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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative forcing in the ACCMIP historical and future climate simulations" *by* D. T. Shindell et al.

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We thank the reviewer for their constructive comments on our paper. Those comments are in italics below, while our replies follow in standard font.

Reviewer 1

This paper studies the radiative forcing in the ACCMIP historical and future simulations. This is very relevant and timely for the upcoming IPCC AR5 report. The paper contains a lot of information including the direct aerosol effect as radiative and adjusted forcing (AF) and the total aerosol AF both from pre-industrial to present-day as well as into the future following different RCPs. It also screens the models based on their ability to simulate the observed AOD and AOD trends and evaluates the relationship between

aerosol AF and equilibrium climate sensitivity. By combining all these aspects into one paper, it is overloaded and the main messages don't come out. It therefore does not make for an easy reading as it is way too condensed.

I suggest to split the paper into 2 or 3 papers and to have a clear and only one message in each of them. As long as it already is, I almost don't dare to ask for clarifications, as it would only increase the paper more. If the paper is not split in several parts it should be shorten it by at least 50

Rereading our paper again four months after submission, we agree that it was very long and not easy to get through, and we apologize for that. In our revisions, we have endeavored to maintain the comprehensiveness that specialists would like to have in order to follow the technical details of what was done in the comparison with observations and in the multi-model analyses while at the same tim improving the readability of the paper and clarifying the key messages. We have done this via several steps: (1) moving some of the very technical details that are not likely to interest most readers to appendicies, (2) removing the discussion of climate response (which we will submit as a separate paper), and (3) shortening the remaining text for clarity and conciseness. We do feel that although the paper is still long, having both the evaluation of model aerosols and the presentation of the aerosol forcing in the same manuscript makes for overall less repetition than two separate papers. The section headings allow readers to go to the analyses they are most interested in if they do not want to read the entire study, and we see no clear advantage to further separating the paper as the remaining material is all closely related.

Specific comments: Page 21113/21114: Focus less on individual models but more on the multi-model mean

The comparison with satellite AOD does focus almost exclusively on the multi-model mean. These pages delve into the difference between clear-sky and all-sky AOD, which is highly model dependent and thus discussion of the multi-mean is not useful. However, we agree that the extensive discussion of individual models here was distracting, and have moved this into a new Appendix on “Clear-sky versus All-sky AOD”.

Page 21129: Why does the CMIP5 subset of ACCMIP underestimate the negative aerosol RF?

This is largely due to the missing components, as adding in the missing components increases the negative forcing from -0.26 to -0.39 W m-2 in the ACCMIP models, and the negative forcing in the CMIP5 models is 0.11 W m-2 too small compared with our best estimate so is biased low by nearly the same magnitude. We've added in the text that the CMIP5 bias is “primarily owing to the lack of nitrate and/or SOA in several models”.

Page 21140: Did ACCMIP and CMPI5 use the same protocol?

The two projects had separate protocols, but in this particular case they performed simulations that were quite similar, which we now note in the text. There is a subtle difference in how aerosol changes between preindustrial and present-day were defined, however. The ACCMIP protocol included changes in both aerosol and ozone precursor emissions. The aerosol emissions obviously affect aerosols, but the ozone precursors could also chemically alter the aerosols although their impact on gases was explicitly excluded from affecting radiation. These simulations thus capture the aerosol forcing due to all anthropogenic short-lived species emissions. In CMIP5 simulations, aerosol concentrations from an all-forcing historical run were prescribed in the protocol, and thus aerosol fields include the response not only to short-lived species emissions but to climate change as well (e.g. changes in rainout due to CO2-induced warming). The ACCMIP protocol thus aims for attribution of AF to emissions while the CMIP5 protocol aims for attribution to concentrations. Without additional studies, it is not possible to unravel the influence of this difference, although we do not see any obvious systematic difference between the CMIP5 and ACCMIP AF results. As we discuss in section 5.1.1, the contribution of climate change to aerosol RF is not yet robust, suggesting that the

contribution to AF may similarly not be consistent across models. However, climate change in some models contributes a very substantial fraction of the historical RF, so may likewise be important to historical AF. Hence we believe that since attribution of forcing to emission changes provides a guide to the effect of future emissions changes it is likely to be more useful than attribution to concentration changes which can include a large response of aerosols to climate change induced by other agents which is more appropriately considered a feedback than a forcing.

Page 21141: Why is the relative std.dev. of AF substantially smaller than on RF over East Asia?

This is more closely related to the standard deviation of the RF being very large in this region than to the pattern of the AF, which is large in East Asia but is as large in many other areas as well. The large standard deviation in the East Asian RF seems to be a result of the differing impact of BC relative to scattering aerosols in the models. Figure 12 shows that the largest standard deviation in BC forcing is in East Asia, and that sulfate, nitrate and OA all have substantial standard deviation in that area (though those are of the same sign). This leads to net forcing that can vary substantially in this region even if individual components are not so different, with the end result that the local RF is more variable across models than the AF.

Page 21142: Shorten by referring to Lohmann et al., 2010 where all these estimates are included.

Agreed, we've referenced the Lohmann et al paper and shortened the text here.

Pages 21144-46: Partitioning of the AF is based on one model only. That's too speculative and does not belong in an intercomparison study. Please delete it.

We agree that there are too few results to warrant an extensive discussion, but do feel that this is an important issue and that the community should be aware of how poorly the aerosol indirect effect can be attributed to individual components. We have revised to explain how this is especially important to understanding the impact of future emissions pathways or policies. We have also obtained results from a second model, and discovered two references in the literature with somewhat comparable results. We now have a single paragraph on this topic (and no Table) discussing these four modeling results and highlighting how uncertain but important this area is. The interested reader is referred to the Appendicies for details on the modeling used and the results formerly in Table 8 (now with MIROC-CHEM results added to the table).

Page 21152: Why is ECS not correlated with aerosol AF?

The earlier study of Kiehl showed that in a previous generation of models, aerosol forcing and climate sensitivity were correlated, which suggested that modeling teams had perhaps kept adjusting their aerosol forcings within the large uncertainty range until they arrived at a value which allowed their model to reproduce historical climate change given their model's climate sensitivity. Our goal here was to see if that was again the case in this generation of models. The answer is no. We did not set out to determine why ECS shows the spread that it does, but clearly this will be influenced by processes such as cloud feedbacks. Further examination of this issue reveals that in contrast to the earlier generation of models, many of the CMIP5 simulations do not capture historical changes well. This suggests that the more sophisticated representations of aerosol-cloud interactions in the newer models, which tend to tie process representations to physical understanding of particular aspects of particle-cloud interactions, are perhaps not as easily adjusted as the cruder parameterizations in earlier models, so that groups utilize their native emergent AF whatever their ECS happens to be. We have added some discussion of this in the text as an answer to the question of the reviewer and an additional figure panel showing the historical temperature changes in the CMIP5 models and their relationship to the imposed forcing.

Page 21153: Why does ANWF not closely track aerosol AF+ozone RF in some models?

It does track closely, but the response to ANWF does not always. This suggests that the feedbacks in response to these inhomogeneous forcing differ across the models. This section of the paper has been deleted however, and will be reexamined at greater length in a separate paper.

Pages 21156-21158: Get rid of autocorrelations

This discussion has been deleted.

Fig 3/4: Show only one of them

Figure 3 makes the point that global AOD may be similar across models but the relative importance of the different components varies markedly. Figure 4 makes a quite different point by examining regional biases in the total AOD. It shows that virtually all models have large negative biases over East Asia, and that the only two that do not greatly underpredict AOD in East Asia have large positive biases over Europe and N America (where the other models match observations fairly well). This suggests those two may be getting realistic East Asian AOD values due to systematic high biases. Hence we believe these figures both make important points, and are quite distinct.

Fig 5/6: Replace by model-mean

We agree these two figures presented more individual model results than most readers might want to see. We have removed Figure 5 (and placed it in the Appendicies) and shortened the text on regional biases. Figure 6, however, we believe makes an important point about how the spatial information can be used to evaluate individual components of the AOD, which is a key step in unraveling the disparity in the relative contributions of different aerosols to the observed total (and an area reviewer 2 asked us to expand). We believe this figure is important to illustrate how the model/AeroNet differences in some areas can readily be attributed to specific aerosol components, setting the stage for the quantitative screening by mass loading we perform thereafter. We have improved the figure, however (see reply to next comment).

Fig 6: Red colors (sulfate vs. model) are not good to distinguish

We agree these were hard to distinguish. To improve the readability of the figure, we have made the lines showing total AOD from AeroNet and from the models much thicker than the lines showing AOD from individual components, which makes them much easier to distinguish.

Fig 8/27: Delete

We agree that these two figures can be deleted. Figure 8 illustrates a similar conclusion to Table 7, so is not critical to the AAOD evaluation (we have moved that figure to the Appendicies). Figure 27 showed the autocorrelations, which we have removed.

Fig 25: Eliminate NCAR-CAM3.5 if it doesn't include indirect effects

This figure has been deleted.

Fig 26: Replace by model-mean

This figure has been deleted.

Typos: Page 21109, line 1: response –> responses Page 21111, line 1: amount radiative –> amount of radiative Page 21114, line 6: all-sky –> all-sky values Page 21137, in this regions –> in these regions

All typos corrected, thank you.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 21105, 2012.