Response to RC1 (Jianhao Zhang)

Thank you for your thoughtful review and suggestions for improved presentation. We enjoyed engaging with your ideas and questions for future work. Our responses are inline below.

Stimulated thoughts after reading (rather than comments):

- Regarding the disagreement between the causal experiment (PD-PI) and the internal variability, is there a possibility that the causal experiment is missing some feedback mechanisms (at longer timescales) that may be present in the internal variability, because of the fixed climatic boundary conditions? (I realize this may not contribute much to the disagreement, but just wondering. . .) About the climatic boundary conditions, you mentioned that SST is fixed, is the circulation (winds) also fixed? (I'm not very familiar with the setup of these experiments) For now, let's assume the internal variability (inverted v) captures the mean climate state where MET (large-scale conditions), Nd, and LWP are in balance (manifested in some climate scale correlations), perturbing Nd initially causes changes in LWP, which may later lead to circulation and/or SST changes (feedback from LWP to MET, and then possibly back to LWP). Is this potential feedback pathway artificially shutoff in these PI-versus-PD runs, based on the configuration?
  It is quite plausible that these mechanisms contribute in the real climate system. To include these effects in our model experiments, we would need to remove the fixed sea surface temperature and the nudging to the circulation. This would be more expensive, because it would require running the ocean model and would require longer integration time to average out the atmosphere/ocean coupled modes of variability; it would also mix in a cloud feedback signal because the aerosol ERF would cause the SST to decrease in a coupled experiment. Nevertheless, we agree – we are trying to understand a multiscale system, and artificially removing the circulation-mediated part of the response can only be an interim solution. We have added a caveat to the manuscript.

- Regarding the "funny" "doubly surprising" thing happened in CMIP6 models. I'm just curious is there any clue on what causes the CMIP6 models to get this inverted v (I understand the case for ModelE)? Are there any speculations? Is this due to the fact that the newer version of models better capture the mean climate states, thereby closer to observationally derived correlations? A following question is that if you use AeroCom IND3 models to predict the PI-LWP, would you get agreement with the causal experiment?
  This is still a puzzle. Unfortunately, the intersection between CMIP5 AeroCom models and CMIP6 models in this study is small (only CAM5/CAM6). We hope that an updated AeroCom experiment will provide comparisons between the CMIP5 and CMIP6 versions of more models. We also hope perturbed physics ensembles of each of the "inverted v" models will explore the effect of physics choices on the N_d–$\mathcal{L}$ relationship. We have included this response in the revised manuscript, since many other readers may have this question, too.

Some notes:

- Line 77, check spelling "ObservaTon"
  Corrected, thanks!

- Figure 7-12, perhaps it's worth mentioning these are results from E3SM in the captions? (I know this is clearly indicated in the main text, so, feel free to ignore this).
  Thanks for suggesting this addition. We have made this change to the figure captions.

- Figure 9, wind vectors are kind of hard to see, I suggest enlarging them (perhaps fewer of them will help too); is it better to indicate translated PBL depth in pressure or meter (more intuitive units)?
  Thanks for the suggestions. We have made the wind vectors fewer and larger. We have noted the (mean ± standard deviation, since the model uses hybrid sigma coordinates) PBL geometric depths in all figures that make reference to the PBL depth.

- Line 229 & Figure 11, regarding Nd-LWP correlation within each PBL depth bin (not shown), I wonder if it's worth showing, as I am curious about whether they look similar to what have been shown in Figure 7, i.e., in classic Simpson fashion, or different?
  We have decided against plotting the stratified regressions, which lie on top of each other and make for a messy plot. We have instead noted the very narrow range of regression slopes (i.e., not Simpson-like behavior) in the text.

- Just want to say that I really enjoyed reading Section 3.4 and the conclusion part. Great discussions! and I think the ACI community should really think carefully along these lines (i.e., representativeness versus/and causality) before producing tons of papers on the topic while not sure about how much of the results are causal.
  Thank you! Fortunately, most of the community recognizes these issues as caveats. We hope this study will provide further impetus to address the representativeness and causality problems.

Response to RC2 (Anna Possner)

Thank you for the thoughtful review and valuable suggestions for improving the analysis of stratified N_d–$\mathcal{L}$ relationships and the presentation. Our responses are inline below.

Comment on Methodology:

My most general comment is with respect to the statistics used in this study in Figs 7. and 11., which are discussed in sections 3.3.1 and 3.3.2. You introduce two distinct confounding variables here in these sections: surface precipitation rate or boundary layer depth. Both of these variables, as you state are not independent from your predictor variable Nd (and indeed your response variable LWP). The problem in binning in one variable, say PBL depth, and then looking at the slope in linear log space between averaged Nd and LWP is that you are already averaging out some of the co-variability that undoubtedly exists between predictor and response variable in each PBL depth bin. It thus skews your statistic (unless you got lucky) and the slope of the linear regression you obtain. It would be more accurate to assume that your response LWP variable co-varies with Nd and PBL and do a multi-variate fit. Or said differently: if you have an expression LWP = const. H_{pbl}^a Nd^b, then you can determine a and b using partial derivatives in log space. Note though that when integrated, these are only valid up to a constant! Therefore when determining a:=dln(LWP)/dln(Nd) at constant H_{pbl}, don't average, but fit slope instead.
Thank you for this suggestion. We have added monovariate and bivariate linear regressions for the potential confounding by precipitation and PBL depth to the text. The bivariate regression results are consistent with the binned analysis, but we agree that they provide useful quantitative information as well.

Minor Comments:

- Can you provide a solid argument for the 30% occurrence threshold. if not, how sensitive are your results to that parameter choice?
  We have updated the text and redesigned Fig. 1 to illustrate the rationale behind the f_{Sc} threshold. The aim was to select the subtropical Sc regions in a self-consistent way (i.e., recognizing that the model's Sc regions might be shifted with respect to observations). Thus, we chose a round-number contour that consistently excludes the midlatitudes across the globe; the limiting factor is the northward extent of the NEP Sc region. Setting the threshold at $f_{Sc} > 0.2$ would also have been a reasonable choice. As shown in Fig. R1, the N_d–$\mathcal{L}$ relationship in the $0.2 < f_{Sc} \leq 0.3$ bin looks similar to the $f_{Sc} > 0.3$ relationship, so this alternative threshold choice would not have greatly changed the N_d–$\mathcal{L}$ relationship.

- L212: PBL depth only goverened by anticyclonic subsidence? What about the gradient in SST?
  The SST influence is undoubtedly the major contributor on long timescales, and this is acknowledged in the discussion of the spatial covariability of cloud and aerosol properties immediately preceding this sentence.

Here, the concern is spatial covariability at a fixed location; our assumption is that the subtropical anticyclone, and thus the location of the subsidence maximum and continental aerosol advection on synoptic timescales, is a stronger contributor than the slowly varying SST field. We have clarified in the text that we are referring to synoptic timescales and that an analysis across timescales would be useful.

- Figs 8 and following: model level is not a meaningful quantity for people not directly involved in the study. Please provide more meaningful height intervals.
  Thanks for pointing this out. We have added the mean and standard deviation geometric PBL depths to the figures.

Edits:

- L53: I would remove brackets, its a stand-alone sentence
  Agreed, thanks!

- L75: Please state explicitly that all other experiments use model diagnosed LWP and Nd.
  We have adopted this suggestion.

- Figs 3 and following: Are these normalised PDFs around the edges? I don't remember seeing this written anywhere.
  We have made the wording of the captions more precise to specify that the marginal distributions are probability distributions and that the $N_d-\mathcal{L}$ relationships depicted are conditional probability distributions.

- Fig4 caption: I would include info that its CMIP6 era experiments in caption
  Agreed, thanks for this suggestion.

- Fig7: clarify that rain intervals intervals are given in brackets
  We have adopted this suggestion.

- L184: sentence containing „Nd distribution is noticably lower" is ambiguous to me. You mean the peak in the distribution is situated at lower Nd? All the distributions overlap, so how do you quantify „noticeably"?
  Thanks for noting this ambiguity. We have changed the text to "the peak of the N_d distribution is shifted lower".

- L232: Please rephrase „... equally accessible to clouds". LWP and Nd are cloud properties, so how can they not be accessible to a real cloud? You mean accessible to an observed or a simulated cloud, upon which limiters are imposed? Or do you just want to point out that the LWP and Nd phase space is not populated uniformly at equal density? Please clarify.
  Thanks for this comment. We did mean, as you surmised, that "not all parts of the $N_d-\mathcal{L}$ phase space are uniformly populated by clouds" and have changed the text to that phrasing.
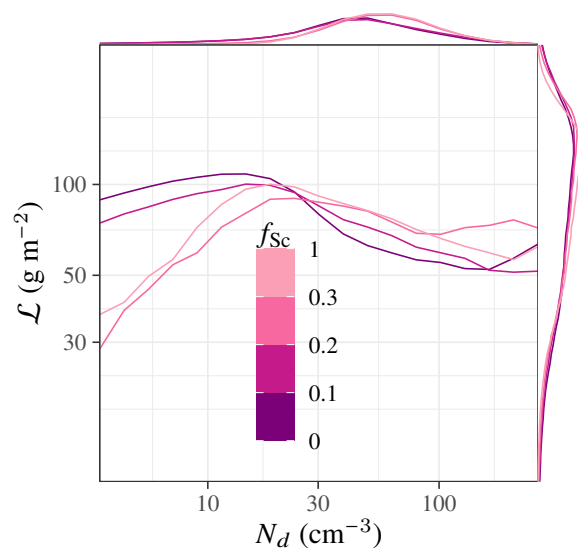
Figure R1: Variation of the N_d–$\mathcal{L}$ relationship with f_{Sc}. The $0.3 < f_{\mathrm{Sc}} \leq 1$ bin corresponds to the selection used in the manuscript.