Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2022-755-RC2, 2023

Comment on acp-2022-755

Anonymous Referee #2

Referee comment on "The evolution of deep convective systems and their associated cirrus outflows" by George Horner and Edward Gryspeerdt, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-755-RC2, 2023

The paper by Horner and Gryspeerdt examines the evolution of anvils cirrus with time using a novel methodology to track air parcels that have been associated with deep convection. They then attempt to discern the evolution of the cloud properties and radiative effects with time. The tendency for the parcels to move poleward from tropical deep convection is an interesting result. The authors find that the longwave heating by cirrus become increasingly important with time as the cirrus associated with deep convection thins. These results are in accordance with previously published results. I feel that this study can be an important contribution to our understanding of the role of cirrus in maintaining the tropical radiative energy budget. However, the authors have failed to adequately explain their methodology and I am left with many questions regarding their results and conclusions. I feel that additional work is needed before this paper is suitable for publication.

My main criticism of the paper is that the analysis method is not clearly explained. Their fundamental source of information is ISCCP retrievals. They then combine this with reanalysis, with cloudsat and calipso data, and with CERES flux data. All these data sets have highly variable spatial and temporal resolutions that can influence their results. How they merge this disparate information is not clearly explained. While the use of cloudsat and calipso is innovative, matching the active remote sensor curtains with ISCCP data seems fraught with uncertainty and sampling issues. This is especially true given the 16-day repeat time of cloudsat and calipso. This all needs to be addressed in more detail and sensitivity to assumptions and sampling quantified. A case study would have been interesting to illustrate the methodology.

Specific Comments:

Line 54: See Schwartz and Mace (2010; *doi:10.1029/2009JD012778) who used Cloudsat and Calipso data to examine the mechanisms proposed by Garret and Hartmann et al*

Line 97: Reader should not have to dig Teslioudis et al (2021) to understand the data product so more information on the ISCCP H product is needed. Daytime only, I assume? How does it differ from earlier ISCCP products, etc?

Line 99: I don't know what a nearest neighbor algorithm means. More information is needed and a reference would be helpful.

Line 103: Given that a realistic optical depth of a convective core on a 1 km scale is on the order of 100, an optical depth of 8.5 is an interesting choice. The old ISCCP data had an optical depth bin of 23. Why is 8.5 chosen here?

Line 105: Where does this expression come from? What is 0.895?

Line 106: Is this the old ISCCP simulator described by Jakob and Klein?

Line 190: Assuming a cloudsat footprint of 2km, and a 100x100 km ISCCP footprint, there are 2500 2x2km pixels in an isccp grid box. A diagonal across the middle of the box (the maximum that could be achieved) gives about 450 pixels. How is this disparity in sampling accounted for in a highly variable field? Are the data averaged?   Since cloudsat makes 14 passes over the tropics in a day, how is the disparity in temporal sampling accounted for? The authors present only 2 years of data.

Line 193: What is the resolution of the CERES data? The native footpring of ceres is 20 km. Is the CERES data averaged? Also, CERES passes over the tropics 14 times per day on MODIS. Does it provide global coverage in a day? ISCCP H has 3 hourly resolution. How are these data products merged?

Line 224: If the mean TSC is 180 hours, it would seem that all air has been in contact with convection by day 5.   At some point it seems impossible to associate the clouds with convection.

Line 227: The authors claim that the TSC is relevant to 360 hours. I dont' seem how this can be surmised. the air moves more and more into regions of subsidence with time. large-scale subsidence may be as much responsible for the change as some sort of post convection decay of anvil cirrus. Can this possibility be discounted? I think the tendency for the cirrus move out of the tropics with time into the large-scale subsidence as illustrated in Figure 5 is one of the more interesting results presented in the paper.

Line 265: The authors need to explain how the DARDAR product is able to deduce Ni when the anvils are optically thick for up to 1-day TSC? This implies that only the radar is able to provide credible information to the retrieval. Even when optically thin, the authors need to comment on the validity and uncertainty of the Ni retrieval given that that the lidar is a cross sectional area constraint and the radar a mass-squared constraint. With the shape of the ice crystals unknown, converting the remote sensing into Ni is fraught with uncertainty. Might it be possible that any Ni signal the authors infer are due to changes in ice crystal habit over time? An evolution in habit as the anvils age would result in a time-dependent ratio of mass and area that is not accounted for in DARDAR.