This manuscript presents an analysis of parameter sensitivities within a CAM6 perturbed parameter ensemble using a decomposition of ERFaci into the Twomey effect, liquid water path adjustments and cloud fraction adjustments. The decomposition provides useful insight into which processes control different components of aerosol-cloud forcing.

The authors show that parameter sensitivities for the Twomey effect change substantially when the ensemble is constrained to parameter combinations that reproduce the default CAM6 climate state. In the unconstrained ensemble, the Twomey effect is primarily controlled by parameters influencing cloud state. After constraining the ensemble to reproduce the CAM6 base state, the dominant sensitivities shift towards parameters more directly linked to aerosol–cloud interactions. I found it useful to see this demonstrated explicitly for the Twomey effect and its decomposition within CAM6.

However, throughout the paper, reproducing the CAM6 default climate is often interpreted as identifying a more “realistic” cloud state. For example, the manuscript states that "allowing unrealistic cloud properties can distort the assessment of parameter sensitivity, leading to incorrect conclusions about parameter importance" (line 86), refers to the constrained ensemble as representing a "realistic state of the model" (line 146), and concludes that "constraining to the base state is therefore essential to inferring the correct parameters and processes" (line 38) and "essential when interpreting PPE output" (line 183). I am not convinced that these conclusions follow directly from the analysis presented here.

I have major concerns regarding the use of the model base state as a constraint, and I do not think the conclusions drawn from it are sufficiently justified by the current analysis. Some of these concerns relate to the interpretation of the results and the proposed use of the methodology to other models, while others reflect a lack of discussion or explanation of important aspects of the approach. I expand on these points below.

Major comment 1: The manuscript treats the CAM6 base state as a trustworthy reference state without sufficiently justifying why it should be trusted.

The central methodological choice in the paper is to define a posterior consisting of parameter combinations that reproduce the default CAM6 global mean LWP, IWP, liquid cloud fraction and TOA shortwave flux. I think I understand the motivation for this approach. It is computationally inexpensive, avoids the additional challenges associated with observational constraints, and provides an easy “window” into the model processes without being obscured by parameter combinations that generate climates very different from the default CAM6 state.

However, very little information is provided regarding why this particular state should be regarded as an “correct” reference.

A lot of caution is needed when constraining to a model state that has itself already been tuned toward observations. In some sense, this is a second-hand constraint, where the PPE is being constrained to a climate that already reflects tuning decisions and model biases. I would need a more balanced discussion of the strengths and limitations of the method.

At minimum, I think the paper needs mention of:

- how the CAM6 default configuration was obtained,
- what observations were used during tuning,
- known biases in the default climate,
- why reproducing this climate should be expected to improve estimates of parameter importance,
- circumstances where this approach could fail.

Overall, I found it unclear what scientific question the proposed constraint is intended to address. Is the objective to quantify uncertainty in ERFaci for this model? To support model development? Or to provide a practical alternative to observational constraint? Clarifying the intended use-case would help evaluate the approach.

I would also need to see more information about how the posterior was constructed. What thresholds were used for acceptance? How many samples were retained? What was the acceptance rate? Were identical criteria used for both the base-state posterior and the observationally constrained posterior? The manuscript refers to rejection sampling and cites Eidhammer et al. (2024) (line 235), but I couldn’t find a discussion of rejection sampling in that paper.

I do acknowledge that the authors include an observationally constrained posterior in the supplement and show that it produces broadly similar conclusions to the base-state constraint. This comparison is central, as it is the main support for justifying the constrain to the model base state as an alternative to observations. However, I do not think the observationally constrained analysis is discussed in sufficient detail given its importance to the manuscript's central argument. For example, I would be interested in seeing the equivalent of Figure 2 for the observational constraint, or at least some discussion of whether the dominant regional sensitivity patterns remain unchanged. This seems particularly relevant given that Figures S8 and S12 suggest the two posteriors are not entirely equivalent.

Additionally, if the methodology ultimately requires validation against an observational constraint, it is not clear to me what additional benefit is gained from first constraining to the model base state. The manuscript ultimately proposes the base-state constraint as a “model-agnostic method that can be easily adopted across all PPE output to produce more physically meaningful parameter sensitivities " (lines 196–198). This recommendation cannot be made based on the current analysis. The conclusions rely on a specific CAM6 base state and a particular set of constraint variables. Different models have different base states chosen based on 1- on different criteria and 2- restricted to how well each model structure can match observations. Constraining to the default model state will not necessarily lead to more physically meaningful parameter sensitivities and could potentially rule out parts of parameter space that are actually more realistic, so I would be very cautious about the recommendation that this approach should be adopted across PPEs.

Major comment 2: Are global mean constraints sufficient?

My other concern is that reproducing these global mean quantities does not necessarily imply that the cloud states governing Twomey susceptibility are appropriately constrained. Many different climate states can share similar global mean LWP, IWP, cloud fraction and TOA flux values while having substantially different regional cloud distributions and cloud regimes.

The central argument of the paper is that unconstrained cloud states distort estimates of Twomey sensitivity, since cloud state determines susceptibility to aerosol perturbations. However, if climate states with very different regional cloud structures can satisfy the chosen global mean constraints, then their regional Twomey sensitivities may also differ substantially.

I do not think it has been demonstrated that these global mean constraints are sufficient for the problem being addressed, particularly given the emphasis placed on the spatial sensitivity patterns in Figure 2. For example, have the retained members been evaluated in terms of regional cloud fraction, regional LWP, stratocumulus regimes, or other cloud properties relevant to aerosol-cloud interactions?

Additional comments

Interpretation of the sensitivity changes in Figure 1

I would appreciate more discussion of the physical mechanisms underlying the sensitivity patterns shown in Figure 1. For example, line 95 state that variations in *micro_mg_autocon_lwp_exp* greatly impact the amount of cooling from the Twomey effect, but additional explanation of the physical pathway would help with interpretation. This comment also extends to lines 96–100 or e.g. *microp_aero_npccn_scale* in line 127. To me, the sensitivity analysis of the decomposed ERFaci components is one of the most interesting aspects of the paper, but I am missing some of the interpretation of why particular parameters emerge as important.

The same applies to the descriptions of LWP and CF adjustment sensitivities. The manuscript shows that the dominant parameters for these adjustments remain largely unchanged after applying the constraint, whereas the Twomey effect is strongly affected. Some additional discussion of the physical reasons for this would strengthen the interpretation of the results and help explain why the Twomey effect appears substantially more state-dependent than the cloud adjustments.

Interpretation of Figure S8

I think the discussion of Figure S8 could be expanded considerably. One of the most interesting aspects of the figure is that the retained parameter distributions do not necessarily converge towards the default parameter values (in some cases, the retained parameter values favour regions of parameter space quite different from the default value, e.g. for *micro_mg_autocon_lwp_exp*)

I would appreciate more discussion of how this should be interpreted. This seems like it could be related to equifinality and compensating errors (where different parameter combinations can reproduce the same global mean cloud state through different physical pathways)?

Line 75: The statement that the authors are not aware of previous studies that have explicitly quantified such state dependence in aerosol-cloud interaction sensitivities should be revisited. For example, Regayre et al. (2026) showed that parameter importance and spatial patterns of uncertainty changed substantially before and after observational constraint, and emphasised the need to reassess process importance after constraining a PPE. It may be more useful to position the novelty of the present study more specifically in its analysis of the individual ERFaci components?

Figure 2: Have the authors examined the second most influential parameter at each location? Is the dominant parameter clearly dominant, or is there often a close second?

Figures S3 and S11: Consider adjusting the y-axis limits to make the distribution shapes easier to interpret.

Figure S2 and S5: "test" left as x-axis label.

References

Eidhammer, T., Gettelman, A., Thayer-Calder, K., Watson-Parris, D., Elsaesser, G., Morrison, H., van Lier-Walqui, M., Song, C., and McCoy, D.: An extensible perturbed parameter ensemble for the Community Atmosphere Model version 6, Geosci. Model Dev., 17, 7835–7853, https://doi.org/10.5194/gmd-17-7835-2024, 2024.

Regayre, L. A., Prévost, L. M. C., Ghosh, K., Johnson, J. S., Oakley, J. E., Owen, J., Webb, I., and Carslaw, K. S.: Remaining aerosol forcing uncertainty after observational constraint and the processes that cause it, Atmospheric Chemistry and Physics, 26, 2293–2317, https://doi.org/10.5194/acp-26-2293-2026, 2026.