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Interactive comment on "Putting the clouds back in aerosol-cloud interactions" *by* A. Gettelman

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This manuscript summarizes a study with a clear message: the representation of cloud microphysics is a significant source of uncertainty in estimates of aerosol indirect effects. The presentation is generally clear and concise. Methods are for the most part appropriate, with exceptions noted below.

General comments

The manuscript mostly discusses sensitivity of the shortwave signature, but the long-wave effect is also sensitive to the various parameterization changes (varying by up to 0.8 W/m2), and contributes substantially to the net sensitivity. More discussion of the processes involved in LWCF is needed. These cannot be explained by changes in LWP only. Too often the manuscript associates %reductions in ACI to %reductions in

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LWP, as if LWP changes drives ACI changes. That is not true if LWCF changes are involved, or if the Twomey effect dominates.

The residual in Table 2 is large because of the use of the change in dirty-sky cloud forcing as the measure of ACI. It would be much smaller if you use the change in clean-sky cloud forcing for ACI. Why not use it? You have the fields you need.

There is a lot of noise in Figure 6 because you only have 5 years of results from simulations that were not nudged. It is therefore difficult to determine which differences are significant. To produce a more definitive result, you should either extend the simulations another 5 years or rerun with nudging of winds to a common wind simulation.

Technical comments

Page 20777, line 9. It doesn't make a big difference, but the sulfate in CAM5 is assumed to be ammonium sulfate, not sulfuric acid. The ammonium is not simulated separately, but is assumed to be available to neutralize the acid.

Page 20777, line 14. You might also cite the following, which reached the same conclusion: Ghan, S. J., S. J. Smith, M. Wang, K. Zhang, K. Pringle, K. Carslaw, J. Pierce, S. Bauer, and P. Adams, 2013: A simple model of global aerosol indirect effects. J. Geophys. Res., 118, 1-20, doi:10.1002/jgrd.50567.

Page 20777, line 19. I don't understand the used of "indicates". The reasoning doesn't follow. I suggest replacing "indicates uncertainties about" with "depends on".

Page 20778, line 8, page 20780, line 5, and page 20794, lines 10-12. Citation is Abdul-Razzak and Ghan (2000): Abdul-Razzak, H., and S. J. Ghan, 2000: A parameterization of aerosol activation. Part 2: Multiple aerosol types. J. Geophys. Res., 105, 6837-6844.

Page 20780, line 12. Replace comma with a semi-colon.

Page 20780, lines 23-24 and page 20785 line 21. Ghan (13) doesn't correct for clear-

sky aerosols. It is based on the clean-sky cloud radiative forcing, so it involves cloudy sky as well as clear sky. I recommend deleting "correcting for clear sky aerosols".

Page 20782, line 25 – page 20781, line 8. What are the albedo changes with respect to? What is the baseline? Is the change the change over time or due to a parameter change? It is very surprising that the LWP term in 2b is so small, given the large changes evident in 1a. Please explain.

Page 20784 line 1. Change converge to coverage.

Figure 8. I don't find this figure particularly informative. Yes, LWP is important. But so many different things are changed in the various experiments that it doesn't make much sense to look about how the Nd response varies with ACI. And I don't understand why delta Re is always negative; shouldn't it increase with increasing aerosol?

Page 20786, lines 18-20. This is not surprising, because autoconversion is decoupled from droplet size in the no lifetime exp.

Page 20787, line 21. Actually, mixed phase clouds in cam5 are sensitive dust, but since dust is not anthropogenic it is better in insert "anthropogenic" before "aerosols". You might also note somewhere that the changes in LWCF are driven by homogeneous nucleation of sulfate, but that is not so much a mixed phase cloud effect. See Ghan et al. J Climate 2012.

Page 20788, line 2. A factor of 10 is quite large. How is that justified? Figure 8. Are all of the colored places statistically significant?

Page 20789, lines 3-5. This implies the clouds in the equatorial east Pacific are not stratocumulus? Are you sure about that?

Page 20791, lines 1-2. This merely reflects the linearity of cloud optical depth with LWP. There are several sublinear relationships in the relationship between emissions and forcing that Figure 7 does not address.

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Page 20793, line 11. Replace is with are.

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