Second review of “The evolution of deep convective systems and their associated cirrus outflows” by George Horner and Edward Gryspeerdt

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I appreciate the authors’ additional anylysis to differentiate between detrained cirrus and in-situ cirrus that form after the detrained cirrus dissipated. However, I am concerned that the method used to do this is not correct. The authors stated that whenever the cloud fraction of the detrained cirrus has dropped below 10 % for the first time, then any subsequent cirrus are considered to form in situ. The authors stated that this method is similar to that used by Luo and Rossow (2004), but I don’t think that this is the case.

For the purpose of this discussion, I have taken the liberty to copy the text describing the method to differentiate detrained and in-situ cirrus by Luo and Rossow (2004) here: “We consider two types of tropical cirrus based on their relationship to convective systems: detrainment cirrus and in situ cirrus. The life cycle for detrainment cirrus is defined by a monotonic decrease of cirrus amount along a forward trajectory like this: last convection becoming some cirrus becoming zero cirrus. The length of this sequence defines the detrainment cirrus lifetime. For in situ cirrus, the sequence is appearance of cirrus with first an increase and then a decrease of cirrus amount like this: zero cirrus (observed at least 1 day after convection) becoming some cirrus becoming zero cirrus. The requirement that the air parcel encounter clear skies after convection ensures the separate evolution of the in situ cirrus.” . . . “In practice, we take zero cirrus to be 1/5 of the maximum cirrus cloud amount along the lifecycle sequence. For example, when cirrus overcast decays below 0.20, we call this the end of the life cycle. But if the maximum cirrus amount along a selected trajectory is only 0.20, then it has to drop below 0.04 to end the life cycle.”

Figures 8(a) and 8(b) of this manuscript show that the cloud fraction at TSC = 0 is 0.3 on average. Therefore, if we apply the method used by Luo and Rossow (2004), then on average the detrained cirrus would disappear when the cloud fraction is reduced below $0.3/5 = 0.06 = 6\,\%$, not 10 %. Moreover, the maximum cloud fraction obtained at TSC = 0 may be different for different trajectories, so the threshold to determine the disappearance of the detrained cirrus would change from one trajectory to the next.

Figure 3(b) does not make sense to me, either. For TSC less than about 10–15 hours, the number of counts of in-situ cirrus is not visible because it is shown beneath the number of counts of detrained cirrus, but let me assume that in this figure these two numbers are equal for TSC less than about 10–15 hours. If this is what’s plotted in the figure, I don’t understand why there would be so many in-situ cirrus already for small TSC.

The authors aim to demonstrate that convection has a significant long-lasting impact on the properties of clouds. However, in Fig. 7(b), the clouds at 370 hPa do not appear to be connected to the initial convection. Please see also my comment on the original version of the manuscript about these clouds. It appears to me from Fig. 7(b) that these clouds are neither convectively detrained nor formed in-situ from the moisture perturbation brought about by the convection. The presence of these clouds certainly affects the average cloud properties, for example, cloud radiative effect (CRE). It follows that the CRE shown in Fig. 10 is not purely from clouds associated with convection and cannot be used to demonstrate the impact of convection on CRE. I believe this is a major issue with this study.

Finally, a specific issue to be fixed is the caption of Fig. 9. In the current version of the manuscript, it is incorrectly identical to the caption of Fig. 10.

References

Z. Luo and W. B. Rossow. Characterizing Tropical Cirrus Life Cycle, Evolution, and Interaction with Upper-Tropospheric Water Vapor Using Lagrangian Trajectory Analysis of Satellite Observations. *J. Climate*, 17(23):4541–4563, 2004.