Reviewer 1

: The study by Horner and Gryspeerdt examining the evolution of tropical cirrus as function of time since interaction with deep convection introduces a new method for determining the time displacement since air parcels in the upper troposphere have been in contact with deep convection. While there do seem to be some uncertainties with the method associated with the injection height and the wind used for transport, their general approach seems reasonable. The authors find that cirrus continue to have an important radiative heating in the upper troposphere well beyond the time of injection by deep convection with some signal still evident beyond 120 hours. The authors also examine the properties of these clouds using the new ISCCP dataset, Cloudsat and CALIPSO-derived cloud properties, and CERES cloud radiative effect data sets. In general, I find the study to be very well done with the conclusions supported by the evidence presented in the paper.

Reply: We thank the reviewer for their helpful comments on our paper. We address their concerns below. Line numbers refer to submitted manuscript, not the diffed version.

General comments:

: My primary concern with the study is the interpretation of the ISCCP data. The authors treat the ISCCP data as a literal rendering of cloud type much as one would interpret active remote sensing data to place clouds accurately in the vertical column. ISCCP retrievals have been interpreted, incorrectly, in this way since the product was first introduced by Rossow and Schiffer in 1989. However, I will quote the conclusion of a paper by Chen and Del Genio in 2009: "ISCCP CTPTAU histograms are neither what they were intended to be (a distribution of highest cloud-top heights) nor what they are sometimes mistaken to be (an actual vertical distribution of clouds),but are instead a hybrid of both." This is especially true for cirrus that very often exist above broken low-level clouds and it is one of the reasons why Klein and Jakob (1999) developed the ISCCP simulator software so that models output could be compared with ISCCP. In Mace and Wrenn (2013), we examined the vertical distribution of cloud layers observed by active remote sensors within the ISCCP CTP-tau histograms. We found that a significant fraction of mid-level clouds (up to half) in the tropics are high-level clouds incorrectly diagnosed to have clout top pressures in the middle instead of upper troposphere. While it is not my opinion that this issue negates the central findings of this study, the authors need to consider their interpretation of Figure 6. It is my opinion that the increase in mid-level clouds seen in successive periods in Figure 6 could be caused by continual misdiagnosis of cirrus as mid-level clouds. This would occur when optically thin high clouds move over the greater low level cloud fractions in the trade cumulus regions on the poleward reaches of the tropics. The CRE results in figure 10 also need to be reconsidered as underestimating the effect of thin cirrus. At the very least,

the authors need to comment on this issue and acknowledge the potential for the ISCCP data to not provide a literal rendering of cloud top pressure or optical depth.

Reply: We are very grateful to the reviewer for these helpful comments in diagnosing the presence of the mid-level clouds. We are aware of these particular issues with the ISCCP data, and have included a paragraph in the discussion section that acknowledges these issues. See lines 415-425: ***Furthermore, it is important to note that the ISCCP dataset can provide a biased representation of cloud top pressure and cloud optical depth. As stated in Chen and Del Genio (2009), the ISCCP CTP-TAU histograms are not an actual vertical distribution of clouds. This means that when considering Fig. 6., the apparent cloud layer that appears at 370 hPa should not be taken to be a true appearance of the clouds at this height. More likely, it is the existence of thin high clouds sitting over low-cloud that causes ISCCP to incorrectly diagnose them as lower level clouds (Mace and Wrenn, 2013). This occurs when the optically thick deep convection dissipates, and the thin cirrus moves over a region of continuous low cloud in the trade cumulus regions. The issue with the ISCCP mid level clouds is not concerning, and should not impact our subsequent results, in particular the CRE calculations in Fig. 10. To isolate our high cloud regions for the CRE calculations we select bins with very little low clouds, making it unlikely that this mid-level cloud effect drives the results in Fig. 10.***

: I also wonder why the authors chose to present the ice crystal number concentration (Ni) in the DARDAR results in Figure 8? Ni is the least reliable parameter of this retrieval and probably represents the least understood parameter and the least interesting in my opinion. Of much more interest would be the evolution of the ice water content and effective radius since those parameters are directly related to the radiative effects of the cirrus. Given the general decrease in Ni with time in the cirrus layer, I wonder if the water content is decreasing (as it should given the optical thickness trend) or if the effective radius is changing with time, or both? One would also expect there to be some amount of size sorting with larger effective radii lower in the cirrus layer. Results using the CloudSat 2C-ICE data set should also be generated and compared with the DARDAR results since both are retrievals with assumptions and likely have considerable uncertainties. The two algorithms, while using the same inputs, are independent in the manner that the retrievals are conducted.

Reply: The reviewer makes a useful recommendation to consider analysing other microphysical properties from DARDAR and CloudSat 2c-ice data. In this paper, Ni was used merely to illustrate the use of the DARDAR data for investigating ice microphysical properties. This analysis is planned for future work that will further examine the behaviour of cloud properties as a function of TSC, where we intend to consider the useful suggestions made here.

Reviewer 2

: I appreciate the authors' additional anlysis 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.
Reply: We thank the reviewer for their helpful comments on our paper. It is evident that we should be more clear in stating that whilst we do believe our method to be similar to Luo and Rossow (2004), it is not the same method. As mentioned by the reviewer, Luo and Rossow (2004) define the end of lifetime for the detrained cirrus (i.e. their 'zero cirrus' case), to be 1/5 of the maximum cirrus cloud amount. In our paper, we define the 'zero cirrus' case to be a 10% cloud fraction. In practice, these methods are very similar, because the maximum cloud amount along a trajectory is, due to the definition used for deep convection, very close to 100% cloud fraction. So a 10% cloud fraction is not significantly different from 10% of the maximum cloud fraction. We have rephrased lines 163-164 to make it clear that our method is similar, but not identical to, Luo and Rossow (2004): ***This is a similar method used in Luo and Rossow (2004), who use a threshold of 20% of the maximum cirrus cloud fraction along a trajectory to identify the 'zero detrained cirrus' case, rather than a 10% cloud fraction threshold as used in this work.***

General comments:

: 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.
Reply: The reviewer is using the cloud amount from DARDAR in Figure 8(a) and 8(b), however in our paper the cloud fraction used to determine whether the threshold has been met is from ISCCP. The maximum cloud fraction from ISCCP is, by design, very close to 100% for TSC=0, as this is a requirement for the identification of deep convection. This data source has been explicitly stated on line 160: ***Once the ISCCP cloud fraction drops below 10% along a given trajectory[...]***

: 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.

Reply: The histogram in 3(b) is additive, i.e. the detrained component and in situ component are stacked on top of each other. We appreciate this isn't immediately clear, so have amended the histogram so that the detrained and cirrus components are overlaid in the histogram. As is hopefully clear from this histogram, there is significantly more detrained cirrus than in situ cirrus at 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.

Reply: The clouds in this 370hPa band are an artifact of both the ISCCP histogram bin aliasing to those used in Fig. 7b), and a retrieval bias in the ISCCP dataset, rather than indicating any true cloud that exists. Note that this bin aliasing doesn't appear on Fig.7a) as the wider bins are less likely to suffer from aliasing effect. The retrieval bias means the ISCCP histograms underestimate the thin high cloud that sits over low cloud, mischaracterising them instead as mid-level clouds (Mace and Wren, 2013). This retrieval bias maybe also contribute to the increase in the mid level cloud at 370hPa. They become more visible as the optically thick convective clouds dissipate. These results should not affect the CRE in Fig. 10. significantly, as we define the presence of high cloud as a deficit of low cloud. This means we aren't relying on the amount of high cloud (which we believe to be underestimated) to define where we expect high cloud to be in the high cloud CRE. We appreciate the need for clarifcation on this point, therefore we have included an extra paragraph in the discussion section to address the shortcomings of the ISCCP dataset on lines 416-425:

Furthermore, it is important to note that the ISCCP dataset does not necessarily provide a true representation of cloud top pressure or cloud optical depth. As stated in Chen and Del Genio (2009), the ISCCP CTP-TAU histograms are not an actual vertical distribution of clouds. This means that when considering Fig. 6., the apparent cloud layer that appears at 370 hPa should not be taken to be a true appearance of the clouds at this height. More likely, it is the existence of thin high clouds sitting over low-cloud that causes ISCCP to incor-

rectly diagnose them as lower level clouds (Mace and Wrenn, 2013). This occurs when the optically thick deep convection dissipates, and the thin cirrus moves over a region of continuous low cloud in the trade cumulus regions. The DARDAR dataset and Fig. 8. is particularly useful here as it can provide us with a more reliable vertical distribution of the cloud amount. The issue with the ISCCP mid level clouds is not concerning, and should not impact our subsequent results, in particular the CRE calculations in Fig. 10. To isolate our high cloud regions for the CRE calculations we select bins with very little low clouds, making it unlikely that this mid-level cloud effect drives the results in Fig. 10.

: 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

Reply: We thank the reviewer for raising this specific issue. This has been fixed, with a new caption included for Figure 9.: ***Zonally averaged DARDAR vertical cloud amount for a given TSC bin. TSC bins are given in the top right corner.***