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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Incorporation of advanced aerosol activation treatments into CESM/CAM5: model evaluation and impacts on aerosol indirect effects" *by* B. Gantt et al.

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This manuscript compares one-year simulations by the Community Earth System Model using its standard aerosol activation with simulations using several more advanced schemes. While several aspects of the clouds are evidently simulated more realistically with the more advanced scheme, the manuscript overextends itself by examining impacts on climate, which is inappropriate given the brevity of the simulation. I recommend limiting the analysis to the response of the cloud properties, which is interesting enough. This will require only minor revision. Other comments will also require special attention, but rerunning is not required.

1. Page 32292, Lines 22-23. Since SWCF is negative, and increase would make the value smaller in magnitude. You might it is 13% more negative. Also, it's a 4% decrease in net surface downward solar.

2. Page 32292, Line 29. A 0.9 C cooling is not slight. Why not focus on impact of activation changes on aerosol radiative forcing, with ocean surface temperature fixed?

3. Page 32293, line 10. Should use the AR5 nomenclature here.

4. Page 32294, line 1. I would say the ARG scheme uses a semi-empirical treatment of supersaturation. It's not based on regressions, but coefficients on physically-based terms are adjusted to achieve agreement with numerical simulations.

5. Page 32294, line 10. Insert "multiple" before "lognormal".

6. Page 32294, lines 20-21. Replace "which" with "that".

7. Page 32295, line 18. New paragraph here.

8. Page 32296. Line 28. Instead of Neale et al., cite Liu et al. (2011a): Liu, X., R. C. Easter, S. J. Ghan, R. Zaveri, P. Rasch, J.-F. Lamarque, A. Gettelman, H. Morrison, F. Vitt, A. Conley, S. Park, R. Neale, C. Hannay, A. Ekman, P. Hess, N. Mahowald, W. Collins, M. Iacono, C. Bretherton, M. Flanner, D. Mitchell, 2012: Toward a minimal representation of aerosols in climate models: Description and evaluation in the Community Atmosphere Model CAM5. Geosci. Model Dev., 5, 709–739, doi:10.5194/gmd-5-709-2012.

9. Page 32298, lines 4-5. Why do you use the entrainment rate from deep convection to treat entrainment effects on activation? CAM5 only treats activation in stratiform clouds. Using entrainment from the deep convection scheme is inappropriate for stratiform clouds. If you want to treat entrainment effects, treat activation in shallow and deep convective clouds.

10. Page 32298, line 22. A one year simulation seems very short for estimating effects

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on SWCF. How do you know it is long enough? What are the initial conditions? Why did you choose a coupled simulation?

11. Page 32300, line 6. NMB is bias normalized by the mean?

12. Page 32302, line 16. Liu et al. (2011) should now be Liu et al. (2011b).

13. Page 32302, line 22. It's likely that the treatment of ice nucleation affects LWP in the arctic. See, e.g., Engstrom et al., J Climate, 2014.

14. 32303, line 12. Changes of 4% for SWDOWN is not small in absolute terms. Note that CAM5 is highly tuned with the ARG scheme to produce a small NMB for SW flux. A variety of cloud parameters have been adjusted. Retuning with FN would be required to produce small NMB values again.

15. Page 32303, lines 15-16. What is the basis for this suggestion? LWP increases considerably, so the increase in CDF can't explain all of the change in SWDOWN.

16. Page 32303, lines 16-17. I really doubt this, as LW saturates quickly, and hence depends more on cloud altitude and CF than LWP.

17. Page Page 32303, lines 20-22. How can NMB be so large for T2? If the mean is 270 C, an NMB of 10% is 27 C! Doubling the NMB is NOT slightly larger.

18. Page 32304, lines 15-17. While relating CDNC biases to AOD biases is tempting given the ubiquity of AOD retrievals, it would be helpful to know if the simulated CCN is biased. There is CCN data available, albeit not nearly as pervasive as AOD data.

19. Page 32305, line 22. Why is the difference so much larger than that found by Ghan et al. (2011)? I really doubt the greater change is because Ghan et al. compared column droplet number rather than low-level droplet number concentration. Has the FN scheme changed? Please note and explain this change.

20. Page 32307, lines 17-20. A more likely explanation is that most clouds in the tropics are convective, which do not treat activation and hence are not dependent on

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the activation parameterization.

21. Page 32307, lines 25-28. The treatment of ice nucleation can have a large influence on LWP in low arctic clouds. You can cite Liu et al. (2011b). The following sentence notes this. The difference between MODIS retrievals and the simulated cloud properties in the arctic is much greater than the difference between the properties simulated by the different activation schemes. This suggests the sensitivity to the treatment of droplet number is not that important there.

22. Page 32308, lines 7-25. Since ocean temperature is allowed to respond to the changes in the cloud properties, one cannot ascribe all of the change in SWCF to the changes in aerosol activation. The feedback of the ocean temperature changes on SWCF must also be considered. It cannot be separated from the experiment design, but the feedback should at least be discussed. Better to have prescribed ocean surface conditions.

23. Page 32308, line 26 – page 32309, line 15. Why do you show and discuss changes in T2 and precipitation? The coupled model is far from being fully adjusted to the solar flux changes after just one year of simulated time. The reduction in precipitation is not simply due to inhibition of autoconversion, as the surface is cooling, thus suppressing evapotranspiration. I suggest you remove this entire paragraph.

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