Comments on *A Lagrangian perspective on the lifecycle and cloud radiative effect of deep convective clouds over Africa* by Jones, Stengel, & Stier

This paper by Jones et al. presents a Lagrangian analysis of anvil cloud properties over West Africa. By focusing on anvil clouds over land, the authors address an important gap in the anvil-radiation-climate literature, which in the past has mostly focused on maritime cloud systems. The synthesis of radiometer measurements, cloud property retrievals, radiative flux estimates, and a complex cloud-tracking algorithm is an impressive technical feat and makes a valuable contribution to the literature.

The text is well written and the figures are clear. The authors' technical expertise in cloud tracking and deep familiarity with the anvil cloud literature is also clear. Overall, the paper was a pleasure to read and I learned a lot from it.

My comments are mostly questions or relatively minor points of clarification. They can be addressed with some additional text and do not require further analysis, although there a few optional suggestions for small additions that could aid interpretation. For this reason, my recommendation is that the paper be accepted pending minor revisions.

Signed,

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Comments/Points of clarification

1. The authors importantly note that SEVIRI cannot reliably detect thin anvil, which have an important impact on net anvil CRE. Observational analyses over tropical oceans show that thin anvil cirrus with optical depth 1-2 are much more abundant than thicker anvils and, on average, have the largest effect on the TOA CRE (e.g., Berry & Mace 2014 Fig 12b; Hartmann & Berry 2017; Sokol & Hartmann 2020 Fig 1). It would be useful to provide an approximate optical depth (or similar metric) threshold that separates the anvils that are included in the analysis from those that are not. A threshold near τ~1 has very different implications than one near τ~4, etc.
   This limitation means that the mean CRE results are almost certainly biased low. While my hunch is that thin anvils are less abundant over land than in the maritime regions examined in those previous studies, I would not be surprised if the bias is of comparable magnitude to, or even larger than, the CERES bias correction. Since quantifying this bias can't be done easily, I think it would be fine just to note an approximate optical depth threshold and its implications for the results (i.e., that CRE is biased low, if that is something the authors agree with).

2. The authors might take interest in Gasparini et al 2022 (https://doi.org/10.1175/JCLI-D-21-0211.1), a modeling study that addresses some of the same questions addressed in this paper. Some of their results (e.g., their Figs 3 & 4) are quite relevant here and could serve as an important and useful point of comparison at several points throughout the paper. Lines 41-43 is where this paper first came to mind.

3. Is the CRE bias correction applied uniformly as the average over the entire region, or is it applied point-by-point using the spatially varying biases shown in Fig 3?

4. After reading through section 3 a few times, I was still unsure how the Lagrangian tracking system described on lines 148-162 differs from the two types of tracking systems mentioned earlier in the section. How does it overcome the issue described on line 145 (that the motion of a large system cannot be accurately described as a single vector)? I may be misunderstanding something here, but it could be worth adding a sentence that more clearly highlights the improvements that are being made over previous algorithms.

5. It would be useful to have a more detailed description of how the number of cores associated with a particular anvil is determined. This variable is used to sort the data in many of the figures. Does the number of cores reflect the total number of connected cores over the whole anvil life cycle, or do the cores have to appear simultaneously? How is an anvil associated with multiple cores if cores developing under existing anvils cannot be detected? Does this mean that the cores need to develop in close proximity at the same moment in time? I imagine many anvils might initially develop from a single convective core but merge with other anvils later on. It could be nice to have some more detail on this, even if it is only included as a supplement.

6. Is the analysis limited to anvils over land, or are all anvils included? Fig 5 shows that land is dominating the statistics either way, but I was unsure of this.

7. My interpretation of Fig 13 is that it shows the instantaneous CRE of anvil clouds as a function of CTT and time of day. But the sentence on lines 282-283 beginning with "Note", as well as the following paragraph, are talking about the lifetime-averaged CRE of anvils. This was a bit confusing. It is suggested on 282-283 that the CTT-dependence of the diurnal cycle of CRE is due to the different cloud lifetimes associated with different CTT. But if Fig 13 shows instantaneous CRE, it is hard to make any conclusions that depend on cloud lifetime.
   It could be a nice addition to show a figure that is similar to Fig 13 but showing the lifetime-integrated CRE as a function of CTT and the *initiation* time of the convective system. This would make it easy to interpret the combined effects of CTT and lifetime on CRE.

Minor line comments

8. Line 62-63: I think the arguments of Seeley et al (2019) are slightly misrepresented here. They found that the radiative tropopause temperature (which differs from the "inversion temperature", i.e. the cold point) was fixed across a wide range of surface temps. But they do not attribute this to FAT physics, which they argue are a weak constraint.

9. Line 69: there is a very recent observational analysis by Liu et al (2023) finding a decreasing trend in CTT that the authors may wish to include here. Reference below.

10. Line 83-84: typo "…to investigate both the CRE of individual CREs, as well as…"

11. Line 137-138: "Firstly, those…" not a complete sentence

12. Line 140-141: "Secondly, those…" not a complete sentence

13. Line 244: "CREover"

14. Line 265: "The overall negative average value of -8.17 W/m2 is approximately zero when considering the negative bias…". Earlier it is mentioned that the mean bias is -3.67 W/m2, which would bring the

total to -4.5 +/- 0.85 W/m2, which is different from zero. I am probably misinterpreting how the bias is applied (see comment #3 above).

15. Line 279: I think this should be "mean anvil CRE becomes more positive with **decreasing** CTT"

16. Line 280-281: I think this should be "This diurnal cycle effect is stronger for those anvils with **warmer** CTT"

References from above that are not already in the paper:

Berry, E., & Mace, G. G. (2014). Cloud properties and radiative effects of the Asian summer monsoon derived from A-Train data. Journal of Geophysical Research, 119(15), 9492–9508. https://doi.org/10.1002/2014JD021458

Gasparini, B., Sokol, A. B., Wall, C. J., Hartmann, D. L., & Blossey, P. N. (2022). Diurnal Differences in Tropical Maritime Anvil Cloud Evolution. Journal of Climate, 35(5), 1655–1677. https://doi.org/10.1175/JCLI-D-21-0211.1

Hartmann, D. L., & Berry, S. E. (2017). The balanced radiative effect of tropical anvil clouds. Journal of Geophysical Research, 122(9), 5003–5020. https://doi.org/10.1002/2017JD026460

Liu, H., Koren, I., & Altaratz, O. (2023). Observed decreasing trend in the upper-tropospheric cloud top temperature. Npj Climate and Atmospheric Science, 6(1), Article 1. https://doi.org/10.1038/s41612-023-00465-5