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

Commented [GE1]: Is it clear this follows that this is the upper bound?

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.

Commented [GE2]: Maybe you ant to say here that it is necessary to put these results in context and develop a stronger consraint on the potential change in CRE through?

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

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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*