Reviewer 1

: 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.

Reply: We thank the reviewer for their comments which we address in turn below. The goal of the paper is not to track the evolution of exclusively detrained clouds from convection, as this has been done before in mesoscale studies (e.g. Luo and Rossow, 2004). This study instead investigates the evolution of air parcels (and the associated cloud properties) advected from convective cores. While some of the clouds studied will have detrained from convection, as the reviewers note, this study is not limited to these clouds. In contrast to previous studies, this work uses time since convection (TSC) to characterise all tropical high clouds and their radiative properties, irrespective of whether they were directly detrained from convective cores. We also include new analysis of how TSC varies as a function of latitude. We have modified the text in the introduction, discussion and conclusion to better highlight this difference to previous work.

During the preparation of this response, the method for interpolating between diverging trajectories has been modified to be more consistent with the description in the methods. These changes are discussed in detail on Page 3 of the response, but have a minimal impact on the results presented in this work.

We have also harmonised some of the terminology, using ‘pixel’ to refer to an element of the 0.1° by 0.1° used for the TSC calculation, where ’gridbox’ is an element of the 1° by 1° grid.

General comments:

: 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).

Reply: We agree that the signal becomes weaker at longer times since convection, but suggest that this is expected. As the reviewer notes, the behaviour of clouds at longer times is less likely to be related to the properties of the initial convection. This will be addressed in future work. However, we note that cloud properties still vary as a function of TSC even at very large TSC values. We have included a new section of analysis and extra plot into the paper that investigates the role that latitude plays in the evolution of the cloud properties, and how the influence of TSC changes as air parcels get further from convection (i.e. at large TSC values). These results are particularly important to isolate the direct influence of TSC on the changes in cloud properties along trajectories, and highlights that these changes aren't merely due to latitudinal influence on cloud properties. This begins on line 315 in the section 'TSC as a function of latitude'.

: 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).

Reply: It is difficult to make a definite statement about the original of the clouds at 370hPa due to the overlying high cloud - but we note that they become visible as the thick initial layer of cloud at 200-250hPa dissipates. This interpretation is supported by the DARDAR cross section plots (Figs. 8a,b), which do not show the appearance of a new layer at 8/9km (approximately 370hPa). We have modified the text to make it clearer that the these clouds may not be directly related to the initial convection on lines 272-275: *There is a significant cloud layer at 370hPa that becomes visible as the high cloud dissipates. These are likely clouds not associated with convection that become visible as the cloud fraction of the anvil cloud above decreases, and any changes in the properties of these clouds, particularly at large TSC values, are not suggested to be directly related to the initial convection.*

: 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.

Reply: A number of clouds along the trajectories may indeed be disconnected from the initial convective event. It is reasonable to assume that the number of clouds unrelated to convection will increase as a function of time since convection. However it is still of scientific value to investigate how cloud properties from clouds not directly detrained from deep convection may change as a function of time since convection, due to the impact of convection on the properties of the UTLS region, including humidity and aerosol environment. As highlighted above, this point has been clarified on lines 272-275, with a discussion section included on line 315 and a plot included that shows how the cloud distribution is independent of latitude for a given TSC value.

: 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?

Reply: This is indeed an important point and we have investigated it further to better characterise the related uncertainty. Divergence leaving an empty gridbox occurs on average in 5% of the 0.1 x 0.1 degree gridboxes at each timestep. The mean distribution of the divergence across the tropics is shown in Figure R1 and is now included in the supplementary information.

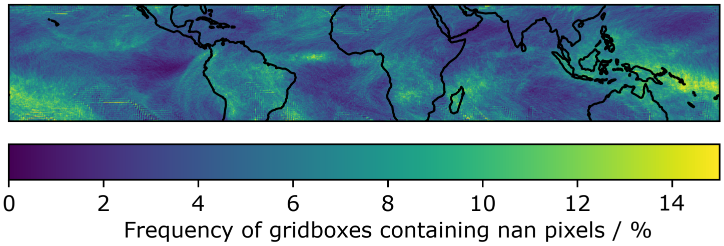

Figure R1: Frequency of occurrence of divergence producing a gridbox that must be filled by interpolation.

The percentage of gridboxes that diverge is greater in regions of low time since convection (compare R1 to Fig. 5b in the manuscript), where as many as 15% of pixels diverge enough to require interpolation at any timestep. This is unsurprising due to the nature of the diverging wind field at this height and in these regions. However, the nature of this divergence, early in the lifetime of the convective outflow, means that the interpolation used to fill these empty gridboxes is primarily from gridboxes that themselves all have very similar values of TSC. As shown in Fig. R2, in the majority of cases where interpolation is

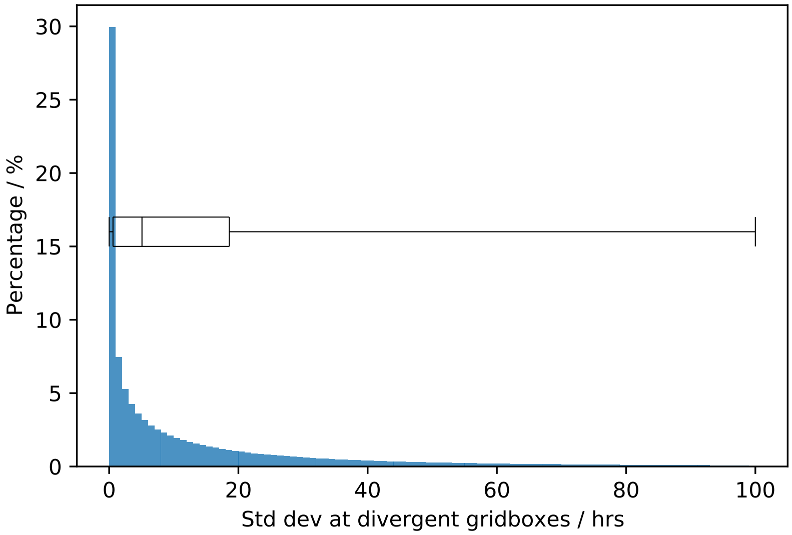

Figure R2: Histogram of the standard deviation of pixels surrounding a divergent gridbox. These neighbouring pixels are averaged to fill the TSC in the divergent region. The lower quartile is 0.6, the upper quartile is 18.6 and the median standard deviation is 5.08 hours.

required, the neighboring pixels all have the same TSC (a standard deviation of zero). Over half of all cases has a standard deviation of pixels used for the interpolation of 3 or less, such that very little uncertainty is introduced through this interpolation process.

As mentioned above, the actual method for the interpolation was not clear from the description provided. The previous method was used to fill in the divergent gridboxes with the average TSC from all the pixels within the same 1°by 1°gridbox. The updated method uses only an average of the neighbouring 0.1°by 0.1°pixels.

Despite this difference in the interpolation process, there was a neglibile difference in the results presented in this work. Figure R3 shows the difference in the median TSC between the old and new method. The TSC is on average a few hours lower in regions of lower TSC in the new method, as we are only including the neighbouring pixels to the divergent pixels in the interpolation, which usually occurs in regions of low TSC.

The methods section has been rewritten for clarity and to account for the change in method on lines 141-147: *The TSC array is interpolated into missing regions after each advection timestep (Fig. 2). When each 0.1x0.1° pixel in the TSC array is advected forward, some of the trajectories converge to occupy the same pixel, necessarily leaving some pixels without a TSC value. When this* *occurs, the missing values are interpolated as an average of TSC values around this empty pixel. Between each timestep, divergence leaves approximately 5% of the pixels empty. When two trajectories enter the same pixel, they will proceed to follow the same trajectory ad infinitum due to the deterministic nature of the trajectories. Therefore the trajectory with the smaller TSC value is the one that determines the TSC value at that pixel from that point on. This increases confidence that any high TSC value really represents air at such long timescales since convection.*

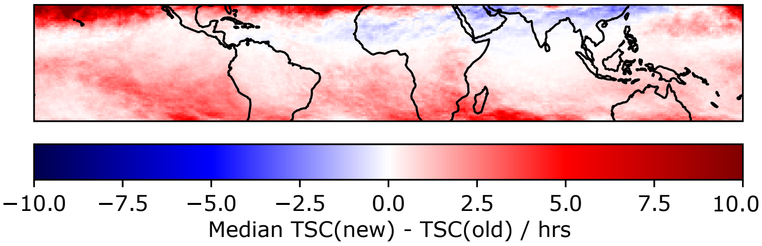

Figure R3: Difference between old median TSC and median TSC with new method.

: 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.

Reply: We thank the reviewer for raising this point that the histogram shown in Figure 3 doesn't show the number of retrievals for each TSC value. While the peak in the TSC histogram is close to 0, more than 50% of the TSC values are at times longer than 120hrs. We have added a cumulative distribution to Fig. 3 to make this clearer and noted that there are over 540 million retrievals in the four years TSC data.

Figure R4 shows the labelled histogram that is added to Figure 3 in the paper.

: 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.

Reply: We agree that there is an increased uncertainty at longer TSC values. However we still see value in investigating changes at these longer times. As

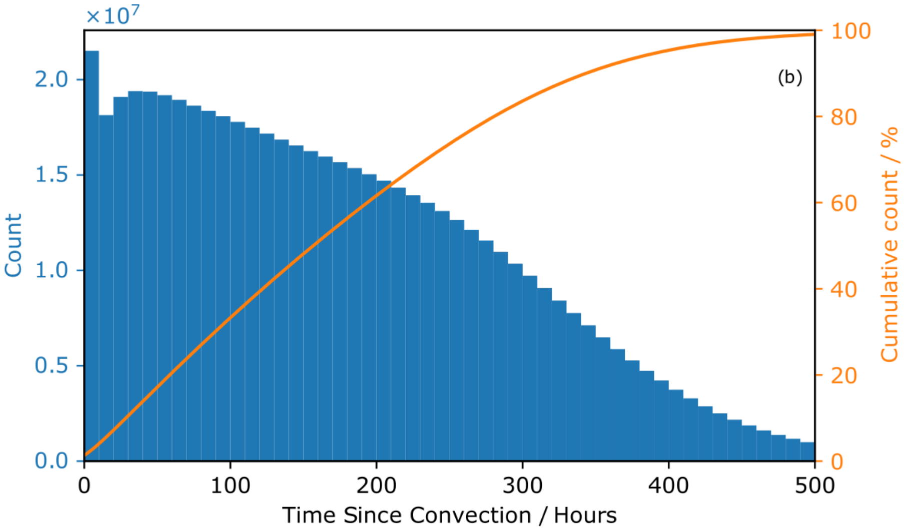

Figure R4: Histogram of the TSC distribution for 2007-2010.

demonstrated in Figs. 6 and 8, changes in cloud properties as a function of TSC are still observed at several hundred hours since convection. This alone is a novel result that was not possible with previous methodology.

For the part of this work that focuses on the period within 120 hours (5 days) since convection, there are still significant advances in this work compared to the important work of Luo and Rossow (2004).

Firstly, Luo and Rossow tracked individual convective events for a limited time. This work creates a TSC dataset for the entire tropics such that we do not track individual convection but rather build a composite picture of convective evolution for the entire tropics, enabling the analysis of divergence and intersections of individual trajectories.

This work also introduces novel analysis of the vertical evolution of the cloud field through the use of the DARDAR dataset. This provides a new picture of the evolution of clouds following convection and accounts for the overlying high cloud issue with ISCCP data used in previous work.

Finally, the radiative evolution in this work is novel, isolating the radiative effects of cirrus clouds along the trajectories.

Wording to better highlight the novel aspects of this work is included in lines 83-91: *Most previous work focused on individual mesoscale studies and capped the analysis at shorter time scales in the region of 120 hours. In contrast, this paper presents a method to run a Lagrangian trajectory analysis across the entire tropics at unbounded trajectory lengths at a low computational cost, leading to changes being found in the cloud properties at time scales far beyond 120 hours after the initial convection has dissipated. Used in conjunction with lidar* *and radar data, the vertical evolution of the cloud properties are characterised as a function of time since convection. The approach in this work builds a composite picture of the lifecycle tropical convective into thin cirrus. This paper also investigates how TSC is a function of latitude, and shows that the changes in the cloud properties along trajectories are a strong function of TSC and are not simply reflective of latitudinal changes as air moves from the tropics to the extratropics. This paper also considers the radiative evolution for just the high clouds along the trajectories.*

: 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.

Reply: We thank the reviewer for these comments. As above, we have now more clearly described the purpose of this study to explain that this work doesn't just look at directly detrained cirrus clouds, and have added new analysis about important uncertainties in the method, along with other changes. We hope that the reviewer is satisfied in our response here and in our improvements to the paper.

Reviewer 2

: 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.

Reply: We thank the reviewer for their comments, which we have addressed in turn below.

: 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.

Reply: We appreciate that the analysis might not have been clearly explained and have modified the wording in section 2.6 on lines 193-218 to improve this. In particular, we have included more details on the datasets used in the methods section and more detail on how we have combined multiple datasets that have different spatial and temporal resolution. In general, we take the nearest TSC and CERES data that matches the time of the ISCCP data. i.e. the TSC and CERES data are sampled every 3 hours to match the ISCCP time. The DARDAR data has a very high temporal and spatial resolution. In order to match this to the 3-hourly, 1 by 1 degree resolution, the DARDAR times are rounded (to the nearest hour) that match the ISCCP time. We have explained this in more detail below.

Specific comment:

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*

Reply: We thank the reviewer for pointing us towards this reference, we have included it in the manuscript on line 55.

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?*

Reply: We have included more information, including an updated reference to the ISCCP-H dataset, at lines 104-107: *This work uses the global weather states in Tselioudis et al. (2021), which uses the ISCCP-H 1 by 1° daytime and nighttime 3 hourly dataset. (Rossow et al., 2017) The ISCCP-H differs from prior ISCCP products in numerous ways, most importantly improving the spatial resolution to 1° from 2.5°. A full list of the differences in the data products is given in Rossow et al., (2017). The weather states are identified by clustering the ISCCP tau-CTP joint histogram, resulting in the cluster centroids listed in Table.1.*

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

Reply: We have removed 'algorithm' from the description of the nearest neighbour method as it isn't strictly an algorithm we have written. The "nearest neighbour" just means that each 1°by 1°ISCCP gridbox is assigned to the cluster with the properties that most closely represent it. In this work, the centroid value of the cloud regime in 3 dimensions (CTP,albedo,CF) is used to assign the gridbox to the centroid that most closely resembles it in those dimensions. The text has been reworded at line 107 to make this clearer: *The ISCCP-H dataset is separated into seven distinct cloud regimes by calculating the nearest neighbours of each gridbox and clustering them into the separate regimes depending on the gridbox mean cloud fraction, cloud albedo, and cloud top pressure.*

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?*
Reply: We only want to define the core itself as deep convection, and not any subsequent outflow that has a lower optical thickness. The ISCCP threshold of 23 applies at the retrieval scale (where as the reviewer states, a deep convective cloud might have a very large optical depth). In this work, we are identifying deep convection based on 1°by 1°data, such that the average optical depth will be smaller - the deep convection cluster centroid has an optical depth of only 10.5 (Tab. 1).

Assigning gridboxes to clusters doesn't have an inbuilt threshold for optical depth or CTP. We found that without setting this condition we risked missing the evolution of the some of the anvil as it was categorised as being 'deep convection'. This is now made clear in lines 118-121: *Additional conditions are applied to isolate the convective cores, requiring a* $\tau_c > 8.5$ *(albedo* > 0.5*) and a cloud top temperature (CTT)* < 220*K. Only the very brightest, thickest cores of the convective clouds are categorised as DCC. If these conditions aren't imposed then thick anvil cirrus are included as part of the convective cores, and the ability to investigate their temporal development is reduced.*

Line 105: *Where does this expression come from? What is 0.895?*
Reply: This expression comes from the ISCCP simulator from Jakob and Klein and accounts for scale effects in the non-linear relationship between albedo and cloud optical depth. A reference has been included on line 111: *...as defined in the ISCCP simulator (Klein and Jakob, 1999)*

Line 105: *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.*
Reply: The DARDAR data is not pre-averaged across the 1 by 1° gridbox. This means that there are many CloudSat overpasses corresponding to the same TSC value, and all these CloudSat pixels contribute to the average CloudSat profile for each TSC bin.

The wording of the text has been modified to make this clearer on lines 202-206: *The vertical evolution of the cirrus is investigated using the DARDAR dataset, a combination of the CloudSat radar and CALIPSO lidar. The overpass locations of DARDAR are matched within the hour to the TSC at that location, at 1 by 1 degree resolution. A 1°by 1°gridbox can contain many DARDAR* *retrievals, each of these DARDAR retrievals is assigned the same TSC value - that of the 1°by 1°gridbox. These DARDAR retrievals then all contribute equally to the analysis in the relevant TSC bin.*

***Line 193**: What is the resolution of the CERES data? The native footprint 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?*
Reply: The CERES SYN-1deg dataset is at a 1 by 1° resolution. CERES data provides global coverage at 1 hourly temporal resolution by combining MODIS satellite data with other geostationary satellites. The data products are merged by sampling the CERES data every 3 hours to coincide with the ISCCP data time. The wording of the Data section (lines 94-101) is improved to make this clearer: *Observational data from the 3-hourly International Satellite Cloud Climatology Project (ISCCP) H dataset at 1x1° is used to define locations of deep convection (Rossow et al., 2017). ECMWF ERA5 reanalysis wind fields are used in the trajectory analysis (Hersbach et al., 2018) and to characterise cloud properties. To examine the evolution of the radiative properties, the CERES SYN1deg L3 LW and SW TOA fluxes are used (NASA/LARC/SD/ASDC, 2017). The CERES SYN1deg product combines MODIS and geostationary satellite data to provide global coverage at a 1x1° resolution and 1 hourly temporal resolution. The vertical evolution of the cloud is investigated by utilising the DARDAR dataset, which is an ice cloud retrieval product that combines measurements from the CloudSat radar and CALIPSO lidar (Delanoë and Hogan, 2008; Sourdeval et al., 2018). The period of study in this paper is 2008-2010 inclusive..*

***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.*
Reply: We are not entirely sure what the reviewer means by this. The reviewer makes a valid point that it becomes more difficult to associate the clouds with convection at longer times since convection, and we have been conscious of not attributing changes in cloud properties at longer TSC to the properties of the initial convective event itself. Nonetheless, TSC provides an important metric to investigate how cloud properties change as a function of time.

***Line 227**: The authors claim that the TSC is relevant to 360 hours. I don't 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.*
Reply: The reviewer is correct that large scale subsidence may be responsible for the change rather than convective decay, particularly at long timescales. We were trying to to show how TSC can be used as a metric to characterise changes in cloud properties along a trajectory, not that the properties are changing due

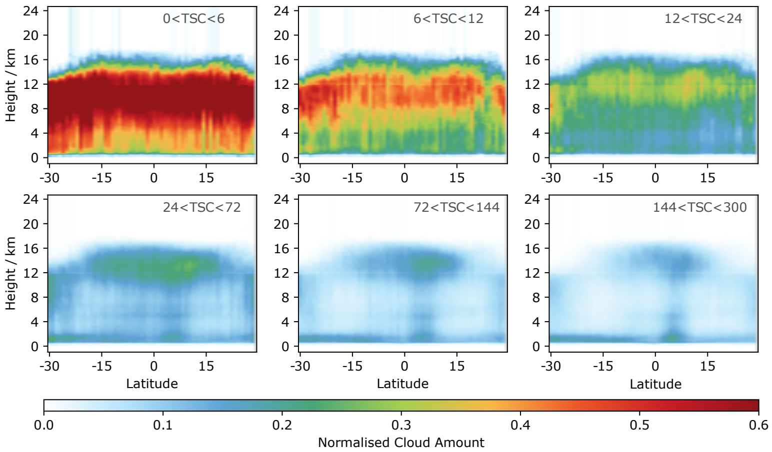

Figure R5: Zonally averaged DARDAR cloud fraction for 12 hour TSC bins.

to the initial convection itself. The word "relevant" has been removed and L243 has been reworded to emphasise that TSC is merely a useful metric for changes in cloud properties. In addition, section 3.6 has been added to the analysis that includes the zonally averaged DARDAR cloud amount, isolating the dependence of cloud amount on TSC from latitude. Future work will assess the impact of the initial convection on the evolution of high clouds as a function of TSC.

We note that latitudinal variations are not purely responsible for the observed changes in cloud properties. Figure R5 shows the zonally averaged vertical cloud amount for increasing TSC bins. It is clear that at very low TSC values, the vertical cloud fraction is almost only a function of TSC, with very little variation in cloudiness profiles as a function of latitude (Fig. R1a). The cloud amount is consistent across all latitudes for a given TSC at early TSC values, showing that TSC is the controlling factor in the vertical evolution of clouds along TSC trajectories, and not just latitude. Latitude becomes important at longer TSC values, where the TSC becomes less central a controlling factor for the vertical structure of high clouds. This plot has been included in the original 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.
Reply: Producing a new validation of the DARDAR product is out of scope for this work, but we note that for clouds colder than about -40C, the DARADR Ni retrieval compares very favourably to in-situ aircraft measurements (Sourdeval et al., 2018) While the accuracy is reduced in warmer clouds, these are not the primary focus of the study.

However, as the reviewer notes, there are still considerable uncertainties surrounding the retrieval, particularly with particle shape. Although this is approximately a function of temperature, variations in shape over time could produce an apparent change in Ni. While this is planned to be investigated in future work, we have added a note to explain these caveats in the discussion at lines 380-384: *While there are significant changes in DARDAR ice properties as a function of TSC (Fig. 8), there are still considerable uncertainties surrounding the retrieval, particularly at warmer temperatures. The DARDAR-Nice Ni retrieval compares favourably to in-situ measurements at the cirrus temperatures that are the focus of this work (Sourdeval et al., 2018). However, the potential temporal variation of factors such as particle shape might introduce TSC-dependent biases in the Ni retrieval, producing an apparent change in Ni. This is will be investigated in future work through a comparison with aircraft data..*

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