Author response to reviewer comments

We thank the reviewers for their close reading of the manuscript, their corrections and suggestions for improvements, and their pointing out where the manuscript, which we have tried to keep brief and to the point for the benefit of the reader, became so terse that it instead did the reader a disservice.

Response to RC1

This manuscript proposes and examines an emergent constraint between the radiative adjustment associated with the liquid water path (LWP) adjustment and the empirical LWP–drop number concentration (LWP–N_d) relationship. The latter is measurable in the present day while the former is the unknown target variable of great relevance to climate change. LWP–N_d relationships have been studied using multiple data sources including satellite remote sensing, climate model output and large eddy simulation (LES). An obvious concern is whether LWP–N_d relationships represent causality or simply correlation.

The manuscript is appropriately brief and well-written. There is a lot of information packed into the list of simulations that were performed and it is hard to figure out what they all mean. On the other hand, perhaps it doesn't matter much, for reasons I will discuss below:

We thank the reviewer for the comments, but we are perplexed by most of them. We respond in detail below.

Major concerns:

1. The methodology is so briefly mentioned. The set-up seems to be a modern era simulation that does not allow for significant circulation changes (fixed SST, sea ice, nudging to MERRA-2 winds) so this isn't a PD vs PI study. I appreciate the need to constrain the system so that clouds are not changing as they might in a true PD vs PI study. But this then begs the question as to why the authors stick with such coarse resolution/such poorly resolved clouds and cloud processes? How long were these simulations run for?

   The model setup is about as standard as it gets for effective radiative forcing studies, so, given the amount of information that needs to be conveyed, we decided to be terse. Nudging the wind fields to reduce the effects of internal variability on aerosol effective radiative forcing studies is as old (Kooperman et al., 2012; Zhang et al., 2014) as the effective radiative forcing framework (Sherwood et al., 2015; Boucher et al., 2014). The use of fixed SST and sea ice is even older because it essential for distinguishing between adjustments and feedbacks. We have added added a representative reference (Ghan et al., 2016). The simulations are one year each so that the required high-frequency output across PPE members could be accommodated. We thank the reviewer for pointing out this omission and have added it to the revised description.

   The reviewer is correct that this not a "true PD vs PI study", because that type of study would mix rather than separate feedbacks, greenhouse gas effective forcing, and aerosol effective forcing. In the main article text, we always use the phrasing "preindustrial emissions" to make this clear. We have clarified that this is the meaning of "$\mathcal{L}_{PD}/\mathcal{L}_{PI}$" in the revised abstract.

2. The one-off PPE set up is troubling because we know that it is very difficult to assess the influence of processes when a limited number of settings are changed. The interactive influence of different parameter settings can

provide unexpected responses. It means that drawing conclusions about the importance of a given process from the difference between the experiments in Table 1 is highly uncertain. Also, the number of parameters in this model is huge and I suspect that different combinations of parameter settings could give similar results, leading (possibly) to different conclusions.

We agree with the reviewer that there is always tension between making changes in model behavior easily attributable to a given process change and respecting the climate constraint and have made the same point (e.g., Mülmenstädt et al., 2020; Wynn et al., 2026). We correct for this by normalizing by the base-state $\mathcal{L}$. We already discuss that emulators of PPEs jointly perturbing parameters will provide an important test of our conclusions (l. 235 ff. in the original manuscript). Given that all of the EC signal comes from precipitation parameters and none from the other model parameters, we are however skeptical that there is a lurking story-altering interaction.

3. Following from 1), have the authors looked at an equivalent set of E3SM simulations at finer resolution/presumably better resolved cloud processes? For that matter, why not take a large set of LES simulations where processes are (mostly) resolved https://rmets.onlinelibrary.wiley.com/doi/10.1002/gdj3.70049. These were developed in-house and should be easily accessible. If results point in similar directions, one would have more confidence in the current results. Absent this, the current simulations are extremely difficult to parse.

   Our aim is to explain the known disagreement between GCMs and LES on the sign of the LWP adjustment. So we already know that the results will point in the opposite, not a similar, direction, but the point of the study is to explain why this is so, not provide yet another data point that it is so. If an LES modeling group is inspired by our results to look for similar emergent constraint candidates in an LES PPE, we will be delighted, but that is not our area of expertise.

4. m_l is dismissed based on Fig. S1. Why, other than it helps to limit the analysis to m_h? The two branches of LWP-Nd supposedly represent physical processes and if the model physics doesn't show a clear emergent constraint, shouldn't we question the representation of the warm phase physical processes that you have shown to be dominant in the emergent constraint with respect to m_h? The autoconversion and accretion processes are, after all, central to m_l.

   We disagree with the reviewer's assertion that we have "dismissed m_l". Rather (l. 170 ff. in the original manuscript), we point out that the absence of a relationship between m_l and $\mathcal{L}_{PD}/\mathcal{L}_{PI}$ is unexpected and explain that recent studies (Mülmenstädt et al., 2024a; Goren et al., 2025) have called into question the interpretation of the inverted-v slopes as a straightforward result of process susceptibilities. What we see in Fig. S1 is consistent with these recent studies.

5. Following from 1)–4), my concern is that the current manuscript presents another set of climate modeling results of limited use to the community because they are not sufficiently well-rooted in real-world clouds and aerosol-cloud interactions. My advice would be to pursue the 'structural' rather than the 'parameteric'.

   As we state numerous times in the manuscript, the goal is to find out what causes the discrepancy between GCMs and other lines of evidence (observations and LES). In our opinion, this is of great use to the community because these other lines of evidence are not infallible: establishing causality in observations is difficult, and we will never be able to afford LES spanning the entire diversity of boundary conditions imposed on warm clouds by the larger-scale circulation.

Other:

If my understanding of the model set-up is correct–i.e., this is not an historical PD vs PI set of simulations, doesn't some of the text need to be more carefully written? E.g., the CMIP6 models are historical PD vs PI runs. In that case there are other reasons why a model might exhibit a negative LWP adjustment in PD while at the same time LWP in PD is higher than that in PI.

We thank the reviewer for pointing out that there were instances in the manuscript where it was not clear that "PI" refers to preindustrial emissions only. We have corrected those instances (the abstract and the legend and caption of Fig. 1).

Coloring the points by class will be more effective than the change in symbol.

We prefer to use colors to highlight the main result (the EC).

Long sentence starting on line 232 is hard to follow.

We thank the reviewer for spotting this run-on sentence. We have split it into more digestible sentences in the revised manuscript.

Response to RC2

Summary

In this paper, Mülmenstädt and Ma propose a new emergent constraint for GCMs: the present day to preindustrial cloud liquid water path ratio (LWPpd/LWPpi) vs. the slope of the downward branch in the PD droplet number (Nd) vs. LWP ("inverted v") relationship. This typically negative slope is thought to be determined by the sedimentation-entrainment feedback. The authors utilize a perturbed parameter ensemble (PPE) in the DOE E3SM model where each ensemble member has been perturbed in their parametrizations of liquid phase microphysics, mixed phase microphysics, activation, and/or turbulence. The strength of the proposed emergent constraint relationship is heavily driven by the liquid phase microphysics perturbed members which have widely varying ratio and slope values. However, this is one of the key requirements from the Klein and Hall emergent constraint checklist: support of the underlying physical mechanism through perturbations of related physics. The relationship between the LWPpd/LWPpi ratio and the positive branch of the inverted v (typically thought of as controlled by precipitation-suppression) is surprisingly non-existent, possibly supporting the idea that precipitation alone is not driving this part of the relationship. Using the satellite-based estimation for the negative slope produces a constrained LWPpd/LWPpi ratio (if extrapolating beyond the PPE members) that implies a significant, positive value for LWP adjustment since the PI. GCMs generally disagree with this significant positive value but these results are consistent with other lines of evidence from the literature.

Overall, the paper presents a clear and elegant case for a new emergent constraint, carefully following the Klein and Hall criteria. The authors have created an exceptional paper, integrating multiple, sometimes competing ideas from the literature into an insightful investigation leading to several thought-provoking results that resolve some of the tension in the field. Using these results to address and reframe the two key questions that have been discussed around the inverted v (see conclusions) is particularly interesting. These results and new emergent constraint have the potential to significantly advance the field and modify the approach to GCM evaluations and parameterizations. I recommend prompt publication after some minor clarifications about the mechanism and specifics of the methodology.

We thank the reviewer for the encouraging comments and the generous highlight paper recommendation.

Minor and Typographical Comments:

Line 42-43: Surprising, does this imply that the underlying model physics is unable to produce this positive response in keeping with Bellouin? But then how can it achieve this in aggregate across the members?

This is a great point, and it touches on one of the unsettling features of emergent constraints: the constraint only exists if most of the ensemble members are "wrong", because if there is no model diversity, there is no empirical relationship that can be exploited for the EC. If all the models are wrong (extrapolation is required to match the PD truth), then presumably this unsettling feature becomes even more unsettling. The implicit hope in the EC approach is that the models are "wrong", but the relationship is still "right", whether for straightforward reasons (e.g., the models are structurally correct but the parameter choices are wrong) or less straightforward ones. Mülmenstädt et al. (2024b) found that the underlying model physics can produce an entrainment-mediated $\mathcal{L}$ adjustment of the Bellouin et al. (2020) sign in single-column simulations of DYCOMS-II stratocumulus, but the effect is vanishingly small in 3D climate runs.

Line 140-146: Thank you for explaining the logic of how you handle the base state and formulate the constraint. How much of an issue do you expect the lower order contributions to be here? Would comparing with other PPEs that have different base states help? No need to include further work, just curious about the implications.

For using $\mathcal{L}_{PD}/\mathcal{L}_{PI}$ as a proxy for $RA_{\mathcal{L}}$ across different base states, fortuitously, Brandon Duran has performed the exact experiment that was required to support the choice of normalizing by the base state $\mathcal{L}$: a PPE with diagnosed $RA_{\mathcal{L}}$ (using the Duran et al., 2025, method) against which $\Delta\mathcal{L}$ and $\Delta log\mathcal{L}$ could be compared as proxies. $\Delta log\mathcal{L}$ performs well. The preprint is now available (Duran et al., 2026), so we cite it in the revised manuscript. For next-to-leading order dependence of the perturbation on the base-state, the definitive answer requires a PPE that samples the parameter space uniformly enough that an emulator can be constructed, the emulated response can be restricted to the regions of parameter space that satisfy the base-state constraint, and the emergent constraint candidate can be tested in emulated observable and emulated climate response. We have added a pointer here to the relevant discussion in the Results and Discussion section.

Line 147-148: Is there an uncertainty on the slope? If so, please include as that potentially changes how much overlap the PPE members have with reality (i.e. the satellite estimate). It might also help to encapsulate some of the other factors potentially contributing to this satellite value (e.g. Goren et al 2024 as discussed).

There is an uncertainty on each ensemble member's m_h. We have added discussion and show each PPE member's $\sigma(m_h)$ in the revised manuscript. To the reviewer's question, the most negative m_h (PPE member E05) is approximately $3.4\sigma(m_h)$ away from the satellite value, so the "overlap with reality" is not zero (potentially even more so if we included the satellite uncertainty on m_h, which is not provided by Gryspeerdt et al., 2019).

Line 147-149: Apologies if I have missed this, but would it be possible to spell out the physical mechanism (i.e., lines 137-139) you expect to connect the LWP ratio and the slope here? I understand that they are both being driven by aci processes (sedimentation-entrainment feedback specifically?) and changes in aerosol from PI to PD but if there is a clearer linkage that you have in mind that would be very helpful to state and/or diagram (here or at the end). This seems particularly important to be precise about since there was a stronger relationship with m_h than m_l, despite the latter being more expected based on the physical mechanism prior.

We thank the reviewer for pointing out that this is the right place in the narrative to provide the expected physical mechanism that underlies the proposed EC relationship at this point in the manuscript. The link between $\mathcal{L}_{PD}/\mathcal{L}_{PI}$ and m_h is that the correlative relationship between N_d and $\mathcal{L}$ in present-day internal variability (as diagnosed by m_h) is influenced to some degree by the Ackerman et al. (2004) and Bretherton et al. (2007)-type ACI mechanisms, which are implicitly represented in the model physics (Mülmenstädt et al., 2024b), even if other mechanisms are present in or even dominate the present-day relationship. We have added this to the revised manuscript.

Line 155: Is it feasible to quantify this agreement with a goodness of fit statistic for all the members? I would be curious if that influences the ratio vs mh result (e.g., do outliers have worse fits to the inverted v?).

We thank the reviewer for suggesting this analysis. We use the residual sum of squares as our goodness of fit metric (each N_d bin is equally weighted so that our results are compatible with the Gryspeerdt et al. (2019) m_h). The RSS has no strong trend across m_h or differences between the experiment groups. The single ensemble member with positive m_h (E10) does have the highest RSS (and highest m_h standard error), but this could be simply a result of the m_h and m_l fit parameters moving closer to degeneracy when both slopes are positive. There are no clear patterns otherwise. We have added the goodness-of-fit discussion to the manuscript.

Line 165: In general, thank you for a detailed discussion of implications from the PPE member responses, very interesting.

We are pleased that the reviewer agrees that there is value to this discussion.

Line 175-178: Please expand on how the Nd outliers are potentially impacting this ml regression while also having a much lower peak location, I don't quite follow.

We probably overcomplicated the text here. The point is simply that the model places very few data points in the ascending branch of the inverted v, so the m_l fit result is uncertain for that reason. We have clarified the text in the revised manuscript.

Line 185: It looks like there is a negative relationship between these, not zero relationship, so is there some precipitation contribution? I think I'm missing your point, apologies.

This sentence fragment appears to have moved here by accident during editing. (Fig. S3 was made while we were analyzing the activation experiments, and the "no strong relationship" statement refers to the activation experiments.) We thank the reviewer for having pointed this sentence out and have removed the erroneous reference to Fig. S3.

Line 250, broader paragraph: Framing the slope as having value as a continuous variable really turns this question on its head and appears, throughout this paper, to be helping us progress beyond this stalemate. I do still wonder what the mechanism is exactly, see earlier point.

This is an excellent point. We have added an interpretation based on additive m_h contributions from "confounders" (e.g., circulation-mediated covariability between $\mathcal{L}$ and N_d Mülmenstädt et al., 2024a) that do not depend on anthropogenic aerosols and therefore do not contribute to $RA_\mathcal{L}$ on the one hand and "causal" contributions from the ACI mechanisms behind $RA_\mathcal{L}$ on the other.

Line 215-217, 251-253, 261-263: Very thought-provoking conclusion. Because extrapolation to the satellite estimate of m_h is needed, this implies that no members satisfy this constraint, right? It will be interesting to see if there are any GCMs or PPE members that do overlap the observed range, presumably a topic of future work based on the discussion.

That is correct, no PPE members actually achieve an m_h at or below the satellite value. We agree; coming up with GCMs with either a robustly positive $RA_\mathcal{L}$ or m_h more negative than the satellite m_h, or figuring out why it is so difficult to do, would be an important next step both in understanding this EC candidate and in finally resolving the disagreements between GCMs and the other lines of evidence.

Line 218: Typo: "candidates for emergent constraints"

Line 223: Typo: "constraint"

We thank the reviewer for pointing out these typos and have corrected them in the revised manuscript.

Response to RC3

The manuscript proposes an emergent constraint between the observable correlation of the liquid water path LWP and the cloud droplet number N_d and the unobservable liquid water path adjustment to anthropogenic aerosol–cloud forcing. The authors use a perturbed parameter ensemble for the E3SM model to investigate this dependency and describe the reasoning behind these parameter perturbations in detail.

A key point in the analysis is that the proposed emergent constraint is mostly reliant on the perturbations in the liquid microphysics. The result implies a positive radiative adjustment regarding LWP. This is in contrast to the negative adjustment usually simulated by most GCMs, but is in line with evidence from observations and process modelling. The strong correlation is identified as one important factor in the reasoning for an emergent constraint as described in Klein and Hall (2015). The remaining conceptual difficulties associated with establishing new emergent constraints are explained clearly.

We thank the reviewer for the comments and suggestions. Detailed responses below.

Major points

- In Figure 2 the variable on the y-axis ($L_{\mathrm{PD}}/L_{\mathrm{PI}}$) is relative, which probably reduces the influence of systematic biases in L. However, for m_h the systematic bias of the aerosol activation is not accounted for, which might be the reason that the runs with a perturbation in the aerosol activation (E12, 13, 18) are distant from the other runs. Could a suitable scaling for m_h, e.g., $d\ln(N)/d\ln(A)$, resolve this? Moreover, Figure 2 shows the "activation" category forming its own cluster. It seems that with more models here, this could form its own regression line parallel but shifted compared to the red line. Have you investigated this further?

See our combined response to this comment and the next comment.

- With the scaling mentioned in the former point, one would have $L_{\mathrm{PD}}/L_{\mathrm{PI}}$ as a function of $d\ln(N)/d\ln(A)$ and $d\ln(L)/d\ln(N)$. Could it be that this relationship already follows from the framework established in Bellouin et al (https://doi.org/10.1029/2019RG000660)?

These are good suggestions for understanding the perplexing differences between the activation experiments and the others. We note that m_h is relative within the PD, i.e., if the PD N_d or $\mathcal{L}$ (either or both) are scaled by a constant factor in one experiment compared to the others, the logarithmic slope is not affected. The suggestion of scaling m_h by $dlogN/dlogA$ is intriguing, since it completes the Bellouin et al. (2020) chain-rule factorization framework, as the reviewer points out. However, it would have to be the PD internal-variability $dlogN/dlogA$, since the x axis of the EC needs to be a PD observable. Given the complications in translating between PD internal-variability $dlog\mathcal{L}/dlogN_d$ and the PD–PI $dlog\mathcal{L}/dlogN_d$ (Mülmenstädt et al., 2024a, and similar studies), we feel adding another susceptibility to the analysis will have to wait for future work.

- You mention that you nudge winds towards MERRA (lines 107-109). Does this constrain the results you have obtained?

Yes; the nudging is effectively mechanism denial for any ACI mechanisms involving changes to the large-scale circulation. This limits the analysis, but the two classes of adjustment mechanisms we attempt to distinguish – microphysical precipitation suppression and microphysical/turbulent evaporation enhancement – should not be directly affected by the large-scale circulation effects. We have added this caveat to the revised manuscript.

Minor points

- You refer to Mülmenstädt 2024a,b multiple times. If the concepts of these publications are important here, it might help to at least shortly explain them in the current manuscript.

We feel that we do explain the concepts from Mülmenstädt et al. (2024a,b), in appropriate detail where they are needed in the narrative, together with work that has been performed by others on this topic. To single out Mülmenstädt et al. (2024a,b) for a longer summary compared to other works would, in our opinion, exaggerate their importance.

- Could the results of Hoffmann et al 2025 (https://acp.copernicus.org/articles/25/8657/2025) help to explain the large influence of the liquid microphysics?

We thank the reviewer for making this connection. We have added discussion of this point to the revised manuscript.

- line 127: You mention "three independent shape variables". To me it is not clear what the three are. m_l, m_h, and what else? The apex point as a single variable? Can you clarify this?

We thank the reviewer for flagging this statement. There are in fact four independent shape variables (m_l, m_h, N_d^{peak}, and $\mathcal{L}^{peak}$). We have corrected the revised manuscript.

- Line 130: the "to an anthropogenic perturbation." seems wrong at the end of the sentence, since you already say "linking an X to a Y" in that sentence.

This sentence is a bit cumbersome, but the "Y" in the reviewer's notation is "an unobservable emergent response of the climate to an anthropogenic perturbation". We have reworded the sentence so there are no longer two "to"s.

- Line 136: the "a" before "statistical techniques" seems wrong.

We thank the reviewer for catching this typo. Corrected.

- Lines 142: I do not understand the term "logarithmic difference" completely, but also do not understand why it is necessary/helpful. The only reference to the concept seems to be line 160. Out of curiosity, what does it help to understand?

We have expanded the parenthetical remark in the revised manuscript. The revised version provides references to studies that have demonstrated that $\Delta log\mathcal{L}$ is a good proxy for $RA_{\mathcal{L}}$ (Duran et al., 2026), motivating its use (or equivalently the use of $\mathcal{L}_{PD}/\mathcal{L}_{PI}$) over the use of $\Delta\mathcal{L}$.

- Lines 172 and 199: The interpretation of what the legs of the inverted-v represent would be helpful when the inverted-v equation is first explained.

We did not include the interpretation in the methods section because explaining the conventional interpretation of m_h and m_l as fingerprints of ACI adjustment processes, the evidence that this interpretation is incorrect, and the reasons why m_h nonetheless provides a good emergent constraint would be both long and duplicate the discussion that follows later. On reflection, we agree with the reviewer that we should provide a brief motivation here and have updated the manuscript accordingly.

References

Ackerman, A., Kirkpatrick, M., Stevens, D., and Toon, O.: The impact of humidity above stratiform clouds on indirect aerosol climate forcing, Nature, 432, 1014–1017, https://doi.org/10.1038/nature03174, 2004.

Bellouin, N., Quaas, J., Gryspeerdt, E., Kinne, S., Stier, P., Watson-Parris, D., Boucher, O., Carslaw, K. S., Christensen, M., Daniau, A.-L., Dufresne, J.-L., Feingold, G., Fiedler, S., Forster, P., Gettelman, A., Haywood, J. M., Lohmann, U., Malavelle, F., Mauritsen, T., McCoy, D. T., Myhre, G., Mülmenstädt, J., Neubauer, D., Possner, A., Rugenstein, M., Sato, Y., Schulz, M., Schwartz, S. E., Sourdeval, O., Storelvmo, T., Toll, V., Winker, D., and Stevens, B.: Bounding Global Aerosol Radiative Forcing of Climate Change, Rev. Geophys., 58, e2019RG000 660, https://doi.org/10.1029/2019RG000660, 2020.

Boucher, O., Randall, D., Artaxo, P., Bretherton, C., Feingold, G., Forster, P., Kerminen, V.-M., Kondo, Y., Liao, H., Lohmann, U., Rasch, P., Satheesh, S., Sherwood, S., Stevens, B., and Zhang, X.: Clouds and Aerosols, book section Chapter 7, pp. 571–658, Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, https://doi.org/10.1017/CBO9781107415324.016, 2014.

Bretherton, C. S., Blossey, P. N., and Uchida, J.: Cloud droplet sedimentation, entrainment efficiency, and subtropical stratocumulus albedo, Geophys. Res. Lett., 34, https://doi.org/10.1029/2006GL027648, 2007.

Duran, B. M., Wall, C. J., Lutsko, N. J., Michibata, T., Ma, P.-L., Qin, Y., Duffy, M. L., Medeiros, B., and Debolskiy, M.: A new method for diagnosing effective radiative forcing from aerosol–cloud interactions in climate models, Atmos. Chem. Phys., 25, 2123–2146, https://doi.org/10.5194/acp-25-2123-2025, 2025.

Duran, B. M., Wall, C. J., Lutsko, N. J., Wynn, S., Watson-Parris, D., and Mülmenstädt, J.: A new proxy for the LWP adjustment from first principles, ESS Open Archive, 2026, https://doi.org/10.22541/essoar.15006786/v1, 2026.

Ghan, S., Wang, M., Zhang, S., Ferrachat, S., Gettelman, A., Griesfeller, J., Kipling, Z., Lohmann, U., Morrison, H., Neubauer, D., Partridge, D. G., Stier, P., Takemura, T., Wang, H., and Zhang, K.: Challenges in constraining anthropogenic aerosol effects on cloud radiative forcing using present-day spatiotemporal variability, Proc. Nat. Acad. Sci. USA, 113, 5804–5811, https://doi.org/10.1073/pnas.1514036113, 2016.

Goren, T., Choudhury, G., Kretzschmar, J., and McCoy, I.: Co-variability drives the inverted-V sensitivity between liquid water path and droplet concentrations, Atmos. Chem. Phys., 25, 3413–3423, https://doi.org/10.5194/acp-25-3413-2025, 2025.

Gryspeerdt, E., Goren, T., Sourdeval, O., Quaas, J., Mülmenstädt, J., Dipu, S., Unglaub, C., Gettelman, A., and Christensen, M.: Constraining the aerosol influence on cloud liquid water path, Atmos. Chem. Phys., 19, 5331–5347, https://doi.org/10.5194/acp-19-5331-2019, 2019.

Kooperman, G. J., Pritchard, M. S., Ghan, S. J., Wang, M., Somerville, R. C. J., and Russell, L. M.: Constraining the influence of natural variability to improve estimates of global aerosol indirect effects in a nudged version of the Community Atmosphere Model 5, J. Geophys. Res., 117, D23 204, https://doi.org/10.1029/2012JD018588, 2012.

Mülmenstädt, J., Nam, C., Salzmann, M., Kretzschmar, J., L'Ecuyer, T. S., Lohmann, U., Ma, P.-L., Myhre, G., Neubauer, D., Stier, P., Suzuki, K., Wang, M., and Quaas, J.: Reducing the aerosol forcing uncertainty using observational constraints on warm rain processes, Science Adv., 6, eaaz6433, https://doi.org/10.1126/sciadv.aaz6433, 2020.

Mülmenstädt, J., Gryspeerdt, E., Dipu, S., Quaas, J., Ackerman, A. S., Fridlind, A. M., Tornow, F., Bauer, S. E., Gettelman, A., Ming, Y., Zheng, Y., Ma, P.-L., Wang, H., Zhang, K., Christensen, M. W., Varble, A. C., Leung, L. R., Liu, X., Neubauer, D., Partridge, D. G., Stier, P., and Takemura, T.: General circulation models simulate negative liquid water path-droplet number correlations, but anthropogenic aerosols still increase simulated liquid water path, Atmos. Chem. Phys., 24, 7331–7345, https://doi.org/10.5194/acp-24-7331-2024, 2024a.

Mülmenstädt, J., Ackerman, A. S., Fridlind, A. M., Huang, M., Ma, P.-L., Mahfouz, N., Bauer, S. E., Burrows, S. M., Christensen, M. W., Dipu, S., Gettelman, A., Leung, L. R., Tornow, F., Quaas, J., Varble, A. C., Wang, H., Zhang, K., and Zheng, Y.: Can general-circulation models represent cloud adjustments to aerosol–cloud interactions?, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/egusphere-2024-778, 2024b.

Sherwood, S. C., Bony, S., Boucher, O., Bretherton, C., Forster, P. M., Gregory, J. M., and Stevens, B.: ADJUSTMENTS IN THE FORCING-FEEDBACK FRAMEWORK FOR UNDERSTANDING CLIMATE CHANGE, Bull. Amer. Meteorol. Soc., 96, 217–228, https://doi.org/10.1175/BAMS-D-13-00167.1, 2015.

Wynn, S., Watson-Parris, D., Duran, B. M., and Mülmenstädt, J.: Base-state dependence of parameter sensitivity for the Twomey effect in a perturbed parameter ensemble, EGUsphere, 2026, 1–16, https://doi.org/10.5194/egusphere-2026-2811, 2026.

Zhang, K., Wan, H., Liu, X., Ghan, S. J., Kooperman, G. J., Ma, P.-L., Rasch, P. J., Neubauer, D., and Lohmann, U.: Technical Note: On the use of nudging for aerosol-climate model intercomparison studies, Atmos. Chem. Phys., 14, 8631–8645, https://doi.org/10.5194/acp-14-8631-2014, 2014.