Response to comments from Reviewer 1

Brett Gantt et al.

Reviewer 1: *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.*

Reply:

We thank the reviewer for positive comments. Please see our point-by-point reply below.

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

Reply:

Since SWCF is a negative variable, more negative actually means an increase in SWCF, rather than decrease. For example, a SWCF of -47 W/m2 is greater than a SWCF of -41.6 W/m2 by 13%, although its numeric value is smaller. To avoid the confusion, we changed "an increase in shortwave cloud forcing of 13 %" to "an increase (more negative) in shortwave cloud forcing of 13%"

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?

Reply:

Although having a fixed ocean surface temperature would help to isolate the radiative feedbacks from aerosols, we chose to run the coupled model to accurately reflect the impact of aerosol activation on the Earth System. While a one-year simulation does not allow us to make conclusions about the impact of aerosol activation on climate, it does allow us to run several sensitivity studies to estimate the sign and potential magnitude of these impacts on climate forcing. Admittedly, simulations on the order of decades or longer would likely be required to allow all components of CESM to come closer to equilibrium even without changing anthropogenic forcing. Due to the limitations of a one-year simulation, we have remove discussion of precipitation and temperature in the updated manuscript and adjusted the tone to reflect the reality that these results are estimates of the potential magnitude to aerosol radiative forcing and that longer simulations are required for more definitive results.

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

Reply:

Corrected in the updated manuscript.

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.

Reply:

Corrected in the updated manuscript.

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

Reply:

Corrected in the updated manuscript.

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

Reply:

Corrected in the updated manuscript.

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

Reply:

Corrected in the updated manuscript.

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.

Reply:

Corrected in the updated manuscript, except that the final revised paper is given as Liu et al., (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.

Reply:

We agree with the reviewer that entrainment from deep convection is not the appropriate parameter when examining activation in stratiform clouds. Because the inclusion of entrainment in shallow and deep convective clouds requires closer linkage of the activation and convection model processes than found in our current implementation, we have removed entrainment impacts from the text and figures.

10. Page 32298, line 22. A one year simulation seems very short for estimating effects on SWCF. How do you know it is long enough? What are the initial conditions? Why did you choose a coupled simulation?

Reply:

We agree that 1-yr is a very short period, however, our experiment is meaningful in examining the impact of different aerosol activation modules on predicted cloud/radiative variables.

To determine if the changes in model predictions such as SWCF due to changes in model configurations from the 1-yr simulation are statistically significant, the student's t-test analysis is performed between the runs pairs of 2001 simulations with different aerosol activation modules. A probability value from the student's t-test is 1×10^{-12}, which is less than 0.05 (i.e., 5%), indicating that the differences between the simulation pairs are statistically significant at the 95% confidence level. The results show that the changes in most cloud/radiative variables including SWCF due to changes in model configurations are statistically significant.

The initial conditions for CAM5 are derived from a 10-yr (1990-2000) CAM5 standalone simulation with the MOZART chemistry provided by NCAR. A 1-year (January 1-December 31, 2000) CESM/CAM5 simulation using NCAR's CESM B_1850-2000_CAM5_CN component set is performed as spinup to provide the initial conditions for meteorological variables and chemical species that are treated in both MOZART and CB05_GE. An additional 3-month (October 1-December 31, 2000) CESM/CAM5 simulation based on a 10-month (January-October, 2000) CESM/CAM5 output using initial conditions from NCAR's CESM B_1850-2000_CAM5_CN is performed as spinup to provide initial conditions for chemical species that are treated in CB05_GE but not in MOZART. to provide initial conditions for chemical species that are treated in CB05_GE but not in MOZART. The initial conditions have been clarified in the updated manuscript.

We selected the coupled version of CESM to realistically simulate the impact of aerosol activation within an Earth Systems framework. This has been clarified in the updated manuscript.

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

Reply:

Correct. The Normalized Mean Bias (NMB) is given by:

$NMB = [\sum_{i=1}^{N}(M_i - O_i)] / \sum_{i=1}^{N} O_i = (\frac{\overline{M}}{\overline{O}} - 1)$**, where** $\overline{M} = (1/N)\sum_{i=1}^{N} M_i$**,** $\overline{O} = (1/N)\sum_{i=1}^{N} O_i$**,** M_i **and** O_i **are values of model prediction and observation at time and location** i**, respectively.** N **is the number of samples (by time and/or location).**

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

Reply:

We kept as Liu et al. (2011) because the other reference is Liu et al. (2012)

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.

Reply:

Reference to Engstrom et al. (2014) is included in the updated manuscript.

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.

Reply:

We agree with the reviewer that the radiative changes occurring from the different aerosol activation parameterizations are not small in absolute terms. We consider retuning the model with a new activation scheme to be beyond of the scope of this work because our purpose is to examine the changes in model predictions (in particular cloud/radiative properties) caused by different aerosol activation parameterizations. Due to the potential tuning issues, we now mention model tuning in the discussion of SWDOWN and other radiation variables in the text of the updated manuscript.

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.

Reply:

We agree that the change in CF cannot explain changes in SWDOWN, which is caused by changes in several cloud variables. The statement has been changed to:

"The larger underprediction of SWDOWN in the FN05 series of simulations is likely associated in part with the overprediction in CF and in part with increases in CDNC, LWP, and COT."

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.

Reply:

This statement has been removed.

17. 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.

Reply:

See response to comment #2 concerning temperature and precipitation evaluation.

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.

Reply:

We agree with the reviewer that evaluation of predicted CCN would be more informative to the attribution of CDNC biases. We now include global satellite-model CCN comparison in Tables 2 and 3 along with brief discussion within the text of the updated manuscript.

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.

Reply:

The FN scheme used in our work was the latest version that contains several updates, although the FN05 remains the same as that used in Ghan et al. (2011).

To address the comments, we have included the following discussion in the updated manuscript: "This increase is substantially larger than the 20–50% increase reported by Ghan et al. (2011) for CAM5 but closer in magnitude (although larger) to the 100% increase reported by Zhang et al. (2012) for GU-WRF/Chem. Such differences can be attributed to differences in mass accommodation coefficients of water vapor used (1.0 in AR-G00 vs 0.06 in FN05), methods in solving max supersaturation, the temperature-dependence in the calculation of Kelvin effects (temperature dependence is neglected in AR-G00 but accounted for in FN05).

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 the activation parameterization.

Reply:

This point has been added in the updated manuscript.

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.

Reply:

We agree with the reviewer that the properties of clouds in polar regions are not sensitive to treatment of droplet number and have cited Liu et al. (2011) and others in the updated manuscript.

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.

Reply:

We have included discussion of ocean-atmosphere feedbacks in the updated manuscript.

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.

Reply:

Discussion of temperature and precipitation changes due to the different activation schemes has been removed in the updated manuscript.