This paper addresses two questions about tropical cirrus: (1) how does the cloud radiative effect (CRE) of cirrus from land convection differ from oceanic convection, and (2) how much does changing the lifetime of detrained cirrus impact the overall tropical CRE. Both questions are addressed with a unique cloud tracking algorithm that sorts ISCCP cirrus by their origin (land vs ocean and detrained vs in-situ) and lifetime. Question 2 is additionally addressed by artificially modifying the lifetimes of detrained cirrus, which increases their statistical weight (relative to in-situ cirrus) when computing the average CRE. Answering both of these questions would provide important constraints on how important land-ocean contrasts in convection and lifetime changes in cirrus are for the TOA budget. I think the paper's methods are sufficient to answer Question 1, but less sufficient to answer Question 2, because it is unclear how representative their idealized calculation of lifetime extension is of a meteorologically- or anthropogenically-driven change in cirrus lifetime.

For this reason, I recommend major revisions. I provide more major comments below which, if addressed, I would then be happy to recommend this paper for publication. These comments are followed by more minor points below.

We thank the reviewer for their helpful comments on our paper. We appreciate the concern regarding how we frame the second question regarding detrained cirrus lifetime. We have made changes to the paper to help address their concerns, in particular we highlight that our lifetime extension is modelled by a change in the distribution, and this may not represent all the different processes that adjust the lifetime in practice. We do still think that our proposed lifetime extension method is helpful, as it can help provide a bound on any future estimates of the change in CRE for a change in lifetime. We provide more detail of our changes below.

Major comments

I believe the paper's methods are sufficient to answer Question 1, which in and of itself should merit the paper's eventual publication because it resolves the land/ocean contrast question that another recently published anvil/cirrus tracking study could not (Jones et al, 2024), and because it touches on questions highlighted in the literature, for instance, how much the timing of convection impacts CRE (Gasparini et al, 2022).

However, I think the paper's methods are less convincing in answering Question 2, because it is unclear whether increasing the statistical weight of detrained cirrus and calculating the resulting averaged CRE is equivalent to the CRE that would result from a change in cirrus lifetime due to meteorological or anthropogenic factors. For instance, the lifetime could change due to stronger clustering of convective cores within each anvil (Jones et al, 2024); increased updrafts via aerosol invigoration (Abbot and Cronin 2021), or diminished sedimentation in detrained cirrus (Beydoun et al, 2021), and I could imagine that each pathway would impact CRE differently from each other and from the idealized calculation presented in this paper.

What I think the authors have better constrained is the impact on tropical cirrus CRE that would result from a redistribution of cirrus from in-situ to detrained. The authors could perhaps rephrase their Question 2 to something like "How much does changing the relative abundance of in-situ vs detrained cirrus impact the tropically averaged CRE?". Or, if they stick to their original phrasing, then they should provide additional analysis, or additional discussion at the very least, of how their method of extending cirrus lifetime and computing CRE is representative of how meteorologically- or anthropogenically-driven changes in lifetime would impact CRE.

Response to major comments

We appreciate the reviewers comments on our paper, and agree with the major points raised regarding the lifetime adjustment we propose being an adjustment to distribution. The point of this paper was to focus on the big picture of how sensitive the tropical CRE was to large, generalised changes to the lifetime of detrained cirrus. Whilst it is not the purpose of the paper to assess the potential mechanisms that may change lifetime, and how great these lifetime changes may be, we agree that some more discussion of these mechanisms should be included to put the results we have obtained into context. We have included a paragraph in the discussion that, as well as the minor points raised, help to address these issues raised. Beginning on Line 388:

The method used to extend the lifetime of the detrained cirrus is relatively idealised, insofar as it models a lifetime extension as a change in the distribution of detrained cirrus at the expense of in situ cirrus. Moreover, the extension in the distribution modifies the distribution mostly at the tail end of the detrained cirrus lifetimes, meaning that the oldest detrained cirrus are the ones whose distribution gets artificially increased. The purpose of this work was not to assess the methods through which a lifetime extension would occur. Instead, we aim to provide an upper bound on the impact that increasing the lifetime of the detrained cirrus would have on the tropical high cloud CRE. By modifying the distribution to represent an increase in lifetime, particularly in a way that may impact the longer lived detrained cirrus more than the short lived cirrus, we do provide such an upper bound, since any modification to the shorter lived cirrus would not increase the CRE by as much, as they are already more cooling. In reality, any physical routes through which a lifetime extension will likely increase the total CRE by less than the values we provide here. Further work is needed to assess the mechanisms through which lifetime extensions might occur, and what the range of impacts this may have on the CRE. For example, the lifetime could change due to a stronger clustering of convective cores Jones et al. (2024) increased updrafts via aerosol invigoration Abbott and Cronin (2021). Each of these mechanisms may impact the lifetime in a distinct way from the idealised set up in this work. Investigating these mechanisms and the specific impacts they had on the lifetime would make for an interesting comparison study to the idealised extension proposed in this work, and would be a necessary addition to put these results into context, as well as developing a stronger constraint on the potential changes of the CRE.

Minor comments

- The captions of Figure 4 and Figure 9 should be switched with one another

  This has been fixed, we thank the reviewer for spotting this.

- I thought the final paragraph of the introduction nicely sets up the rest of the paper. However, the rest of the introduction could be written more succinctly to help propel the reader to the questions that this paper will address. For instance, Lines 31 - 36 could be rewritten as “Cirrus clouds cover approximately 60-80% of the tropics (refs), with about half being formed in-situ and the other half from detrainment (refs).”

Lines 75 - 82 could be shortened in a similar way. I encourage the authors to prune the introduction and keep its scope as focused on the two research questions as possible.

We have made some attempts to prune the introduction, however another reviewer commented on the high quality of the literature review, so we were hesitant to remove too much.

Line 124: What is the physical reasoning behind choosing a 10% threshold?

The 10\% threshold is a a somewhat arbitrary choice, but is related to the threshold used in Luo and Rossow, who use 20\% of maximum cloud fraction (in our case that would be 20\%, since the max cloud fraction is approximately 1 at point of convection). We have included a discussion of the 10% threshold, and the sensitivity of our results to this threshold, in the discussion, beginning on Line 378:

The second area of uncertainty in this work surrounds the definition of detrained cirrus. This work defines the end of a detrained cirrus lifetime, and the beginning of the in situ air parcels, as the point at which the cirrus cloud fraction along a trajectory from deep convection reduces below 10% for the first time. Any cirrus that then appears after this time is classified as in situ in origin. This is similar to Luo and Rossow (2004) who define the end of their cirrus lifetime as the point at which the cirrus cloud reaches 20% of the maximum cloud fraction along the trajectory. Changing the definition of detrained cirrus would not change the overall high cloud CRE. However, it does change our calculated lifetime of detrained cirrus, which is shown in Figure S1 in the supplementary. There is no universal definition for "detrained'' or "anvil'' cirrus, and as such the lifetimes of these clouds vary depending on how they are defined. Nevertheless, our lifetimes fall within the expected ranges given in the literature Luo and Rossow (2004) and as shown in Figure S2, the final values for the change in CRE for a given lifetime extension are not particularly sensitive to the threshold used to define the convection.

Line 327: You have found that a 50% or 15 hour increase in detrained cloud lifetime results in an increase in the overall high cloud CRE by about 0.6 W/m^2. It would be interesting to know how much cloud lifetime is expected to increase due to, say, the aerosol invigoration hypothesis. If the expected increase in lifetime is much smaller than 50%, then you could say that aerosol invigoration *might* not matter all that much in terms of its impact on CRE. I think that making these quick assessments with all of the proposed mechanisms that change cirrus lifetime, by connecting to the wider literature, would help make readers care more about your results. And it would illustrate how your result "provides an important constraint on the impact of changes in the lifetime of detrained cirrus in a future climate or in response to aerosol perturbations on the total tropical CRE."

We thank the reviewer for this helpful suggestion – we have included this in the discussion on line 414:

Comparing this to aerosol invigoration studies which have suggested lifetime extensions of detrained cirrus on the order of 30% (Zang et al. (2023)), it may be the *case that aerosol invigoration may not have a large impact on the total tropical CRE, however more work is needed to constrain the lifetime extension from aerosol invigoration, which is currently highly uncertain.*

- This manuscript, either in the introduction or in the conclusion, could mention how it distinguishes itself from other recent papers using cloud tracking of anvil/cirrus systems (e.g. Jones et al, 2024). For instance, the observations used in this manuscript have a longer time record and cover the whole tropics, which allows regional variations such as land/sea contrasts to be addressed.

  This is a good point from the reviewer, we have added a sentence in the conclusion on line 433 comparing this work to other detrained cirrus tracking methods:

  The tracking approach used in this work differs from previous studies, such as Jones et al (2024) by using ISCCP data with a much longer time record, as well as covering the entirety of the tropics, without explicitly tracking individual clouds at all. This allows for regional variations such as the land ocean contrast to be thoroughly investigated.

References

- Abbot and Cronin, 2021 *Aerosol invigoration of atmospheric convection through increases in humidity*
- Beydoun et al, 2021 *Dissecting Anvil Cloud Response to Sea Surface Warming*
- Gasparini et al, 2022 *Diurnal differences in tropical maritime anvil cloud evolution*
- Jones et al, 2024 *A Lagrangian perspective on the lifecycle and cloud radiative effect of deep convective clouds over Africa*

This study uses a time since convection algorithm operating on ERA5 reanalysis and ISCCP data from the entire satellite record to create fields of flags for the tropics that indicate, if there is cirrus, if it has originated from deep convection over land or over ocean, and if it was formed in-situ or detrained from deep convection. They compute cloud radiative effects (CREs) as a function of time since detrainment, using CERES, separately for land vs ocean origin cirrus, and in-situ vs detrained cirrus. They also artificially alter the ratio of in-situ to detrained cirrus and re-calculate the CREs to obtain an estimate for how much warming one could get by increasing the lifetime of detrained cirrus by various factors. This study is a standout in terms of its writing quality and clarity, and was a pleasure to read. The manuscript will be an asset to the cirrus community both because of its nice science results and its thorough and well-written literature review. I do not have major concerns but I do have a list of minor and medium recommendations below that I hope can help the authors improve the clarity and relevance of the manuscript even more.

We thank the reviewer for their very helpful comments on our manuscript. The minor and medium recommendations given are addressed below.

• Be more precise when discussing CREs in lines 4-5, 24 and 28. I recommend giving estimated ranges for SW and LW CREs in one or all of these places. I also recommend changing the word "large" in line 5 to substantial or significant given that the difference is still small compared to the other "large" numbers that you are talking about.

- 'Large' replaced with 'significant' on line 4.

• Lines 8-12: Mention somewhere here that positive numbers indicate warming.

- 'Warming' added when referring to CRE's in this section.

• Lines 18-19: Again, it is good to be more precise and put an estimated range of optical depth or ice water path after the word "thin" as many people in the cirrus community use that word to describe subvisual cirrus and would consider these outflow cirrus to be quite thick.

- We agree – we have included a sentence giving optical depth ranges of 'thin cirrus' on line 25.

• Lines 38-40: I'm not convinced that longer lifetimes of anvils definitely suggest some mechanism for sustaining water vapor. My instinct is that because detrained cirrus are typically thicker than in-situ cirrus, it might just take longer for them to be eroded by the same processes that act as sinks for both in-situ and detrained cirrus.

- This is a good point, we have removed the reference to longer lifetime being due to sustained water vapour, and mentioned that they are thicker than in situ on lines 40-41.

• Line 44: I believe that Sweeney et al. 2023 is about the QBO and not ENSO

- This reference has been removed

• Line 71: Suggest changing "is" to "has been"

- This has been changed.

• Line 89: Suggest changing “clearly” to “overall”

- This has been changed.

• Lines 90-93: Do any of the cited studies on aerosol invigoration explicitly make connections to cirrus? If there is a study that makes this connection, I suggest citing it after “...leading to higher altitude longer lived anvil cirrus”. If there isn’t such a study, I think you should convey that this is speculative

- This sentence has been changed to make it clear that although higher altitude cirrus is cited in the studies, any impact on lifetime is speculative.

• Line 98: Some typos here: double “is” and add “we” between “and” and “investigate”

- These typos are resolved

• Line 100: move “is also investigated” to the end of this sentence

- - These typos are resolved

• Line 123: The 10% threshold is rather arbitrary and the authors acknowledge and justify this in the discussion, but I think some acknowledgement of that should also go here. I also think the authors can be more precise in describing the sensitivity or insensitivity of their results to this threshold in the discussion.

- A sensitivity analysis into the importance of the threshold of detrained lifetime used is included in the supplementary materials. It is shown that changing the threshold of detrained cirrus lifetime from 10% to 20% does not impact the results substantially. This is because the radiative contribution of the cirrus by the end of their lifetimes is small, and the shift from 20% to 10% happens rather quickly. We have also included in the discussion a paragraph that discusses the impact of the threshold used in this study, beginning on line 378:

  The second area of uncertainty in this work surrounds the definition of detrained cirrus. This work defines the end of a detrained cirrus lifetime, and the beginning of the in situ air parcels, as the point at which a cloud along a trajectory from deep convection moves below 10\% cirrus cloud fraction for the first time. Any cirrus that then appears along a trajectory after this point is defined as in situ in origin. This is similar to Luo and Rossow (2004) who define the end of their cirrus lifetime as the point at which the cirrus cloud reaches 1/5 of the maximum cloud fraction along the trajectory. Changing the definition of detrained cirrus would not change the overall high cloud CRE. However, it does change our calculated lifetime of detrained cirrus, which is shown in Figure S1 in the supplementary. There is no universal definition for ``detrained'' or ``anvil'' cirrus, and as such the lifetimes of these clouds vary depending on how they are defined. Nevertheless, our lifetimes fall within the expected ranges given in the literature Luo and Rossow (2004) and as shown in Figure S2, the final values for the change in CRE for a given lifetime extension are not particularly sensitive to the threshold used to define the convection.

• Line 198: Change fig. 3 to fig. 4

- This has been changed.

• Fig. 4 and Fig. 5: I suggest adding vertical lines to these figures to indicate when the cirrus go from being predominantly detrained to being predominantly in-situ. I suggest this because the authors discuss the CREs of detrained cirrus being different over the cloud lifetime between land-origin and ocean-origin cirrus due to the amount of incoming solar being different at the time of their detrainment, and at first I thought that these figures contradicted that, but then I realized that the detrained cirrus are only existing at the far left side of the plot.

- This is a useful suggestion, and has been implemented.

• Lines 317-324: The authors state that their purpose isn't to obtain a concrete value of CRE for a given lifetime extension, but in fact, they do obtain a concrete value and reference it throughout the manuscript. So, more effort needs to be made here to connect this experiment to the real world. Do the studies suggesting that anvils will be longer lived in the future also suggest that the anvil properties will remain the same throughout the longer cloud lifetimes? One can imagine anvils being longer lived but resembling in-situ cirrus towards the ends of their extended lifetimes. Also, the in-situ cirrus are artificially removed to make space for the longer lived anvils, so the estimate here is sort of a lower bound. How do the CREs of the removed in-situ cirrus compare to the CREs of the added anvils? In general, it would be good to cite the literature here and argue more precisely for the plausibility of this experiment.

We thank the reviewer for this comment. Similar points were raised from other reviewers, particularly regarding how modelling a lifetime change as a shift in the distribution may compare to physical mechanisms for lifetime extensions. We have included an extra section in the discussion to try and address these points. The discussion begins on Line 388:

The method used to extend the lifetime of the detrained cirrus is relatively idealised, insofar as it models a lifetime extension as a change in the distribution of detrained cirrus at the expense of in situ cirrus. Moreover, the extension in the distribution modifies the distribution mostly at the tail end of the detrained cirrus lifetimes, meaning that the oldest detrained cirrus are the ones whose distribution gets artificially increased. The purpose of this work was not to assess the methods through which a lifetime extension would occur. Instead, we aim to provide an upper bound on the impact that increasing the lifetime of the detrained cirrus would have on the tropical high cloud CRE. By modifying the distribution to represent an increase in lifetime, particularly in a way that may impact the longer lived detrained cirrus more than the short lived cirrus, we do provide such an upper bound, since any modification to the shorter lived cirrus would not increase the CRE by as much, as they are already more cooling. In reality, any physical routes through which a lifetime extension will likely increase the total CRE by less than the values we provide here. Further work is needed to assess the mechanisms through which lifetime extensions might occur, and what the range of impacts this may have on the CRE. For example, the lifetime could change due to a stronger clustering of convective cores Jones et al. (2024) increased updrafts via aerosol invigoration Abbott and Cronin (2021). Each of these mechanisms may impact the lifetime in a distinct way from the idealised set up in this work. Investigating these mechanisms and the specific impacts they had on the lifetime would make for an interesting comparison study to the idealised extension proposed in this work, and would be a necessary addition to put these results into context, as well as developing a stronger constraint on the potential changes of the CRE.

- Line 338: too many m’s in “fromm”
  - This typo has been resolved
- Line 354: Missing parenthesis
  - This typo has been resolved

General comments:

In “How does the lifetime of detrained cirrus impact the high cloud radiative effect in the tropics?”, Horner and Gryspeerdt present compelling new observational evidence on the importance of detrained cirrus lifetime and the diurnal to the tropical high cloud radiative effect. Understanding the lifecycles of both detrained anvil cirrus and in situ cirrus and how these may respond to climate change is a major focus of present research, and so I consider this work highly relevant for publication. The Lagrangian trajectory method developed for use here allows investigation of the lifetimes of detrained and in situ cirrus to an extent not possible through other observational techniques such as cloud tracking. In addition, the long time span of the dataset (exceeding 30 years) provides a great wealth of observational data to analyse.

I found the manuscript in general to be very well written and presented and interesting to read. However, I found that the “lifetime extension” experiment presented in section 3.5 was much harder to parse. From my understanding, this approach investigates how the net high cloud CRE would change if the proportion of long lived detrained cirrus was to increase. This is not quite the same as uniformly increasing the lifetime for all detrained cirrus, as the present day long-lived cirrus is more likely to result from different conditions such as more intense or organised convection, and may have colder temperatures and occur at different times of day, introducing other factors that affect the net CRE other than the lifetime. This is still a very valid to question to ask, as warming has been hypothesised to lead to an increase in both more intense and more organised convection. I think it would help to rephrase the lifetime extension experiment in these terms.

The main areas I found that were lacking in the manuscript was a discussion of the mechanisms affecting cirrus lifetime, including radiative effects and cloud height. The authors make a good argument for the importance of the diurnal cycle on the net high cloud CRE and the difference between land and ocean origin convection due to differences in the CRE near the start of the trajectories. Figure 7 appears to show an impact of the diurnal cycle on the lifetime of the detrained cirrus as well, which would provide observational evidence for processes previously investigated using models. Further, detailed discussion of the differences seen in the lifetimes of detrained cirrus, including the differences between land and ocean as well as the change in the net CRE along observed trajectories vs the detrained cirrus lifetime would greatly add the to manuscript. I also feel that contributions that the authors make contributions to understanding the behaviour of detrained cirrus are more important and novel than they present them in the paper!

Overall, I recommend that the manuscript undergo major revisions. My suggestion to the authors, should they wish, is that the “lifetime experiment” section of this manuscript be split and expanded into a separate paper with a clearer hypothesis and objective, which could, for example, be published in a letters format. That would also leave more space in this paper for further discussion of the observed differences in detrained cirrus lifetimes and their CRE. However, I fully understand if the authors do not wish to split this manuscript.

I have included full comments below. Many of these suggestions may be more suitable for further work than inclusion in this manuscript, but reading through raised many interesting questions that I think would make valuable future work.

Specific comments and suggestions:

Line 4: It wasn't clear to me that hypothetical changes in anvil lifetime has been discussed in the introduction. While there are many factors that influence the decay rate and hence lifetime of in situ cirrus, I am not aware of a clear hypothesis that explains the mechanisms through which detrained cirrus lifetime would change with global warming

- We have included a further paragraph in the introduction to outline some potential mechanisms through which the lifetime of anvil cirrus could change in the future (Line 109. The reviewer is right that there is no clear hypothesis that explains exactly how detrained cirrus would be expected to change. The purpose of this work is to bound the impact that any potential change could have on the cloud radiative effect, to evaluate the importance of lifetime of detrained cirrus on the tropical CRE, and not to evaluate potential mechanisms through which this lifetime change could occur. We appreciate that this goal was perhaps not clear enough, so have included in the abstract that this is the main aim of this work, on line 14:

  "Whilst there is uncertainty in the strength of mechanisms responsible for a change in cirrus lifetime, this work provides an important constraint on the impact that any potential lifetime extension may have.

Line 20: Possibly not the best reference for this? I'm not sure that this needs referencing

- This reference has been removed

Line 21: radiative effect, rather than forcing

- Changed forcing to effect on line 21

Line 49: While both detrained cirrus and in situ cirrus are affected by changes in deep convection, I am not sure there is a clear direct link between the two types of cirrus

- The point made here is that any significant change in detrained cirrus amount will also impact the in situ cirrus, and since deep convection is the main route through which water vapour is transported to the upper troposphere, this will also likely have an impact on in situ cirrus. We have added a sentence on line 43 to clarify what is meant by this point:

  "… given that the formation of in situ cirrus relies on water vapour in the upper troposphere, transported via convection."

Line 54: It may be helpful to include that albedo reduces faster than LW emissivity, resulting in the switch from net cooling to net warming as cloud thickness decreases (e.g. Berry and Mace, 2014)

- This sentence is added at Line 58: *"The albedo decreases faster than the LW emissivity, contributing to this switch from cooling to warming in the net CRE"* including the reference.

Line 67: Possibly think about combining these two paragraphs + highlight the importance of lifetime along with diurnal cycle. e.g. if the timing of convection remains the same, changes in lifetime may result in more cirrus existing at night or day, which could change the net cirrus CRE without any changes to the optical properties of the cirrus.

- These paragraphs have been combined, along with other brevity changes to the introduction, as requested by another reviewer.

Line 75: Changes in anvil cloud height and area are general seen as separate (albeit opposing) feedbacks, as per e.g. Sherwood et al 2020

- This line has been removed due to brevity concerns by another reviewer

Line 78: The original proposed precipitation efficiency iris feedback (Lindzen et al., 2001) is generally no longer considered valid. I would focus discussion here on more recent discussion of evidence for and against the stability iris feedback, e.g. hypothesised suppression (e.g. Jeevanjee 2022) and enhancement (e.g. Seeley and Romps, 2015) of convection with warming.

- We have removed this section in our introduction for brevity, as another reviewer has requested we do for our introduction. We don't believe this discussion helps guide the reader towards understanding the questions regarding detrained cirrus lifetimes.

Line 80: This is only true if the advection of anvil cirrus remains the same. However, the stability iris hypothesis is based on a reduction in the large scale overturning circulation, which would mean that for the same (Lagrangian) lifetime of cirrus, the anvil would cover a smaller area

- The reviewer raises a good point, however the sentence is meant to highlight how one would interpret the Time Since Convection in relation to the physics highlighted. To make this point more clear, we have added the caveat on line 80:

  "assuming the advection of anvil cirrus remains the same, which under the stability iris hypothesis may not be true".

Line 82: It may be good to include discussion of the findings of Raghuraman et al. 2024 on that changes in tropical anvil cirrus properties

- This is a good point, the paper hadn't been released when this manuscript was prepared. This reference has been added to the discussion on line 83:

  Raghuraman et al. 2024 used observational evidence to investigate these feedbacks, finding a near zero high-cloud amount feedback, providing evidence against an iris feedback.

Line 86: Average anvil lifetimes are very sensitive to the choice of method, thresholds for cirrus detection and the type of convection studied. Older studies tended to detect less isolated convection and more MCSs so tended to be biased towards larger, longer lived anvils. It may be good to discuss some of the factors driving this, such as the difference in spatial and temporal resolution between the two studies, to put your own results into context

- This is a good point, we have briefly mentioned this in the introduction on line 89:

  These studies are sensitive to the choice of methods and thresholds for cirrus detection used. For example. Luo and Rossow determine cirrus lifetime to be the point at which the cloud fraction along a trajectory first drops below 20\% of the maximum cloud fraction along the trajectory (nominally 100\%). There is no robust definition of when detrained cirrus have dissipated, therefore there is some spread in the given lifetimes of detrained cirrus in the literature.

  We return to this question in the discussion, and the impacts that lifetime definition has on our own results.

Line 87: Beydoun et al. 2021 similarly found negligible change in anvil lifetime in RCEMIP-style models, which may make a useful second point of reference. However, these models do not necessarily represent processes affecting cirrus lifetime accurately (e.g. lack of diurnal cycle, ocean only) so more research is needed.

- We thank the reviewer for this useful reference, and they make a good point regarding lack of diurnal cycle/oceans etc. We have included this reference on line 95:

  Beydoun et al. 2021 also found a similar negligible change in anvil lifetime using RCEMIP-style models. It should be noted that neither of these studies represent all the processes that would impact cirrus lifetime, particularly the lack of diurnal cycle. More observational studies are needed to assess the impact that these particular processes may have on the lifetime of detrained cirrus clouds.

Line 90: Herbert and Stier 2023 is a good reference for linking aerosol effects on convection to anvil CRE. However, I would argue that we still don't have a good understanding of how changes in convective intensity affect anvil properties.

- This is a very useful reference, and the reviewer is right that more understanding is needed to fully link the impact of aerosol invigoration on longer time evolution of anvil properties. We have included the reference on line 103 below:

  Herbert and Stier (2023) provide an observational study investigating the impact of AOD on deep convection over the Amazon, however this is limited to short timescales, close to the convection. There is still more work needed to understand the full impact that aerosol invigoration may have on the lifecycle of detrained cirrus as they evolve beyond the deep convection itself.

Line 102: One area of discussion missing from the introduction is factors affecting the lifetime and decay of cirrus clouds. Both solar heating (and hence diurnal cycle dependence), such as Gasparini et al. 2022, Sokol and Hartmann 2020, and sublimation/sedimentation of ice clouds e.g. Seeley et al. 2019

- We thank the reviewer for raising this point. We agree with their point, we also think it is particularly important to consider the timing of the convection as a control for the lifetime of detrained cirrus. (Sokol and Harmann 2020, Gaspirini et al. 2022). We have included an extra paragraph in our introduction to highlight these points, and help guide the reader to questions that we answer in our paper, such as the difference in lifetime from land and ocean convection (due to changes in timing). This paragraph begins on line 109:

  There are many factors that impact the decay, and therefore lifetime, of detrained cirrus clouds. Gasparini et al. 2022 used idealised cloud resolving models to investigate the impact that the diurnal cycle has on the cirrus lifetime. They found that tropical anvil clouds forming in the day are more widespread and longer lasting than those formed out night, due to the shortwave radiative heating during the day which lofts and spreads the anvil clouds. During night there is an increase in ice nucleation, however this doesn't compensate for the entrainment of dry air that also acts to reduce anvil lifetime. Sokol and Hartmann 2020 also found that anvils thin more rapidly during the night due the net radiative cooling eroding the cloud top. In both cases, it is found that the timing of convection is an important control on the lifetime of the detrained cirrus cloud.

Line 106: ISCCP-H data is every three hours, but the ERA5 winds and TSC step is hourly. Are new convective events defined only every three hours, or is some sort of interpolation used to increase the temporal resolution of the ISCCP data?

- New convective events are only defined every 3 hours, even though the TSC runs at an hourly resolution – we have included a sentence on line 134 to clarify this:

Where new deep convection occurs (locations updated every three hours), the TSC is reset to zero and advected as new convection.

Line 110: Is there much variance in the wind field in this pressure range? Would this cause much uncertainty in the Lagrangian trajectories?

- Analysis into the uncertainty in the Lagrangian trajectories is given in Horner & Gryspeerdt (2023), which shows that the trajectories follow HYSPLIT trajectories closely for the entirety of the detrained cirrus lifetimes. This point has been clarified on Line 133:

  As shown in Horner & Gryspeerdt (2023), these trajectories remain at this pressure level for the entirety of the detrained cirrus lifetime.

Line 110: May be clearer to move this reference to immediately after "ERA5 reanalysis" to ensure that it refers to the data source, not the method

- This has been changed on Line 110

Line 114: Suggestion: CERES data has not yet been discussed, so it might be clearer to rephrase along the lines of "…any results involving CRE only cover the period after 2000 when CERES was operational". Also, does the CERES acronym need to be defined?

- This has been changed. The CERES acronym has also been defined

Line 117: This is also known as "liquid-origin cirrus" e.g. Luebke et al. 2016, and it may be helpful to make the link here.

- This link has been made and the reference included

Line 119: This last sentence is a little unclear, and could be rephrased to be clearer e.g. "In situ cirrus are those which appear along the trajectory after a period of clear skies"

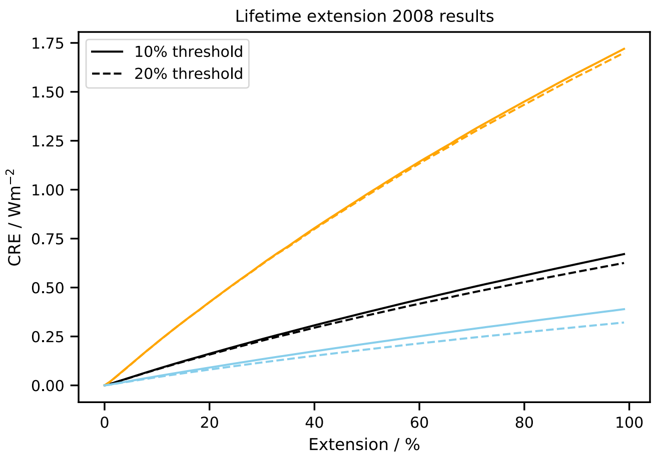
- This suggestion has been implemented.

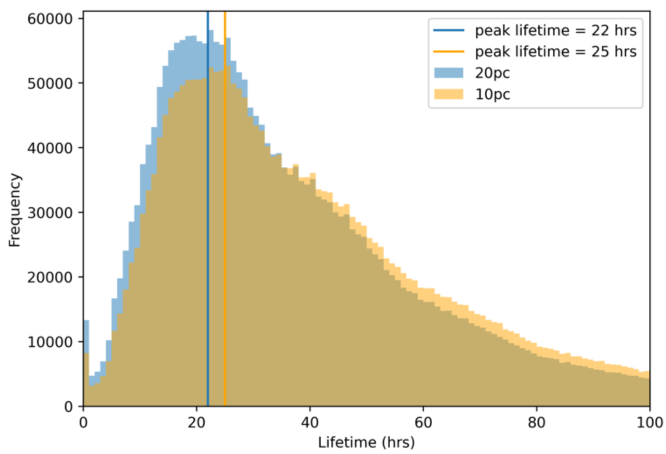
Line 121: How sensitive is the detrained/in situ divide to the cloud fraction threshold? Other studies have used 20%, would using this threshold have notably changed the results?

- The overall CRE evolution, and the associated lifetime extension, is not significantly sensitive to how we define the threshold. Below show the histogram of detrained lifetimes, and then the lifetime extension plots using a 10% and 20% cloud fraction threshold. As we can see, by definition there is a small change in the histogram of lifetimes when we change the threshold. This doesn't translate to a significant change in the CRE extension change, which remains within the bounds of interannual variability. Note these results were only compared for 2008, however it isn't expected to be sensitive to the year chosen. These results are included in the supplementary, and a note on the sensitivity is mentioned in the discussion section on lines 388:

The second area of uncertainty in this work surrounds the definition of detrained cirrus. This work defines the end of a detrained cirrus lifetime, and the beginning of the in situ air parcels, as the point at which a cloud along a trajectory from deep convection moves below 10% cirrus cloud fraction for the first time. Any cirrus that then appears along a trajectory after this point is defined as in situ in origin. This is similar to Luo and Rossow (2004) who define the end of their cirrus lifetime as the point at which the cirrus cloud reaches 1/5 of the maximum cloud fraction along the trajectory. Changing the definition of detrained cirrus would not change the overall high cloud CRE. However, it does change our calculated lifetime of detrained cirrus, which is shown in Figure S1 in the supplementary. There is no universal definition for "detrained'' or "anvil'' cirrus, and as such the lifetimes of these clouds vary depending on how they are defined. Nevertheless, our lifetimes fall within the expected ranges given in the literature (Luo and Rossow, 2004), and as shown in Figure S2, the final values for the change in CRE for a given lifetime extension are not particularly sensitive to the threshold used to define the convection.

Figure 1: Is the annual mean for 2008?





- Yes this has been clarified in the caption.

Line 125: It would be of interest, for future studies, to include a third "cloud free" flag in the algorithm, which might allow more investigation into the formation and persistence of in situ cirrus that forms in airmasses diverged from convection

- We agree this would be very interesting to see in a future study.

Line 154: ISCCP cloud type histograms have previously been shown to over-attribute toa cooling to low clouds (see e.g. Figure 5 of Stephens et al. 2018). This may lead to a positive (warming) bias to your results, which would be good to discuss.

- We thank the reviewer for raising this, we have addressed this in the discussion, line 374:
- *It is also known that the ISCCP cloud type histograms may over attribute the TOA cooling to low clouds (Stephens et al. 2018). This may lead to a positive (warming) bias in our results, therefore it should be understood that our results represent an upper bound. However this bias is likely small in comparison to other uncertainties mentioned above.*

Line 155: How is the monthly mean calculated? As the high cloud fraction varies with the diurnal cycle, in particular over land, simply averaging all points with low high cloud fraction could bias the sampling to certain times of day which would in turn affect the SW mean. Binning observations by local time, calculating the mean for each time bin over the month then averaging all could reduce bias.

- We thank the reviewer for highlighting this point – we have now also calculated the monthly mean in this way. This change had a small impact on the magnitude of the forcings, increasing them all, since binning by local solar time increased the albedo, meaning that the background SW is larger, reducing the high cloud SW impact. The change in these results does not change any of the conclusions drawn, just the numerical values for the CRE and a small change in the CRE from lifetime extensions. A table has been included below to highlight the changes in the results, and all the relevant figures have been update

Old values	New values
All CRE: **3.6+-0.4**	All CRE: **5.0+-0.4**
Ocean CRE: **2.9+-0.4**	Ocean CRE: **4.4 +-0.4**
Land CRE: **6.3 +- 0.6**	Land CRE: **7.7+-0.6**
100% extension all: **1.2+- 0.1**	100% extension all: **0.7+-0.1**
100% extension land: **2.3+-0.1**	100% extension land: **2.0+-0.1**
100% extension ocean: **0.9+-0.1**	100% extension ocean: **0.3+-0.1**

Interestingly this reduces the impact of the extension, whilst increasing the baseline CREs. The reason for this is that this change in the calculation of the background CRE reduces the difference in the CRE between detrained air parcels and in situ air parcels. The increase is largest in over land, which is presumably most affected by the biases incurred in the old method.

Figure 2: Am I correct in thinking that fig. 2 a and b show ToA SW flux, not CRE?

- Yes the all sky ToA flux, and then the background flux which is a combination of the clear sky and low cloud CRE. We have relabelled these plots.

Line 169: In what way do they disappear in the annual mean? Would they not have a positive bias to the measured CRE? What happens if you set all positive high cloud SW CRE values to 0?

- The logic we follow is that there are also regions and times when the high cloud SW is overestimated, as well as these times where the SW is underestimated (positive SW). Removing these underestimations (i.e. by setting positive SW to zero), would bias our results in favour of the overestimated times. More generally, there is no perfect way to isolate the high cloud radiative effect, although we believe this method to be robust, particularly in comparison to using any method similar to the CERES flux by cloud type product, which biases geographically towards regions where there is no low cloud, for example.

Line 170: While I agree that the low cloud LW CRE will be small, so is the net high cloud CRE you find later in the paper, and so this bias could be significant. How large is the difference in the background flux if you include low cloud CRE as with the SW fluxes?

- We originally thought about calculating the LW CRE in the same way. However the ISCCP joint histograms are only available in the daytime, therefore we could only retrieve the daytime background LW CRE, which wouldn't suffice for this analysis and would bias the background LW CRE, introducing a large amount of uncertainty. Therefore an approximation had to be made that the all cloud LW CRE would fairly well represent the high cloud LW CRE.

Line 191: What is the standard deviation of the detrained cirrus lifetime? It would be interesting to compare the variance in lifetime vs that seen by Luo and Rossow, 2004

- This is an interesting point, we thank the reviewer for raising it. We found the standard deviation in the lifetimes to be 30 hours, this is double that of Luo and Rossow, showing that this method captures a much wider range of lifetimes and trajectories. We have included a sentence on this on line 223:

  The standard deviation of the lifetime is found to be 30 hours. This is much larger than that found by Luo and Rossow, showing perhaps this method is more suited to capturing a wider range of lifetimes, by not selecting specific convective events but instead tracking all air from convection.

Figure 3: There appear to be a lot of trajectories that are very short (only one time step?). Do these occur in regions with very frequent convection, so that existing trajectories get replaced by newly initialised ones very rapidly?

- Yes these short trajectories occur when convection is replaced by fresh convection, these often occur within one timestep, where convection between time steps is often the same convective event.

Figure 3: What causes the scallop-like pattern along the lines? Is it linked to the 3-hourly resolution of the ISCCP data?

- Yes this is a 'scar' from defining the convection every 3 hours, whilst the TSC runs at 1-hourly resolution, we have added this explanation on line 214:

  Note, the scallop-like pattern along the lines in Figure 3a) is due to the temporal resolution of ISCCP data meaning that convection is only initiated every 3 hours.

Line 194: The description of the weighted average is a little unclear, it may be better to rephrase along the lines of "…CRE for detrained, in situ and the net of all air parcels"

- This line has been amended for clarity, now on Line 253:

  Figure 5 shows the LW, SW and net high cloud CRE for detrained, in situ and net of all air parcels as a function of TSC. They are weighted in each TSC bin by the relative occurrence of each bin.

Line 194: Are these averages weighted by the area of each pixel?

- The averages are weighted by the area of each pixel. This is clarified on line 271:

  "…weighted according to the distribution of TSC values in the tropics and the latitude that the values occur at…"

Line 199: While the number of in situ parcels early on is low, there does seem to be a clear trend in their CRE. Perhaps many of these cases are caused by mid-tropospheric clouds (e.g. congestus) which don't classify as high cloud by the ISCCP definitions and so have low high-cloud fraction, have lower LW CRE but still have larger SW CRE

- This is a very good suggestion. In general this trend will be due to any convection that isn't identified with our method, therefore you see some evolution in the low TSC regions of convective activity that we don't flag as detrained cirrus. We have included a sentence on this on line 261:

  There is still a small trend in the CRE of the in situ air parcels even when their occurrence is low. This is likely due to shallower, mid-tropospheric convection that is not identified as deep convection, but still contributing to the CRE along trajectories.

Line 211: This should be referred to as a radiative effect, not a forcing.

- This has been amended.

Line 211: It would be helpful to provide separate values for the detrained and in situ cirrus lifetime CRE to provide perspective for the lifetime experiments of section 3.5

- We have included the detrained and in situ lifetime weighted CREs on line 273:

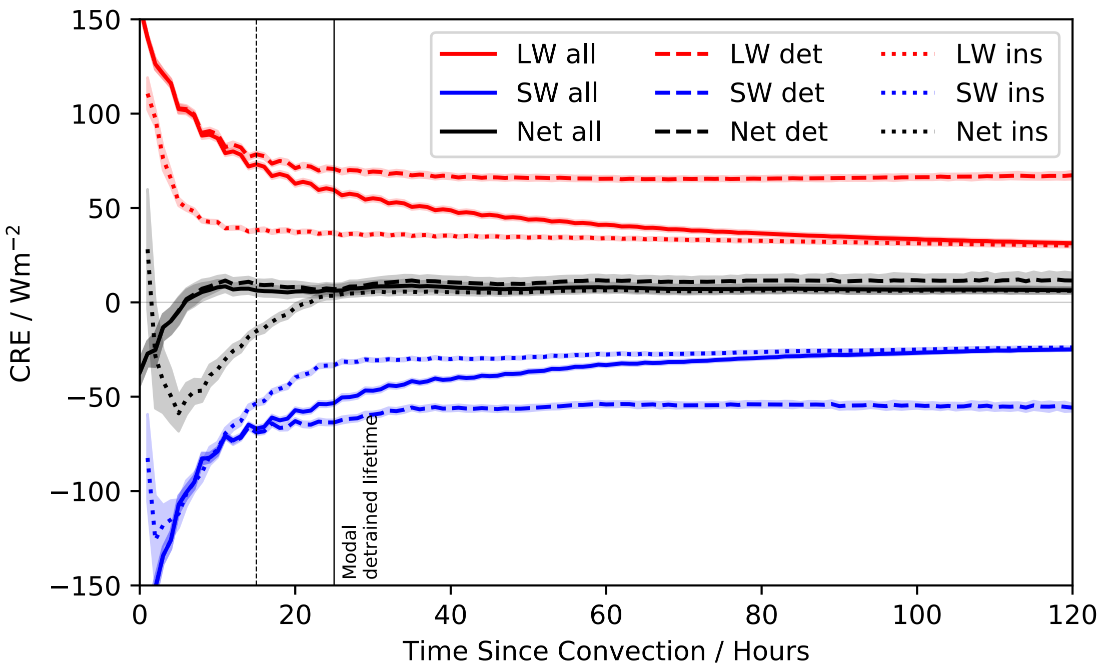
  "The total forcing of the detrained cirrus is found to be 1.4 +- 0.1 Wm^2, whereas the total forcing from the in situ is found to be 9.2+-0.5 Wm^2. Although summed over their whole lifetime detrained cirrus are more cooling than in situ, extending their lifetime still increases the overall warming because they have a greater instantaneous warming at longer TSC values than the in situ."

Figure 4: It might be useful to combine figs. 3 & 4 as panels within one figure, to show that the early net CRE is mainly due to detrained cirrus, and the later CRE more due to in situ cirrus

- Another reviewer suggested adding a vertical line to fig 4 and 5 to show the modal lifetimes on the plot, to indicate something similar. We have done this, which we think also helps to answer this question.

Figure 4/5: It would be interesting to show variance of the CRE in figs. 4 and 5, possibly using the interannual variability as done in fig. 6

- We did think about showing this, however we felt that, given the variances are relatively small, it didn't provide the reader with a huge amount of extra information, whilst making the plots less readable. We will include them in the supplementary however, see below:



Section 3.3: This subsection could possibly be combined into 3.1

- These sections have been combined, with more of a discussion included regarding the distribution of lifetimes in land-origin versus ocean-origin detrained cirrus as requested by the reviewer.

Line 214 typo: figure 1(d) -> 1(c)

- Fixed typo

Line 221: Does it hide details? I would rephrase it as showing different things, fig. 1f shows where land origin air parcels are most commonly seen over oceans (and vice versa) with the SE Atlantic off the coast of Africa being the most common, but fig. 1c highlights that these instances occur everywhere and can travel a long way from the coast

- We appreciate that there is a better way to rephrase this, and we have changed it accordingly, now on line 232:

  The average air parcel origin map (Figure 1f) shows which type of air parcel is most common for a particular region. For example, land air parcels are most likely seen over oceans in the SE Atlantic off the coast of Africa. Figure 1c) shows that these instances of land origin air parcels can occur in many regions over ocean, and can travel long distances from any land source.

Line 226: It would be useful to provide a value for the proportion of oceanic/land origin parcels

- This is a good point and has been added on line 279:

  "It is found that 20% of air parcels are land-origin, and 80% are ocean-origin."

Line 228: Is the difference in surface albedo between land and ocean may also be a cause of these differences. It would be interesting to separate land-origin parcels that are seen over the ocean (and vice versa) to try and isolate some of these effects

- This is a good point. We think this would make an interesting future study – i.e. what are the differences in land-origin parcels when they exist over land v ocean etc. We have added a sentence on line 288:

  Some of the difference is also likely due to the difference in surface albedo between land and ocean. Further work is needed to quantify the contribution of both the albedo difference, and the diurnal cycle difference, by focusing on land origin convection that exists over land and over ocean.

Line 231: I would also mention that while oceanic convection does tend to be more uniform, it does have a peak in the early morning which would add further weight to your contrasting diurnal cycle argument

- Thank you, we have included this point, now on line 286:

  "… with a smaller peak in the morning, which adds to the contrasting SW diurnal cycle between land and ocean"

Figure 6: How is the DCC count normalised? The values here are a little confusing. It may be clearer to plot the y axis as a fractional frequency/proportion

- They are normalised relative to the maximum value for the entire study period. This is clarified in the caption.

Line 240: The land-origin parcels also have larger SW cooling over their lifetime, which could indicate that they are optically thicker than ocean-origin

- This is a good point, and it is made clearer in the manuscript on line 292:

  The land-origin parcels also have a stronger SW cooling after the initial stage of convection, suggesting that the detrained cirrus from land-origin convection are optically thicker than their ocean-origin counterparts.

Section 3.5: It was quite difficult to understand the process throughout this section. It may be clearer to lay it out as a full experiment, with some background as to the causes of changes in cirrus lifetime in a future climate, a hypothesis for how you expect it to affect high cloud CRE, etc. although then it is becoming more of a paper on its own.

- As the reviewer says, this might then become its own paper of sorts. We hope that the improvements throughout the rest of the paper are sufficient to help aid the understanding of this section. We have added more detailed discussion and introduction to help answer some of the uncertainties surrounding the method and help make the paper more cohesive – we do not want to separate this into two papers, and believe the results belong together.

Line 247: The discussion here of the detrained lifetime is very short, and I think some important findings of this paper have been rushed over. The difference between land and ocean lifetime shown in fig. 7 is very interesting, and could do with more discussion either here or in section 3.1

- This discussion has been added to section 3.1, we thank the reviewer for highlighting this oversight. As they say, it is an interesting set of results that needs more discussion. We begin the discussion on line 240:

The difference in land and ocean origin detrained cirrus lifetime is significant, and evidence of an impact of the diurnal cycle of convection playing a role in determining the lifetime of the detrained cirrus clouds. The land origin lifetimes are much shorter than the ocean origin lifetimes likely due to the timing of the initial convection. Recalling that most land convection occurs closer to the evening, this means that the thicker anvil exists for most of its life during solar night. This is compared to the oceanic convection that sees a small morning peak, followed by a much greater proportion occurring during the day than at night. This means that for ocean, most of the thick anvil will spend its evolution in the solar day. Detrained cirrus is likely to dissipate much faster during the night due to, among other factors, the entrainment of dry air acting to reduce anvil lifetime (Gasparini et al., 2022). These lifetime differences also support findings by (Sokol and Hartmann, 2020) who found that anvils thin more rapidly during the night due to the net radiative cooling which erodes the cloud top. As well as the lifetimes being different between land-origin and ocean-origin detrained cirrus, the distribution shown in Figure 4 shows an oscillation in the rate of the decay for both origin cirrus. Further work is needed, however this may provide some extra evidence of a diurnal cycle effect on the lifetime.

Line 248: Are only parcels which are observed to transition from detrained to in situ included in the analysis here? Do these parcels have a different net CRE to those which don't undergo a transition to in situ cirrus before being replaced by new convection?

- All air parcels are included in this analysis, as limiting it to only those that are observed to transition would bias the results more strongly towards a large impact of cirrus lifetime on the overall CRE. Keeping cases where the cirrus is replaced by new convection allows a more straightforward comparison to other CRE estimates, which don't apply this kind of restriction either.

Line 250: This could be rephrased to make it clearer that this lifetime modification technique is the original development of the authors.

- This clarification has been included: *"...a novel method introduced in this work"* on line 312

Figure 7: Mean is possibly not the best statistic to use here, given the skewed distributions. It could be informative both the provide the mean and the peak lifetimes for both

- The peak lifetime has been included in place of the mean lifetime in the figure, however the median lifetime is mentioned in section 3.1 with regards to comparison with Luo and Rossow, 2004.

Figure 7: Both the land and ocean distributions have the same shape, with an interesting oscillation in the rate of decay, but with different phase. Is this further evidence of a diurnal

cycle effect on the lifetime of cirrus, with daytime lofting/nighttime decay? This would provide further evidence for the impact of land vs ocean origin for detrained cirrus impacts. Would be interesting to explore further in future

- We agree, this would make interesting further study, this is discussed further in section 3.1 regarding lifetimes of land vs ocean origin cirrus, beginning on line 244:

  Detrained cirrus is likely to dissipate much faster during the night due to, among other factors, the entrainment of dry air acting to reduce anvil lifetime (Gasparini et al. 2022) These lifetime differences also support findings by Sokol and Harmann, 2020, who found that anvils thin more rapidly during the night due to the net radiative cooling which erodes the cloud top. As well as the lifetimes being different between land-origin and ocean-origin detrained cirrus, the distribution shown in Figure 4 shows an oscillation in the rate of the decay for both origin cirrus. Further work is needed, however this may provide some extra evidence of a diurnal cycle effect on the lifetime.

Figure 7: There are a number of very short trajectories which look to be anomalous. Would removing these have a noticeable impact on the results?

- Unfortunately, this is not possible in this work, since the TSC does not track individual trajectories. These short trajectories are often related the deep convection that exists across multiple time steps, and as such are replaced at each time step.. Removing lifetimes that are less than 5 hours, increases the median lifetime of both land and ocean detrained cirrus by less than 1 hour. This has been clarified on line 236:

  It should be noted that many of the very low lifetimes occur when convection replaces itself between timesteps. However, these instances have a negligible impact on the median lifetime. Removing these very low lifetimes (lifetimes less than 5 hours), increases the median lifetime by less than 1 hour.

Line 256: The wording of this is slightly unclear, as the radiative properties along each individual trajectory are not modified. The lifetime extension method appears to me to be asking "what if we saw an increase in the occurrence of long-lived detrained cirrus and a corresponding decrease in in situ cirrus". It might be clearer to rephrase the purpose of the lifetime experiment in these terms.

This is a good point, and the fact that our lifetime increase can also be thought of as a change in the distribution is something we have included in the discussion. We do not think that it negates the lifetime extension experiment, and understanding the extension in the frame of a change to the distribution may help aid readers to understand the lifetime extension a bit more, this has been included in the discussion beginning on line 288.

Line 260: I don't think that this approach is entirely analogous. The proposed lifetime extension approach increases the weighting given to longer-lived detrained cirrus at the expense of in-situ cirrus. As these longer lived cirrus are likely to belong to a distinct population (e.g. more organised and more intense convection, and may occur at different times of day…) this may have different results to stretching the observed properties included CRE along the TSC axis. I would expect that longer lived detrained cirrus tends to occur at higher altitudes and therefore has a more warming LW CRE than shorter lived cirrus at a lower altitude

- This is true, and as mentioned above it is something we have now included in the discussion beginning on line 388, that our lifetime extension works by increasing the distribution of already longer lived cirrus.

Line 263: It would be interesting to look deeper into how the lifetime of the observed cirrus trajectories relates to their properties. E.g. do shorter lived detrained cirrus have different average CRE to longer lived cirrus? Either over just the detrained cirrus lifetime or the entire duration of the trajectory.

- As mentioned above, this work doesn't track individual convective trajectories so it would require a rework of the methodology to look at individual trajectories and split up by lifetimes.

Figure 9: The change in CRE doesn't appear to be linear with the lifetime extension, it would be good to discuss this in the text

- This is true and is worth mentioning. The discussion of this begins on Line 339:

Interestingly the increase is not linear over time, with the first 50% lifetime extension increasing the CRE by 0.06Wm^2 more than the final 50% increase. One reason for this is the decreasing difference between the in situ CRE and detrained CRE at longer TSC values. Therefore any adjustment to the relative distributions at longer TSC will have a slightly smaller impact than initial adjustments to the shorter lifetimes.

Line 274: How sensitive are these values to changes in the previously calculated lifetime net CRE for detrained and in situ cirrus? I expect that uncertainty in these values may cause a larger uncertainty in the lifetime extension CRE than the interannual variability

The uncertainties given are in the change relative to the initial CRE with no lifetime extension. It is true that these, combined with the initial uncertainty, would likely be larger, we have included a discussion of this on line 331:

The uncertainties given represent the interannual variability in the increase in CRE for a given increase in detrained cirrus lifetime. In practice these uncertainties are likely larger due to the inherent uncertainties in the initial calculation of CRE, however these are normalised to a baseline, therefore there is no uncertainty in the lifetime extension when the lifetime extension is zero hours.

Figure 10: Would it be clearer to show 50% instead of 25%?

- We have updated our plot to show 50% instead of 25%.

Line 304: This is a little difficult to understand. Do you mean to say that this situation occurs in regions where the average low cloud cover is high (e.g. oceanic cold pools), and hence the average background albedo is also high. If a trajectory is over this location with an optically thin high cloud and no/little low cloud then the observed albedo would be lower than the average background, hence resulting in a "negative" high cloud SW CRE?

- Yes that is correct. This was a clearer way of putting it so we have included this 'case study' in the text to clarify, Line 371:

  This situation occurs in regions where the average low cloud cover is high, and therefore the average background albedo is high. Therefore, if a trajectory is observed at this location that contains an optically thin high cloud, and little to no low cloud, the observed albedo will be lower than the average albedo, thus resulting in a 'negative' high cloud SW CRE.

Line 305: This assumes that there is no correlation between high and low cloud cover. I am not sure whether or not this is the case, however.

- This is true, however explicitly considering the overlap between low and high cloud when calculated the CRE is out of scope for this study. We have added a sentence on this, beginning on line 368:

  This assumes that there is little correlation between the low and high cloud fraction, a fair assumption in many cases (Tompkins et al. 2015.)

Line 314: It would be interesting to see how changing the threshold for detrained vs in situ changes the CRE for each, possibly as supplementary materials

- This has been included in the supplementary as it was also requested by another reviewer (shown in the figures attached in the earlier comments)

Line 334: Is this such a simple statement to make? From a traditional view, if the average CRE of anvils is 0, then the lifetime should not matter… However, if the lifetime does not change proportionally (e.g. the cirrus decays more slowly at lower optical thicknesses than at higher), or there is a coupling with the variability of convection across the diurnal cycle (which you have shown is the case over land in this paper) then it may be important. I think you could make a much stronger argument from the results which you have presented here in favour of the anvil lifetime having an important role in the net anvil CRE, and that the impact of changes in anvil lifetime may be missed in previous studies such as those using RCEMIP style models with no diurnal cycle, or studies focusing on the tropical oceans where the diurnal cycle of convection is much weaker.

- Perhaps this isn't as simple as we made out in this sentence, we have therefore edited it below, on line 424:

  "The lifetime of detrained cirrus clouds from convection plays an important role in determining the overall detrained CRE in the tropics, and this may impact the total high cloud CRE in the tropics."

Technical corrections: We thank the reviewer for highlighting these, they have all been fixed.

Line 169: typo disappear -> disappear

Line 195: acronym; time since convection -> TSC

Figure 4/9 captions swapped

Line 240: do -> due

Figure 10: 0 value is not aligned between the two axes

Line 329: This sentence is duplicated from line 327

Line 362: Extraneous "9" at the end of the sentence