Review of He et al. 2026

State dependence of aerosol forcing and its implications for climateResponse

In this work, the authors examine the state dependence of aerosol forcing using fixed-SST simulations in two GFDL model configurations across a wide range of base states (+/- 8K). They illustrate that aerosol forcing weakens in warmer base state climates, for different reasons. They establish an interesting LW aerosol forcing pathway due to more complex representations of aerosol-ice-cloud interactions in the MG2 variant of the model. Changes in warm clouds in response to aerosol are connected to diminished cloud susceptibility due to optically thicker clouds in warmer base states. Direct aerosol effects are related to changes in aerosol burden, removal, and relative humidity changes. Lastly, the connection between aerosol forcing and climate feedbacks is discussed, with potential implications for future studies.

My main concern with the manuscript is that several of the main claims are not supported by the figures. This does not invalidate the author's arguments entirely, but suggests that either 1) the results do not follow the arguments put forth or 2) the figures do not demonstrate the results clearly enough. Either way, this must be resolved before this work is published. Additionally, many of the appendix figures are mentioned throughout the text, so the authors should think further about how they want to visually convey / support their interpretations. As it stands, I believe there are both 1) too many figures and 2) they don't consistently support the arguments of the work.

Upon major revisions, I believe the work would be a meaningful contribution.

Minor Comments

Line 77: I believe White (2025) have quite a wide array of different base state and perturbation experiments, so this description might need revising. (their Table 1)

Line 122: Would it be possible to add one sentence here about how the total aerosol forcing between the two model configurations differs in a base state scenario? (E.g., how much does accounting for aerosol-ice-cloud interactions change how the model simulates aerosol forcing).

Additionally, given the results that follow, it should be clearer what exact model changes exist between AM4 and AM4-MG2 that directly affect ice nucleation (how was ice nucleation parameterized in AM4? Was there no parameterized aerosol effect on ice clouds?

Line 138: I might be missing this, but what is the justification for using 1990s aerosols for present day (PD)? I'm aware of year 2000 vs 2014 being a common difference between simulations, but I'm not familiar with 1990s as a PD reference year. (I'm aware this likely does not change the actual forcing by that much, but for consistency with prior studies this choice should be justified).

Line 177: add citation to COSP, since this is satellite simulator output (Bodas-Salcedo 2011 and Swales 2018)

Line 184: “decomposed by layer” Layer here meaning pressure layers?

Line 191: “Both GFDL-AM4.0 and GFDL-AM4-MG2 show a systematic weakening of aerosol forcing as the base-state climate warms”
I don’t see this. In neither case is the decrease systematic (m8K less negative than m4K in Am4, as well as p4K more negative than 0K; More systematic in AM4-MG2 for 0K and colder climates, but then the forcing flatlines for p4K and p8K). And I don’t think it is the case if you isolate either the SW or LW forcing, either.

Fig 1: The linear slopes given in each subplot are not very easy to interpret.

Line 207: “In GFDL-AM4.0, variations in shortwave ERF primarily determine the state dependence of total ERF”
I’m not sure I see this either. It looks like the SW forcing in AM4 varies between [-1.2, -1] W/m2 whereas the LW varies between [0, +0.15] W/m2, so the range seems quite similar.

Line 222: “ERFaci is the dominant forcing component and exhibits substantial weakening with warming”
Again, I don’t know if Figure 2 supports this. Sure, in MG2 there is more of a consistent weakening trend from -8K to +8K, but in AM4, only +8K is much weaker.
Fig 2: As in fig 1, the linear slopes in each panel are difficult to interpret. I’d suggest omitting them.

Line 238: “This non-monotonic behavior may reflect limited liquid or supercooled liquid water availability in the coldest base state, which could constrain aerosol activation and reduce the sensitivity of cloud optical properties to aerosol perturbations.”
Given that the non-monotonic behavior is notable, do the authors investigate this further? Is there substantially lower N_d in -8K, which would support the aerosol activation hypothesis?

Line 242: “Shortwave ERFaci is comparatively less state dependent, whereas longwave ERFaci becomes substantially less negative as the base-state climate warms”

I understand the argument for why the LW forcing is stronger in the MG2 configuration, but why is the SW ERFaci less state dependent? Is the warm cloud response similar to AM4, but the SW effects of ice cloud changes partially mute this? Or are the warm cloud changes also not consistent between the two model configurations?

Figures: suggest using a color scheme for the different experiments that is more consistent with their mean state (-8K, -4K blue, +4K +8K red, 0K grey). This would help with interpretation, as the figures currently are a bit hard to distinguish (especially the zonal mean plots)

Fig 4: is this full NH mean (0-90) or 0-40? I thought it would be 0-40 since that is the division mentioned in the text. I think panel labels would be helpful.

Line 293: It could be useful to look at the mean state cloud fraction distribution for each of the different base states, to demonstrate / confirm the hypothesis that warm clouds on average are optically thicker (and hence less susceptible to aerosol) under warming.

Line 298: “but can strengthen in colder or mixed-phase regimes as warming increases liquid or supercooled liquid water availability, allowing aerosol perturbations to more effectively modify cloud optical thickness”

Line 300: cloud layers or pressure layers?

Line 330: What does “high cloud” refer to? Is it non-low clouds from the ISCCP simulator (CTP < 680 hPa), or a more traditional measure (CTP < 440 hPa)? Define

Line 339: Is it possible to examine this pathway more clearly? Are CT ice crystal sizes larger, N_i smaller, and IWC decreased in the colder climates? This is an interesting mechanism, but more evidence needs to be shown. However, changes in optically thin high clouds in response to aerosol have been found in other GCMs (Duran 2026), so it might be interesting to compare / contrast the GFDL results to the CESM2 results. Has this ice cloud response been noted in other base state studies?

Line 354: how does static stability change in these simulations?

379: “precipitation climatology increases” Mean changes? Or changes in extremes?

Section 3.3: The results in this section are interesting, but beyond what is demonstrated in figure 5 (which is that column aerosol mass and AOD don’t always yield the same picture), the lack of figures makes the proposed mechanisms challenging to follow. Specifically, lines 377-416 give several explanations for changes in direct forcing, reliant on precipitation, precipitation efficiency, relative humidity, and abundance / lifetime changes. While it seems plausible that all these mechanisms influence the final response, it was difficult to feel convinced by the arguments here. I’d suggest the authors rethink how they form (and support) this section.

Section 3.4: I found this section difficult to follow, and not immediately clear what its conclusion was, beyond the potential shared link of changes in cloud susceptibility between cooler and warmer climates applying to both aerosol forcing and cloud feedbacks. I’d suggest a more concise summary.

Line 509: Can the authors cite an example or two of studies which neglect to consider or mention this nuanced distinction?

Section 4.2: It is not immediately clear from these 3 paragraphs what is being argued, other than interpreting aerosol forcing should be done with more caution.

Line 535: Is it possible to make any comparisons to other base state work (e.g., White 2025, or Zhu 2025)? Further discussion could help contextualize the GFDL results here, which alone are hard to interpret.

References:

Bodas-Salcedo, Alejandro, et al. "COSP: Satellite simulation software for model assessment." Bulletin of the American Meteorological Society 92.8 (2011): 1023-1043.

Duran, Brandon M., Nicholas J. Lutsko, and Casey J. Wall. "Aerosol–Ice–Cloud Interactions in a Perturbed Parameter Ensemble." Journal of Climate 39.14 (2026): 4183-4203.

Swales, Dustin J., Robert Pincus, and Alejandro Bodas-Salcedo. "The cloud feedback model intercomparison project observational simulator package: Version 2." Geoscientific Model Development 11.1 (2018): 77-81.

White, Kayla, Dunyu Liu, and Geeta Persad. "Absence of aerosol indirect effect dependence on background climate state in NCAR CESM2." Journal of Climate 38.1 (2025): 147-163.

Zhu, Jiang, et al. "Investigating the State Dependence of Cloud Feedback Using a Suite of Perturbed Parameter Ensembles." Journal of Climate 38.16 (2025): 4063-4081.