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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 2

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.

We agree that we tried to include too much in a single paper, and have decided to split the paper into two parts. We have removed the discussion of the response to the aerosol and ozone forcings from the paper and will create a new paper from the material. We have also attempted to reduce the remaining text by moving some of the very technical details that are not likely to interest most readers to appendicies, shortening many other sections, and merging some sections to reduce repetition.

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.

We do discuss the AOD trends from observations, but a detailed analysis of the strengths and weaknesses of the various AOD analyses is beyond the scope of this paper and is contained in the references cited in our discussion of the satellite-based AOD trends, especially Li et al (2009) which discusses these issues extensively. The spatial patterns of direct forcings follow the distribution of the aerosol or aerosol precursor emissions fairly closely in most cases, but of course there is transport within the atmosphere so the RF extends well beyond source regions. AF shows a weaker correspondence with emissions. We could add more figures and discussion of this issue, but as determining the spatial relationship between emissions and forcing is not a main objective of our analysis that would not fit with the goal of focusing the paper more closely on the main objectives.

Aerosol pathways are fairly similar in the four RCPs, as shown in the ACCMIP overview paper of Lamarque et al (GMDD, 2012). We have added a line about this.

The fixed-SST 'adjusted forcing' purposely includes rapid adjustments to obtain what has been shown to be a more useful metric of the radiative perturbation that drives the eventual climate response (e.g. Hansen et al., 2005; Lohmann et al., 2010). The AF includes some processes that would have formerly been called feedbacks, but the AF concept is clearly defined. The strong adjustments for aerosols include the cloud responses, of which the cloud-albedo effect was also stronger than the direct RF in the AR4 (though it was called an RF there even though it is clearly a cloud adjustment to

the aerosols).

The ANWF discussion has been deleted, while the abstract has been substantially shortened.

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 differences, 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.

We have added additional description of CMIP5's goals in the Introduction, where ACCMIP is already motivated. In section 2, we added two sentences describing the RCP aerosol-related emissions (further details are in the Lamarque et al. GMDD ACCMIP overview paper).

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.

BC can act as CCN in most models, but not in GFDL-AM3 or HadGEM2, or in NCAR-CAM3.5 and CICERO-OsloCTM2 which do not include indirect effects. We will add a summary of which aerosols affect CCN and IN in the models, and more generally the inclusion of ice/mixed phase clouds in the aerosol-cloud interactions.

Representations of SOA and of non-methane hydrocarbons in general vary widely across the models, as described in the ACCMIP overview paper of Lamarque et al. (2012). As SOA forcing is typically very small, we do not believe a great deal of discussion on how models treat SOA is warranted but will add text commenting on the model-to-model differences.

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.

We've added forcing values from AR4, and additional explanation of the goals of CMIP5 and the relationship between ACCMIP and CMIP5 models.

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

While that could be a reasonable way to approach the analysis, in some cases we believe this would not be as useful as our current method. For example, MODIS does not report values over the high-albedo desert areas, but MISR does, and the latter are thought to be fairly high quality and the models are in reasonable agreement with those. Over South America, MODIS and MISR show substantial differences, but both instruments report larger AOD values than the models, so that despite the disagreement the analysis suggests a low bias in the models there. Neither of these areas would be included in our evaluation if we demanded the two instruments to be in fairly close agreement.

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?

We have followed the suggestion to separate the discussion into seasonal and spatial components. The spatial biases are much larger and varied across the models, so we devote the bulk of our attention to those. As suggested, we have expanded the discussion of how the regional pattern in the zonal mean observations can or cannot be used to constrain individual components.

The comment on AAOD refers to text that was in section 3.3 rather than 3.2 (that text is now in the appendicies). The underestimate being largest in high AAOD regions suggests that the underestimate in RF might be relatively smaller than the AAOD estimate as there could be some non-linearity at high AAOD values. Such an effect seems likely to be small as the AAOD attenuation is much less than 1 in most locations, but further work to assess the impact of the BC/AAOD biases on RF is clearly required to understand how linear this relationship is.

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.

We agree that the long description of the methods for calculating BC albedo forcing was distracting here. We have moved most of that material to the Appendicies, giving only a brief description here that is now combined in a separate paragraph with the brief descriptions of the methods used for calculating aerosol direct and indirect forcings. We feel that putting these in section 2 would have them so far from where they are first used in section 5 that it would be difficult for readers to remember them.

We have extensively revised parts of section 5. For example, the screening of modeled global mean preindustrial to present-day forcing based on evaluation against satellite observations formerly in section 5.1.3 has been merged into 5.1.1 on global mean preindustrial to present-day forcing. We have also added in 5.1.1 comparison with the AR4 assessment, which includes the results from AeroCom models as well as other aerosol modeling results and estimates based in part on satellite observations.

We have clarified the rationale for exclusion of results for SOA and nitrate for atypical models.

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.

We have changed the title to more clearly reflect that this section is concerned with the temporal evolution of global mean forcing (as opposed to the 'snapshot' present vs preindustrial). As suggested, we have substantially reduced the material on BC albedo forcing.

Section 5.1.3 has been shortened and merged into section 5.1.1 as suggested.

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?

There is only a single AF provided at any time other than the last paragraph where AF by component is discussed, so additivity doesn't seem relevant to our discussion. It is a good question as to whether the same answer would be obtained for AF decreasing emissions to 1850 under present-day conditions as opposed to increasing emissions to 2000 under 1850 conditions, but as those simulations were not run we cannot answer this. Any dynamic changes in the model would have minimal influence on AF as the runs had constant climate (via fixed SSTs), so they would only impact the RF calculations (which are from runs including climate change, as discussed in the paper). As such, they are not of major importance to the analyses presented here, so we do not go into those, but there are plans to evaluate those as part of the various ACCMIP analyses.

We looked at data on solar insolation, but found that the long-term measurements were very limited in coverage and that very specific diagnostics that were not part of the ACCMIP output would be required for a proper comparison, so we cannot compare dimming and brightening trends with measurements.

We have tried to clarify the discussion.

The models include aerosol impacts on clouds that depend non-linearly on the aerosol loading, so the representation of these physical processes in the models does make them sensitive to the background loading.

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

This is a reasonable suggestion, but if we say "Aerosol radiative forcing" rather than "Radiative forcing" it will not be clear that anything other than aerosols is included, and adding something like 'especially by aerosols' is awkward. Thus we have kept the original title.

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

Table 11 and the discussion of autocorrelations have been deleted.

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?

No, and in fact in the revised discussion of this, which has expanded the speciated AF results, we see that different models produce fairly different relative impacts of the various components.

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

Figure 5 has been removed. Table 6 and Figure 10 do indeed share some information, but these are such key results from this analysis that we felt it important to include both as the table provides the quantitative values for each model that others may refer to while the figure allows the mean and variance of the relative impacts of the various terms to be more easily discerned.

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.

This analysis does indeed cover different areas in the various models, but they are not contiguous areas but many locations across the globe so they cannot readily be listed in the table.

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.

The first sentence in the table caption says exactly what the reviewer suggests, that forcings are relative to 1850. It is indeed somewhat arbitrary to double the AF uncertainty, but the main point here is to indicate that since the RCP aerosol emissions decrease very strongly by 2100 in all the RCPs (and especially for SO2 and BC they are very similar), the bulk of the present-day AF is virtually certain to go away under any of the RCPs. Hence although we did not perform these experiments, partially because the aerosol emissions are in fact quite similar at 2100, the doubled uncertainty reflects that the precise 2100 values are less certain although we are confident in the qualitative similarity across RCPs.

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

The model values at AeroNet locations were compared with the observations, then the zonal mean taken. Indeed there is some confusion about all-sky versus clear-sky outputs, but as we discuss in many models the differences are small. In principle we have aimed to use clear-sky in our comparisons whenever available, but in practice few models output separate clear-sky and all-sky AOD.

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?

In most cases we believe both the trends and variation in spatial pattern are meaningful because they both follow the underlying emissions trends to a large extent and they are consistent across the models that are available at multiple times. Figure 19 provides and indication of the variance across models in a much more concise way that showing each of the 8 individual models with 2000 vs 1850 AF and RF would (and is in keeping with the suggestion of reviewer 1 to emphasize the multi-model means and not each individual model). As AF includes RF, we do not believe it is the best use of space to show these together for all times, but we note they are presented side-by-side for 2000 vs 1850 in Figure 17.

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

Added.

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

This Figure and accompanying text have been deleted.

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.

We have improved several figures. In Figure 11 we show only results at the time points indicated by the symbols, which has been clarified in the caption.

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