The response to Community:

This comment is a joint review created as part of EGU's Peer Review Training Workshop 2024. The reviewers were Erin Raif (University of Leeds), Piotr Markuszewski (Institute of Oceanology Polish Academy of Sciences) and Sebastián Mendoza-Téllez (Universidad Nacional Autónoma de México).

In this paper, the authors used a combination of satellite observations and model reanalysis data to constrain the contribution to effective radiative forcing from aerosol-cloud interactions (ERFaci) in low clouds. In doing so, the authors suggest that previous estimations overestimate ERFaci. They also find that the activation rate of aerosols into cloud droplets must be considered to reduce the uncertainty on effective radiative forcing.

This is an interesting hypothesis with important consequences for the calculation of cloud feedbacks. The data used is comprehensive, the analysis is thorough and the figures are largely clear. However, there is limited discussion placing this work into the context of works that have preceded it, and the importance of the results is not fully explored. Additionally, there are significant issues with the structure of the paper, which does not conform to a typical ACP structure and at times impedes comprehension of the content.

As such, we jointly recommend that this paper be reconsidered after major revisions.

We sincerely appreciate the community's constructive feedback and insightful suggestions. Since our manuscript was submitted to *ACP Letters*, which enforces strict word limits, we have prioritized presenting the main findings concisely. Where necessary, additional details have been included in the Appendix to provide further support for our findings. We kindly request your understanding on this matter. Below, we present specific responses to each comment, highlighted in blue.

Major comments:

1. The authors provide excellent detail to many aspects of their methodology. However, this should generally be contained within the main body of the paper, with only aspects that are unnecessary for comprehension of the methods remaining in the appendices. Additionally, the current structure of the methods/appendix means it is difficult to understand how the individual components fit together – we suggest that the authors **a)** add a short summary at the start of the new methods section to introduce them; **b)** explicitly discuss how their cloud-controlling factor analysis compares to the approach of Wall et al. (2022) and **c)** clearly introduce each data source to help readers familiar with either satellite or reanalysis methods only.

   a) Thank you for your suggestion. Due to the strict word limits of *ACP Letters*, we are unable to provide a more detailed expansion. However, we believe the current structure sufficiently introduces each ERFaci estimation method in Section 2.3, "Perfect-Model Cross Validation", with additional details thoroughly explained in Appendix A4. We appreciate your understanding.

b) Thank you for your comment. We compared our cloud-controlling factor analysis approach with Wall et al. (2022) in Section 2.2, "Observationally Constrained ERFaci", by differentiating between methods that account for activation rate and those that do not. Given the depth of comparison already provided, we believe further discussion in the main text would be redundant.

c) Thank you for your comment. To help readers distinguish between observational and reanalysis data sources, we clarify each data source's origin.

2. While the authors have done an good job of explaining their own approach, there is little discussion as to how this compares to previous work in the field. This leads to two key issues which should be addressed by further discussion of existing literature. **a)** It is difficult to establish from the introduction how ERF_aci is currently estimated, why this approach is limited and how this work differs from those previous approaches. **b)** Reading the discussion and conclusion, it is difficult to establish the relative importance of these new results that the abstract implies. Similarly, it is also difficult to understand the limitations of this approach.

   a) Thank you for your comment. I agree that a comprehensive introduction is important. However, due to the strict word limit of *ACP Letters* and the extensive content of our study, we are unable to expand further on the details you mentioned in the introduction. We appreciate your understanding. Additionally, Section 2.2, "Observationally Constrained ERFaci" compares our methodology with previous study and highlights our improvements in subsequent sections.

   b) Thank you for your comment. In response, we expand the conclusion section to align more closely with the journal's recommendations. This revision now provides a comprehensive discussion of the uncertainties our study has, ensuring a balanced view of the findings. However, given the strict word limit of ACP Letters, it is challenging to provide more details. We appreciate your understanding regarding these constraints and thank you for your thoughtful review.

3. The results and discussion may be better separated, or at least delineated further with some discussion of each result followed by section for discussion of the results in synthesis.

   Thank you for your comment. As mentioned above, we expand the conclusion to include a more comprehensive discussion of our results, including potential ERFaci values based on different data. However, due to the journal's word limit, we streamline this section as much as possible. We appreciate your understanding regarding these constraints.

4. If appropriate, it would be useful to present *p*-values alongside *r*-values throughout the paper to improve the statistical rigour of the findings.

Thank you for pointing this out. We revise the text to include p-values alongside r-values wherever correlation coefficients are reported, and we update captions to indicate “and their associated p-values (p)” for clarity.

Minor comments:

Line 34: The authors should consider providing a definition of radiative forcing while still early in the introduction to the paper.

Thank you for your comment. We revise the text to introduce radiative forcing earlier in the introduction. We now state: “Anthropogenic aerosols impact the Earth’s radiation balance at the top of the atmosphere, with this perturbation quantified as radiative forcing (e.g., Boucher et al., 2013; Raghuraman et al., 2021; Kramer et al., 2021).”.

Lines 34-36: Not all aerosols act to reduce precipitation and increase cloud liquid water path. For instance, ice-nucleating particles initiate ice formation, which has the opposite effects (though these are unlikely to affect tropical low clouds).

Thank you for your comment. You are correct that not all aerosols serve as CCN; some, such as ice-nucleating particles, initiate ice formation, which can have opposite effects on cloud properties. However, this study focuses specifically on aerosol-cloud interactions within low-level clouds, where ice formation is minimal, and CCN-related interactions are most prominent and well-documented (Christensen et al., 2016; Bellouin et al., 2020; Forster et al., 2021). This generalization is intended to streamline the discussion and maintain relevance to our specific focus on CCN-driven interactions.

Line 49: “the conventional assumption is that…”. It would be useful to know who makes this assumption.

Thank you for pointing this out. We revise the sentence to specify: “In some studies, the activation rate is not explicitly incorporated into the estimation process of ERFaci as it is implicitly assumed to have a one-to-one relationship (e.g. Chen et al., 2014; Christensen et al., 2016; Douglas and L’Ecuyer 2020; Wall et al., 2022, 2023).”.

Line 51: Is there a reference that justifies the "one-to-one aerosol cloud relationship" argument? [see also comments re. lines 70 and 80]

Thank you for your comment. In response, we revise the sentence to specify: “In some studies, the activation rate is not explicitly incorporated into the estimation process of ERFaci as it is implicitly assumed to have a one-to-one relationship (e.g. Chen et al., 2014; Christensen et al., 2016; Douglas and L’Ecuyer 2020; Wall et al., 2022, 2023).”.

Line 66: Can the authors here explicitly summarise the main focus of the "story" by presenting a research hypothesis or clear research questions?

Thank you for your comment. We appreciate the suggestion to clarify the focus. In the introduction, we highlighted the considerable uncertainties in estimating ERFaci and presented the main focus of the study, starting with "Estimating the ERFaci, especially in low-level clouds…". Additionally, we introduced the specific aerosol proxy we focus on. We believe this structure effectively conveys our research questions and objectives.

Lines 70-74: This goes some way to answering the comment on line 51, so should probably be moved to the introduction. However, further expansion may also be helpful for the reader – why is the assumption of a 1-1 ratio wrong?

Thank you for your comment. The assumption of a 1:1 ratio implies a direct, linear relationship between the variables in question—meaning that any increase in one variable would yield an equal proportional increase in the other. However, this assumption is often oversimplified in climate and atmospheric sciences because numerous factors influence the interaction between these variables. We clarify this in the main text with the addition: "…underscoring that not all SO_4 in the atmosphere are converted into cloud droplets.".

Line 80: The relative strength of the relationship in different regions is very interesting. In most regions, the relationship is proportional but not one-to-one. Could the authors clarify why this might not be expected, as SO4 is only a subset of aerosol?

Thank you for pointing this out. We clarify this in the main text with the addition: "This variation may be attributed to differences in local environmental conditions and the role of aerosols in which these clouds occur (e.g. Douglas and L'Ecuyer, 2019, 2020)."

Figure 1: We think the interpretation of the plot is good. Can correlation coefficients of 0.4 be described as highly linear?

Thank you for pointing this out. To avoid confusion and improve clarity, we remove the correlation coefficient from Figure 1 and Figure S1, as it was not ideal for visually representing the activation rate. Regarding linearity, we previously used diagonal hatching to indicate areas where correlation coefficients exceeded 0.4, as we considered this value relatively high for each grid point.

Line 113: Eq. 1 implies there are ten states that LWP can be in. Can the authors clarify what they mean by this, and perhaps briefly detail them?

Thank you for pointing this out. Initially, we considered using liquid water path (LWP) bins to further specify cloud properties based on LWP. However, to maintain consistency and directly compare our results with those of Wall et al. (2022), which do not constrain LWP, we decide to

exclude LWP in our equations. Even without this constraint, our results remain consistent, supporting the robustness of our findings.

Figure 2: The difference in meanings between panels (b) and (c) and panels (d) and (e) could be clarified by adding description to the colourbar adjacent to plot (c).

Thank you for your suggestion. We considered adding descriptions to the colorbar for clarity; however, we believe that the current labels "With activation" and "Without activation" more directly convey the differences between the panels.

Line 179-188: In Figures 3a and 3c, the authors clearly show the ensemble ERF_aci is improved by considering activation rate. However, there are similar absolute numbers of models which perform well regardless of the treatment. Are these the same models in each case, and if so, is there an indicator as to when considering activation rate is important to capture ERF_aci and when it is not?

Thank you for your comment. Upon review, we found that the models aligning well along the y=x line are indeed the same, indicating they consistently perform well in simulating aerosol-cloud interactions. However, as this detail falls slightly outside the main focus of our study, we opt not to include it in the main text. We appreciate your insight nonetheless.

Line 200-201: "…our estimates offer further evidence to support estimates on the lower end of [the WCRP's] range". This seems to contradict Fig 4, where the red bars indicating ERFaci_obs have the largest values. Do the authors mean to say less negative?

Thank you for pointing that out. We revise the text to "higher end" and add "(less negative)" for additional clarification.

Line 202: Could the authors clarify what a "top-down" approach is and how it differs from their analysis?

Thank you for your comment. We understand the concern and, to ensure clarity, we decide to replace the term "top-down" in the main text with "recent".

Line 220: What threshold was chosen for models to fall into the GOOD HIST category?

Thank you for your comment. We add further explanation in Appendix 1.4: "For analysis, we select the 15 models with the lowest GOOD HIST indices (Table S1).".

Figure 5: We assumed that, like other plots, the solid dots referred to values obtained when activation rate was considered. However, it would be useful to specify this in the caption. Additionally, there is no colourbar – we think this is because the colours correspond exactly to

the x-axis. If this this the case, consider removing the colours as it implies an extra variable (such as each colour representing a different model) and the yellow unfilled circles are difficult to see.

Thank you for your comment. For clarity, we add "when the activation rate is accounted for" to the caption. We choose to retain the current color scheme to visually highlight how our observationally constrained ERFaci values compare with model outputs, emphasizing the relative differences. We hope this provides a clearer comparison, though we appreciate your suggestion.

Line 247: For readers who are reading the paper non-linearly, consider specifying the degree to which the influence of aerosols may be less substantial than assumed.

Thank you for your comment. We add our estimates alongside the IPCC estimate in the text: "(e.g., -0.93 ± 0.7 W m^{-2} in IPCC AR6, 90% confidence)".

Line 335: Does the choice of a 50 year period remove interannual variability or reduce the influence of it? And if so, relative to what?

Thank you for pointing this out. The choice of a 50-year period helps reduce the influence of interannual variability by averaging over a longer timescale, which smooths out year-to-year fluctuations that might otherwise disproportionately affect shorter-term analyses. We revise the text to say "…to minimize the influence of interannual variability" for clarity.

Line 335-337: Was there a specific reason why the 13 and 9 models were chosen for SO4 and AI respectively?

Thank you for your comment. The choice of 13 models for SO_4 and 9 models for AI was based on model availability within CMIP6. We add the phrase "Due to the limited availability of models for aerosol proxies…"

Line 370: To make this clearer for the reader, consider ending this sentence with e.g. "in this case, SO4 concentrations or AI".

Thank you for pointing this out. To improve clarity, we add the following sentence: "Specifically, we consider either the natural logarithm of SO_4 at 925 hPa from the MERRA-2 reanalysis or the natural logarithm of the AI from MODIS.".

Line 403: What is the 1pctCO2 scenario?

Thank you for your comment. The 1pctCO2 scenario represents an experiment in which CO_2 concentrations increase by 1% per year, as provided by CMIP6. I revise the text to clarify this, now stating: "...where α_{1pctCO_2} represents the low-level cloud feedback, derived from the 1% CO_2 increase per year (1pctCO_2) scenario...".

Line 437: Consider using “more negative” rather than larger.

Thank you for your suggestion. In this context, the values represent multiples, so we believe “larger” is appropriate in this sentence to convey the intended meaning accurately.

Line 457: Is there any literature to back up the assertion that the polar oceans will not contribute largely to the ERF_aci?

Thank you for pointing this out. Assessing aerosol-cloud interactions in polar regions remains challenging due to significant cloud model uncertainties, spatial and temporal observation limitations, and the difficulty of obtaining some types of remote sensing information at high latitudes. These limitations make it difficult to quantify the polar oceans' contribution to ERFaci reliably.

Instead, we expand our study domain to cover the area between 60°S and 60°N, and our conclusions remain consistent within this extended area. We believe that excluding polar regions does not significantly impact our conclusions, as sulfate mass concentrations are primarily concentrated in major industrial regions like East Asia and North America, which are well represented within our study domain.

Additionally, to clarify, we adjust the text to avoid potential confusion regarding polar region contributions. We now highlight the extrapolation approach using CMIP6 single-forcing experiments, where we apply a scalar multiplier to the domain-average value to estimate a global-average value, as explained in Appendix A6 and illustrated in Figure A3.

Line 477: Consider a section title that is more specific than “Uncertainty”.

Thank you for your comment. We revise it to “**A7 Uncertainty from ERFaci_obs estimation**” to provide better specificity.

Table A1: Please add more detail to the caption, such as what the circles mean and brief redefinitions of the variables.

Thank you for pointing this out. We revise the caption to clarify: “CMIP6 models used in the analysis are represented, with each circle indicating the availability of data for a given model. $\Delta\ln(SO_4)$ and $\Delta\ln(AI)$ represent changes in sulfate mass concentration and aerosol index, respectively, from present day to pre-industrial levels on a natural logarithmic scale. ERFaci_true refers to ERFaci derived from single-forcing (aerosol-only) experiments, while ERFaci_SC17 is calculated using the method from Soden and Chung (2017). ERFaci_est (SO_4) and ERFaci_est (AI) denote estimated ERFaci values based on simplified version of Eq. (1) and Eq. (2) in the

CMIP6 models (Appendix A4). The GOOD HIST index represents the absolute difference in global-mean historical warming compared to observations (Appendix A1.4).".

Technical corrections:

Throughout: the authors should consider when to italicise and when to romanise variables and subscripts in equations, which is discussed in the ACP guidelines (https://www.atmospheric-chemistry-and-physics.net/submission.html#math)

Thank you for pointing this out. We correct the formatting of variables and subscripts in equations to adhere to the ACP guidelines.

Line 14: replace "it is assumed" with "assume".

Thank you for pointing that out. We revise the text, "While some studies assume that…".

Line 39: ERFaci has not yet been defined in the text, just in the abstract, so this should precede the abbreviation.

Thank you for pointing out. We revise the text to include the full term before introducing the abbreviation "ERFaci".

Figure 1, line 794: add the word "hatching" after diagonal.

Thank you for your comment. However, for improved visibility, we remove the diagonal hatching from Figure 1 and Figure S1.

Figure 1, line 796: "stippling" might be my new favourite word!

Thank you for your comment.

Figure 1, line 797: "Stduent's" should be "Student's".

Thank you for pointing out. We correct it.

Line 326: The sentence beginning "So, the models…" is a clause that doesn't form a full sentence. Consider a change such as "This suggests that the models…"

Thank you for your suggestion. To avoid multiple use of "that" we instead revise it to "This suggests the models that…".

Line 491: delete the second instance of the word "the" in the phrase "hence **C**ii represents the diagonal components of the **C**".

Thank you for pointing out. We correct it.

Lines 521 and 530: these are quite unwieldy and should probably be standalone equations.

Thank you for your suggestion. We revise these expressions as standalone equations to improve readability.

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