Atmos. Chem. Phys. Discuss., 15, C8418–C8424, 2015
www.atmos-chem-phys-discuss.net/15/C8418/2015/

Atmospheric Chemistry and Physics
Discussions

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

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Received and published: 23 October 2015

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

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 #2

This manuscript performed sensitivity tests to examine how different processes contribute to the uncertainties in ACI. Based on the sensitivity results, the author argued that uncertainties in cloud microphysical processes contribute more to the uncertainties in ACI, stronger than uncertainties due to natural aerosol emissions. Given the large uncertainties in ACI and given the large uncertainties in cloud-related processes in climate models, the topic is timely and highly relevant to ACP. The method is generally appropriate. I would recommend the publication of the manuscript after my following comments are addressed.

Major comments:

The main conclusion of the paper is that cloud-related processes contributed more to the uncertainties in ACI than aerosol-related processes (the author used "cloud microphysics" in the abstract seems not accurate, as CLUBB in itself is cloud macrophysics). This conclusion may not be a surprise to many of us in the field, as this has been hinted in many previous sensitivity studies (on this aspect, I would suggest the author to add more relevant papers).

» The conclusion is not surprising to many, but seems to have gotten lost, hence the need for this work. To better reflect the previous work, we have added references suggested below.

But the hard part is to provide solid evidence to make an assertive statement on this. One challenge is that whether the sensitivity experiments performed in the manuscript were designed in a way to systematically examine key uncertainties in cloud-related processes. I would suggest the author to add more discussions on this.

» We have added a discussion of the motivation for these tests to the details of the description of the experiments in the introduction to section 4. The motivation for each set of tests comes from previous work, which we now cite. We have added some discussion to introduction and conclusions putting this in context as well, and noting that a more comprehensive statistical ensemble is in the planning stages.

Another even bigger challenge associated with these sensitivity tests is whether these experiments are equally realistic. This is less a problem with aerosol-related processes, as the perturbation in aerosol-related processes usually has less impact on the model climatology, but this can be a big issue for cloud-related sensitivity experiments as cloud-related changes can significantly perturb the model climatology. Table 2 documented the anthropogenic radiative forcing from these different tests, but it is not clear how realistic each of these experiments are. To partly address this issue, I would also think the relative change in radiative fluxes may be more relevant than the absolute changes, as cloud radiative forcing may be different across different experiments. Adding how the corresponding fields in present-day simulations in Table 2 can be helpful as well.

» Added columns to Table 3 with the base state CRE: actually these are not that different between experiments. The experiments all have fairly realistic climates. We also

added the base state of the cloud microphysics and the changes to cloud microphysics in the table.

I would also suggest the author to use less assertive statement in the abstract and the main text about how cloud-related processes contribute to the uncertainties in ACI, as the current assertive statement may require more evidence that is not supported by the manuscript.

» We have added language to the abstract and the main text indicating that we are exploring a subset of possible uncertainties identified by previous work, so as not to claim more than we are showing. The main point as well is to show relative importance of clouds and aerosol processes, and we also note this in the conclusions. In the conclusions we noted that these sensitivity tests may not be fully representative in all models. Also added a note that a more quantitative investigation (using PPE methods similar to Carslaw et al 2013) is in development.

The manuscript includes both off-line microphysical tests and global sensitivity tests. But it seems that the off-line tests do not add much. Removing the off-line tests would have little impact on the main conclusions of the manuscript.

» We respectfully submit that the off-line tests do add to the paper by showing that similar results are gained at the process level. We have noted this better in the conclusions and added some of the key results to the conclusions.

Specific comments: P. 20777, line 6: Many previous studies have examined how cloud microphysics may affect ACI (e.g., Menon et al., 2002; Rotstayn and Liu, 2005; Penner et al., 2006; Wang et a., 2012).

References added.

P. 20777, line 14: Ghan et al (2013) is highly relevant here

» Added reference.

P. 20777, lines 18-21: This statement is unclear. It is not clear to me how “the sensitivity of ACI to pre-industrial aerosols” indicates the second part of that statement.

» Clarified with some reorganization. “The cloud microphysical state, defined as the combination of cloud liquid water path and drop number, determines cloud microphysical (precipitation rates) and radiative properties. As a result, perturbations to this state from aerosols (ACI) may depend on the base state, i.e. the response of a cloud to a change in CCN may depend on the unperturbed CCN and resulting drop number.”

P. 20779, Eq. (1): any reference for Eq. (1)?

» Added reference (Zhang et al 2005).

P. 20778, Section 2.1: four off-line test cases. It is not clear why these four cases are chosen. Readers also need to refer back to Gettelman and Morrison (2015) to understand these four cases.

» Added a sentence explaining that these represent some basic idealized clouds commonly used to evaluate microphysical schemes.

P. 20784, line 21-28: Any explanation why the autoconverion changes have different effects in different cases?

» Clarified: auto-conversion matters in the cases with multiple updrafts where cloud coverage is most sensitive (W2 and W3), and it matters more for the oscillating (W2) than decaying (W3) updraft case. This is likely because with a limited updraft, the timing of precipitation matters.

P. 20786, Fig. 7: how are cloud top drop number and effective radius calculated? Is this for a particular cloud type, such as warm clouds?

» Yes, it is for liquid only. This is now clarified when Table 3 is introduced, and noted that it applies to the figures.

P. 20788, Section 4.4: Many previous studies examined the sensitivity of cloud lifetime

effects to autoconversion schemes (e.g., Menon et al., 2002; Rotstayn and Liu, 2005; Penner et al., 2006; Wang et a., 2012).

» Thanks for noting this. We have added a mention to the introduction as well as to this section.

P. 20792, line 8-11: The explanation here provides little help on why Berg0.1 produces a large increase in ACI compared to the default case.

» Added a sentence of explanation to clarify: Reducing vapor deposition in the mixed phase increases liquid over ice. Liquid has a longer lifetime (and hence larger average shortwave radiative effect), and liquid clouds are more readily effected by sulfate aerosols than ice clouds are (only homogeneous freezing is effected by sulfate).

P 20786, line 20: “in”! “an”

» Corrected.

P 20792, line 20: “can can” ! “can”

» Corrected.

Menon S, Del Genio AD, Koch D, Tselioudis G (2002) GCM simulations of the aerosol indirect effect: sensitivity to cloud parameterization and aerosol burden. J Atmos Sci 59:692–713.

Rotstayn, L. D., and Y. G. Liu (2005), A smaller global estimate of the second indirect aerosol effect, Geophys. Res. Lett., 32, L05708, doi:10.1029/2004GL021922.

Penner, J. E., J. Quaas, T. Storelvmo, T. Takemura, O. Boucher, H. Guo, A. Kirkevag, J. E. Kristjansson, and O. Seland (2006), Model intercomparison of indirect aerosol effects, Atmos. Chem. Phys., 6, 3391–3405, doi:10.5194/acp-6-3391-2006.

Wang M., S. Ghan, X. Liu, T. L'Ecuyer, K. Zhang, H. Morrison, M. Ovchinnikov, R. Easter, R. Marchand, D. Chand, Y. Qian, J. Penner, Constraining cloud lifetime effects of aerosols using A-Train Satellite observations. Geophys Res Lett 39, (2012)10.1029/2012GL052204).

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