TTL is certainly not new.

As noted by the authors, a number of past studies have shown that TTL cirrus tend to form in anomalously cold airmasses. Therefore, the authors' finding that most cirrus in the TTL occur in times and locations where the temperature is below the climatological mean is no surprise. In general, I do not think the analysis here really clarifies the relationship between convection and tropical cirrus beyond what was already known.

Minor comments:

1. Abstract, lines 8-9: The authors state that "The remaining clouds that are not directly influenced by convection are driven by negative temperature perturbations." As shown later in the paper, this statement is not correct. Some of the clouds with negative SPH perturbations have positive temperature perturbations. In fact, this "unidentified" category comprises 10% of the cloud samples, which is not negligible.

2. Line 32: The authors should cite *Forster and Shine* (2002) since this paper quantified the impact of stratospheric humidity on the radiation budget well before the papers cited.

3. Line 48: When discussing dissipation of convective cirrus, the authors mention cloud horizontal spreading and sublimation, and they cite a series of Dinh et al. papers. However, these papers did not address the evolution of convective cirrus; further, other studies have shown that sedimentation is the dominant process reducing the IWC of convective cirrus *Boehm et al.* (1999); *Jensen et al.* (2018).

4. Lines 60-61: The authors state that "*Ueyama et al.* (2015) calculated backward trajectories that end at the tropopause..." The study actually used curtains along trajectories to simulate TTL cirrus. Further, the authors state that *Luo and Rossow* (2004) "simulated clouds along the trajectories." As far as I can tell, this paper did not use cloud simulations. They simply tracked the clouds in the satellite imagery.

5. Lines 68-69: In addition to the papers cited here, *Jensen and et al.* (2020) recently documented cases of convective hydration of the lower stratosphere.

6. Lines 117-121: The mean specific humidity and temperature are apparently averaged over the entire data time period (although this is not stated explicitly). This would mean the averages are climatological, annual means, and the resulting correlations between cloud occurrence, humidity anomalies, and temperature anomalies will largely just represent the seasonal variations in these quantities. I think it would make more sense to use seasonal means for the clear-sky averages.

7. Lines 171-177: The authors note that the peak of the non-convective cloud frequency occurs below the CPT, and as an explanation, they cite the modeling study indicating that radiatively-driven circulations will be damped if the temperature in the cloud increases with height. First, there is no observational evidence showing that radiatively-driven circulations routinely occur in cirrus near the tropopause, and the lifetime of TTL cirrus (limited by wave-driven temperature perturbations) is typically too short for these circulations to develop (*Jensen et al.*, 2011). Second, there is a much simpler explanation for the peak cloud frequency altitude occurring below the tropical mean CPT: The CPT altitude varies considerably from location to location and time to time. Therefore, at the mean CPT altitude, you are often above the local CPT, in which case cloud formation would be suppressed. As shown by *Pan and Munchak* (2011), TTL cirrus cloud tops generally occur just below the local CPT.

8. Lines 185-192: The authors use their convective occurrence height distribution to estimate the frequency of overshooting above the CPT. I do not believe there is any quantitative value to this calculation. First, it is entirely possible that the few convective clouds (with positive SPH anomalies) observed above the CPT are just places where the SPH is above the climatological mean with no relationship to deep convection. Second, as noted above, the CPT height varies spatially and temporally. Therefore, for this calculation to have any value, the authors would need

to carefully determine which of the clouds are above the *local* CPT.

References

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