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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.

Anonymous Referee #2

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The manuscript contains several analysis threads that provide an important basis for interpreting the simulations conducted in support of the IPCC AR5 report currently in preparation. The authors quantify the range of aerosol, both direct and indirect, defined as both radiative and adjusted forcings, and compare these to well-mixed greenhouse gas forcing agents. They further conduct a fairly thorough evaluation of the aerosol distributions in the models, and go to some length to disentangle the contributions from the individual aerosol species within each model and in some cases can meaningfully conclude that certain models are outside the range determined from observations. The examination of the relationship between equilibrium climate sensitivity and aerosol forcing in individual models is also important and the finding that the perhaps fortuitous compensation between these factors in the previous generation of IPCC models does not hold for the adjusted forcing should be of broad interest.

Unfortunately, the paper tries to accomplish all these related goals at the expense of distilling the key messages. I share the first referee's opinion that the paper may be more effective as two papers. For example, one paper could delve into the aerosol model evaluation and the details of the individual models including the major factors leading to the diversity across models. The second paper could focus largely on the multi-model forcing results. Cleanly splitting these, however, will require thought since they are clearly linked through the model screening analysis which leads to new "best estimates" of the forcing though the strengths and caveats of these constraints should be clearly communicated in the abstract and conclusions. Another option is to make use of supplemental documents for much of the information on the individual models linked to the main text.

For either option, the text must be dramatically reduced including in any new supplemental sections. There are sections where discussion borders on rambling and better organization would eliminate the need for repetition. Most importantly, what are the authors' overall objectives of this manuscript? The paper should be re-written with focus on those.

Specific comments

Abstract, and elsewhere. Some discussion is needed as to the strength of the observational constraints. How well do we know AOD changes from 1980 to 2000 (cf Section 4)? Do the spatial patterns of AOD mirror those of the aerosol emissions? How similar are the aerosol pathways in the RCPs? There seems to be a blurring of forcing and feedback, since the "adjustments" are referred to as stronger forcing than RF at bottom of p. 21107. The ANWF discussion is important but awkward phrasing makes the discussion confusing. Abstract needs to be shortened dramatically to focus on key messages.

The authors seem to assume readers are well-versed in ACCMIP versus CMIP5 and also the emission changes under the RCPs. A brief paragraph describing the differ-

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ences, and a figure or table or at least one line in the text reporting percentage changes of the major aerosol precursors under the scenarios seems appropriate.

Does BC act as CCN in all of these models or are only direct and semi-direct represented? Are there aerosol indirect effects in mixed phase clouds or warm clouds only? It would help to know which models include a consistent set of processes. What do the models include for SOA chemistry? Uncertainties abound, ranging from the emissions themselves to the chemistry, SOA burden, and optical properties and this context should be made clear.

Intro. First paragraph should include some references and numbers for prior estimates to better set the context of new findings. This applies throughout text where there are many general statements. The ACCMIP/CMIP5 discussion is confusing for those not immersed in these activities.

Section 3.1. Why not focus this by starting off where the two instruments agree to within some level and then evaluate the models?

Section 3.2. Separate evaluation from the beginning into spatial and seasonal components. Is the point that the models show systematic biases spatially? The use of regional evaluation to place constraints on the dominant component is very useful and deserves more emphasis. What is the implication for the forcing estimates here if the models are most biased where observed AAOD is largest?

Section 5. BC surface albedo forcing methods belong in Section 2. This section needs major rewriting as it delves into individual models and differences between them but also presents the RFs/AFs. Some comparison of the results with Aerocom as well as the values and ranges in IPCC AR4 Chapter 2 would be appropriate. The application of SOA and nitrate to all models, with exclusion or weighting of individual models needs better justification since it appears somewhat arbitrary. The differences in mechanistic treatments in different models should be more clearly explained and their implications for RF, e.g. p. 21129-30.

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Title of 5.1.2 is awkward since preindustrial to present-day was already considered. If BC albedo is covered elsewhere, it can be limited here to a few sentences at most. Section 5.1.3 can be shortened and woven into the discussion of specific time periods.

Section 5.2. Are the AFs additive? Would the same answer be obtained by using present-day climate and greenhouse gases but 1850 emissions? How robust are the dynamics changes in the models? Do regional short-wave flux changes agree with observations? Discussion is difficult to follow in some places here particularly when jumping from one time period to another. Is the finding that AF is sensitive to background loading built into the model parameterizations of aerosol indirect effects?

Section 6. Clearly the paper has a heavy aerosol focus compared to other forcings and this should perhaps be reflected in the title.

Section 7. Where is TCR in Table 11? The autocorrelation discussion is difficult to follow.

Section 8. The conclusion that sulfate has a stronger impact on clouds than BC is interesting but is it fundamentally built into the model parameterizations?

Several of the tables and figures have overlapping information such as Figures 4 & 5, Table 6 & Figure 10.

Table 4. From the text, these are for different regions in each model. It would be informative to include a column in the table giving the region.

Table 10 and elsewhere. Best to specify if RF or AF is always relative to preindustrial. It seems arbitrary to double uncertainty to use RCP8.5 estimates for the other RCP scenarios rather than only report what is actually calculated with the models. While it's tempting to argue that if AOD itself evolves similarly in all scenarios this approach should be reasonable, one of the conclusions from the paper suggests that AOD is not a reasonable proxy for aerosol AF.

Figure 6. How was the averaging across sites done? Here and elsewhere, the mixing

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of clear-sky and all-sky complicates interpretation of biases and assessment of the models particularly since a conclusion is there is no obvious relationship between a model's cloudy and clear-sky estimates.

Figure 20 and elsewhere. Are the trend and changing spatial patterns meaningful given the small numbers and varying sample sizes with each time slice? It would be more convincing to show this for individual models. Why not plot AF and RF together for a straightforward comparison?

Figure 23 should give the number of models included in each period.

Figure 27 and accompanying text. The methodology here is not clear, nor what is learned.

Several figures are nearly illegible. Are all of the simulations in Figure 11 transient? If not, symbols should be used to more cleanly distinguish the time slice from transient.

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