

Interactive
Comment

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

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I thank the reviewers for their detailed comments. I have made several substantial changes to the manuscript in response to the reviews, and I believe have been able to answer all of the reviewers' concerns. These have significantly improved the manuscript.

ACI is now reported following Ghan 2013 as the 'clean sky' ACI, as requested. This changes some of the numbers, but not the conclusions.

Significantly, I have done some additional simulations to better characterize the uncertainty in the TOA forcing from 5 year simulations as requested by reviewer 1. This includes a 20 year simulation, and two nudged simulations. The 20 year simulation allows an analysis of variance of 5 year periods. The nudged simulations actual produce

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slightly different clouds and ACI, so this is mentioned.

In addition, better justification to why the sensitivity tests (with references) is noted in several places in the manuscript as requested by reviewer #2, and we have noted some further discussion of the LW cloud effects in several places. We have tried to make sure our statements in the abstract and conclusions are consistent with the results, and made the statements less assertive as requested.

The off line tests are still in the paper, with a bit more text better linking the tests in the conclusions to the rest of the text. But the idealized tests are important in showing a consistent message.

I think all these improvements will satisfy the reviewers and hopefully make the manuscript suitable for publication in ACP.

Detailed replies are below:

Review #1 (Ghan)

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/m^2), and contributes substantially to the net sensitivity. More discussion of the processes involved in LWCF is needed.

»We have done some further investigation of the changes to the LWCF. In fact, much of the LWCF changes are due to changes in high clouds, with offsetting local LW and SW effects. We now note this in the text.

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. Noted. Also Gettelman et al 2012.

These cannot be explained by changes in LWP only. Too often the manuscript associates %reductions in ACI to %reductions in LWP, as if LWP changes drives ACI changes. That is not true if LWCF changes are involved, or if the Twomey effect dominates.

» As noted above, the LWCF changes appear to be locally offset by SWCF changes. We have looked at the LW in more detail, and added some discussion of the LW effects to the discussion of the mixed phase ice nucleation, where it matters the most, and to the discussion section as requested.

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.

» We have redone the tables and figures, and now report ACI throughout the paper as the 'clean sky' ACI following Ghan 2013. This does reduce the residual.

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.

» We have done some significant extra work to better characterize the variability in ACI as suggested by this comment, and added this to the text. We performed 2 additional nudging experiments, as well as experiments with 20 years of simulation. Nudging was performed by nudging to another CAM simulation using (a) U,V and T or (b) just U,V. The latter was performed to explore whether 'semi-direct' effects might matter. Results indicate significant differences in ACI relative to the free running model. This is noted

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in the text.

A better approach is to actually run out one of the base simulations to see what the variance is. We performed a 20 year simulation for present and pre-industrial emissions with the 'MG2' case. This analysis indicates that with a 5 year simulation we get within 10% of the 20 year value: within 0.05-0.08 for ACI and LW/SW components and within 0.04 Wm⁻² for direct effects. We now note this in the text.

»The 'noise' in Figure 6 is actually real variance in the zonal mean, and the nudging experiments have similar structure in the zonal mean. We also note this in the text.

»We thank the reviewer for these comments, it has resulted in a better paper.

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.

» Added reference. Thanks for pointing me to this paper.

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".

» Changed.

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.

» Changed

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

» Changed

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".

» Changed as suggested. ACI using clean sky CRE is now used throughout.

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?

» The albedo change is a difference between the time average of two simulations. Clarified in the text.

It is very surprising that the LWP term in 2b is so small, given the large changes evident in 1a. Please explain.

» Cloud mass is changing with cloud coverage (noted in Figure 1D), so most of the difference in figure 1A (Cloud Mass) is not in-cloud water content, but the extent of clouds. This is now noted in the text.

Page 20784 line 1. Change converge to coverage.

» Thanks for catching that. Changed.

Figure 8. I don't find this figure particularly informative.

» I think you mean Figure 7.

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.

» We have added a sentence explaining the logic here: We explore a variety of differ-

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ent metrics that might contribute to radiative effects: changes in cloud mass, number concentration, effective radius and total cloud cover.

And I don't understand why delta Re is always negative; shouldn't it increase with increasing aerosol?

» Re gets smaller with increasing aerosols (higher number concentrations, smaller drops), so for Present - Preindustrial, drops were larger in the past, and smaller today, hence negative.

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

» ??? If N_c doesn't change, and LWP doesn't change, how does Re change???

Page 20787, line 21. Actually, mixed phase clouds in cam5 are sensitive dust, but since dust is not anthropogenic it is better to insert "anthropogenic" before "aerosols".

» Changed.

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.

» Added under more discussion of the LW effects.

Page 20788, line 2. A factor of 10 is quite large. How is that justified?

» We now note this in the text. There is previous work that found better agreement with extremely cold Antarctic supercooled clouds by reducing the vapor deposition rate by 100 (Lawson and Gettelman 2014), and recent work by Korolev (2008) showing that the dynamics of clouds mean that the vapor deposition onto ice should act about half the time.

Lawson, R. Paul, and Andrew Gettelman. "Impact of Antarctic Mixed-Phase Clouds on

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Climate.” Proceedings of the National Academy of Sciences 111, no. 51 (December 23, 2014): 18156–61. doi:10.1073/pnas.1418197111.

Korolev, Alexei V. “Rates of Phase Transformations in Mixed-Phase Clouds.” Quarterly Journal of the Royal Meteorological Society 134, no. 632 (April 2008): 595–608. doi:10.1002/qj.230.

Figure 8. Are all of the colored places statistically significant?

» Yes, noted. The region in white is chosen based on the average local standard deviation of annual TOA flux of about $\pm 3 \text{ Wm}^{-2}$.

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

» Re-phrased so as not to imply the E. Pacific does not have stratocumulus clouds.

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.

Page 20793, line 11. Replace is with are.

» Done

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 20775, 2015.

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