Review

A modified parameterization of stratiform cloud microphysics for the Community Earth System Model.

Recommendation: Accept with revisions.

The paper illustrates the effects of modifications to the microphysics parameterization in the Community Atmosphere Model, version 6 (CAM6). The modifications deal with species representation; droplet activation; additional mechanisms for secondary ice production (SIP); and emulations from bin microphysics for aggregation, accretion, and riming. The paper also diagnoses the impact of homogeneous freezing and SIP and analyzes the effect of large increases in ice nucleating particles (INP). Only one case is analyzed, using SCAM6, the single column model for CAM6.

Overall, the paper provides important documentation of the behavior of advances in cloud microphysics in atmospheric models and warrants publication. The mechanism denial experiments for homogeneous freezing and SIP do not align well with the modifications to the microphysics but are nonetheless of interest, as are the sensitivity experiments for INP.

Revisions are suggested in the following.

Major Revisions

1. ll. 93, 478: The statement should describe more clearly the Jadav et al. (2025) convective parameterization. Is it the Zhang-McFarlane (ZM) parameterization in SCAM6 (Gettelman et al., 2019), but with only changes in microphysics, or are there also changes in other basic attributes like its plume model, closure, and entrainment-detrainment assumptions? The MG08 simulations presumably use the convection parameterization in the default SCAM6. If Jadav involves changing more than just the convective microphysics in ZM, the differences between the MG08 and LS24 (or LS25, hereafter LS) simulations are not just due to the microphysics modifications but also other differences in the treatment of convection. If this is the case, the paper should distinguish between the effects of microphysics modification and changes in treatment of convection not related to microphysics. L. 478 refers to “improved treatment of convective detrainment.” Is the source of

this improvement just microphysical, or does it also involve other changes introduced in the Jadav convective parameterization?

2. Fig. 1-3: In general, the observational uncertainties are large. In some cases, e.g. Fig. 3a, it's difficult to confidently choose between the two parameterizations regarding possible consistency with observations. On Fig. 3b, MG08 correlates better than LS with CIP but is more biased. For Figs. 1-3, I recommend adding metrics to the revised text (bias, RMSE, correlation coefficients) for MG08 and LS relative to observations.

3. Only one case in a single field campaign is presented. The authors should note this limitation and offer any insights possible on how general their results are.

Minor Revisions

1. CESM would more accurately be referred to throughout the paper as CAM6. CESM is generally taken to refer to the fully coupled Earth system model, of which CAM6 is a component. The paper is restricted to a single-column application of CAM6 with no coupled or global results.

2. Consider re-titling to avoid "for CESM." The parameterization has not at present been included in CESM, and the parameterization is sufficiently general that it could be used in other general circulation models for climate or numerical weather prediction. A revised title could be something like "Single-column evaluation and diagnosis of modified cloud microphysics in the Community Atmosphere Model, version 6".

3. l. 460: The paper does not show the relative roles of homogeneous droplet freezing versus homogeneous aerosol freezing. Can this statement be supported further?

4. On l. 251, specify whether any nudging to observations was done when applying large-scale tendencies in SCAM6.

5. ll. 417, 421, 470: Text states snow number concentration is reduced in high-INP, but for almost all temperatures warmer than -36^{O}C Fig. 3c shows increased snow number concentration at high-INP.

6. ll. 278, 326, 327: The LS radiation predictions are more accurate for SW but not LW.

7. l. 86: Define acronym AC here, its first occurrence, rather than later at l. 95.

8. The paper mixes use of "LS24" and "LS25." This should be consistent throughout.

9. It's unclear whether the "MG08" model against which "LS" is compared used the Jadav et al. (2025) convective microphysics or the convective microphysics cited in Gettelman et al. (2019). If the latter, titling and other characterizations of the microphysics should not be modified by "stratiform," as both convective and stratiform microphysics would be modified.

10. The Ming et al. (2006) activation scheme depends on vertical velocity. The grid-mean vertical velocity is not representative of subgrid variability in vertical velocity, e.g., at cloud bases, cf. Golaz et al. (2011, *J. Clim.*). How are vertical velocities specified in your application of Ming et al. (2006)?

11. Are aerosols also depleted by dry deposition? Mention in text, if so.

12. Table 1, Mineral dust geometric mean diameter: Should second number be smaller than first?

13. l. 112: The slope parameter p_x would be better defined here rather than later at l. 124. Consider moving the definitions for the distributions to this part of the paper, rather than including them later in the text. Mention early in the text, when symbols first appear, that you have provided Table A1 with a complete list of symbols.

14. l. 131: Ming et al. (2006) also links the droplet number concentration to vertical velocity.

15. Fig. 5 legend: errors regarding item (c)

16. ll. 369-370: Fig. 5(d) shows noticeable impacts from excluding breakup and homogeneous freezing. Also, at temperatures just below freezing, excluding SIP cancels much of the effect of excluding homogeneous freez-

ing. Can this be commented upon?

l. 72: “Grabowski” $->$ “Gettelman”?
l. 320: subscript error
l. 338: “stratospheric” $->$ “stratiform”?