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

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The authors aim to study the evolution of deep convective systems and their associated cirrus outflows, as the title of the manuscript suggests. As far as I understand, the most significant finding of this work compared to existing literature is the sustained warming effect of convectively detrained cirrus clouds. The authors argue that this effect is sustained past 5 d since the time of the last deep convective event. My main concern is whether the cirrus clouds captured along the trajectories, especially at an increasingly long time since convection (TSC), are actually associated with convection. If the goal of the paper is to evaluate the radiative effect of the clouds associated with convection, the authors would need to properly separate the clouds associated with convection and those that are not. The authors have not provided the analysis to do this.

The main argument that the authors gave to justify that deep convection leads to the formation of the clouds along the trajectories is the expectation of changes in cloud properties with the TSC. Specifically, as the anvil cirrus decays into thin cirrus, it is expected that the cloud fraction decreases, the cloud top pressure increases (as clouds descend to lower altitudes), and the cloud optical depth decreases. These behaviours are seen clearly in the data the first 24 h (Fig. 7), but then the signals become weak (Fig. 6) and/or disconnected in time and/or space (Fig. 7). In Fig. 7(b), high clouds are seen at 250 hPa, 310 hPa, and 370 hPa, but only the clouds at 250 hPa and 310 hPa appear to be connected to the convective anvil, while the clouds at 370 hPa do not. Where do the clouds at 370 hPa come from? It is odd that clouds appear at 370 hPa at TSC = 10 h before the thick convective anvil above dissipates. Is this an artifact of the data and/or the method? If so, is the data and/or method trustworthy? In addition, the cloud top pressures of the high clouds at respectively 250 hPa, 310 hPa, and 370 hPa do not increase with time (Fig. 7b) as expected, and the optical depth of the clouds does not decrease with time either after about 30 h (Figs. 7c and 7d).

Figures 8(d) and 8(f) show that the convective outflow decays within 36 h–42 h (line 285 in the manuscript). Subsequent anomalies in ice number concentration are not discussed in the manuscript, but they appear to me to be disconnected to the convective outflow anvil in these figures. In short, both Figs. 7 and 8 suggest that a large number clouds along the trajectories may not be associated with convection (or at least not directly), particularly for long TSC.

Besides the lack of identification of clouds that originate from convection and those that do not, the other issue I see is the error and uncertainty associated with the trajectory calculation, which increases with TSC. This means that the results for TSC > 5 d may not be meaningful. The error associated with omitting the vertical motions and using the wind averaged between 200 hPa and 300 hPa has been discussed in the manuscript (Section 2.5). However, I am concerned with the error associated with the method of calculating the TSC, in which if a grid box becomes empty, then the value for the TSC in that grid box is taken as the mean value of the surrounding grid boxes (as stated in lines 343–344 in the manuscript). How much is the error that this introduces? In addition, the number of retrievals decreases with TSC. Figure 7 shows the distribution of clouds along the trajectories, but it has been normalized so we do not see the actual number of retrievals. How many retrievals are there for TSC longer than 5 d? Is this number sufficiently large to guarantee a reliable result? I suppose that a detailed analysis of the error and uncertainty as TSC increases is needed. Given the error and uncertainty, should the trajectory analysis be limited for TSC < 5 d? But if so, what would be the new finding of this work compared to Luo and Rossow (2004)'s? Recall that Luo and Rossow (2004) already performed forward trajectory calculations for 5 d starting from deep convective systems to address a similar topic.

In summary, the major issue of this work is the lack of identification of clouds that originate from convection and those that do not. In addition, the error and uncertainty associated with the trajectory calculation method have not been assessed properly. I hope that the authors will consider these points to improve their work before it can be published in ACP.

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.