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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The net climate impact of coal-fired power plant emissions" *by* D. T. Shindell and G. Faluvegi

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We thank the reviewer for their constructive comments and suggestions, especially the suggestions of where more explanation was needed to clarify what was done. We have taken into account those suggestions, as well as those of reviewer 2, and believe the paper is now substantially clearer and the results easier to follow and presented in a more useful way.

P 21259, L 14-15 – Agreed. Revised as suggested.

P 21259, L 18-20 – Agreed. We revised this discussion and added several references, as suggested.

P 21259, L 27 – We have substantially revised the last paragraph of the introduction

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to put the work into context with prior studies more clearly, including the references mentioned by the reviewer here, and to better clarify what's new in this study.

P 21260, L 8 – We find it difficult to keep track of letters or numbers for scenarios, so have followed the spirit of this suggestions but instead use short names that are easy to remember for the scenarios (Table 1 and text).

We have clarified the assumptions for NOx and SO2 emissions in the air pollution control scenarios, and note that very little BC is emitted by power plants.

We also thank the reviewer for pointing us toward the Hayhoe et al paper, which was indeed quite relevant and is now cited in our study.

P 21261, L 7 – We've changed the wording here to match the table (and cited the table as well) for clarity, as suggested.

P 21261, L 12 – We've clarified that pre-2000 emissions are not included in any of the future scenarios, removing the word 'historic' and rephrasing that sentence. We also have added the impact of current emissions so that those can be summed along with any future scenario if desired. We discuss multiple time frames in the text, but try to make it clear that it is useful to consider emissions at various time depending on whether one is interested in the total effect of coal plants through time in the past (and of existing plants in the future) or the marginal effect of adding new plants.

We have added a sentence stating: In all scenarios, no emissions changes other than these changes in power generation-related emissions are included as we wish to examine the RF due to these emissions alone. (end of section 2).

P 21262, L 21-22 – We agree that this was an odd way of describing this. We now instead point out that: Aerosol indirect effects (AIE) are highly uncertain (Penner et al., 2006; Forster et al., 2007) and hence not robustly characterized using a single model. Then we discuss how we include an estimate of the AIE and its uncertainty, as suggested. In the revised version of the paper, AIE and its uncertainty is included for

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all scenarios, not just one example as in the original ACPD paper.

P 21263, L 4-7 – Added suggested discussion of the simplified CO2 model.

P 21263, L 14 – Revised text as suggested and cited the Wild et al paper.

P 21263, L 19 – This explanation was given a few lines later (L 25-27), so we have left this as is in the revision.

P 21263, L 20 – Revised as suggested stressing Figure 1 shows constant emissions and is used as a reference (reference cases now that there are two sets of constant emissions in Figure 1).

P 21263, L 20 – As suggested, we now show the components of RF in Figure 1. We agree that this provides quite useful information, and thank the reviewer for this valuable suggestion.

Figure 1 – We have clarified in the text and caption that all RF values are relative to 2000, that background is fixed to isolate the effects of changes in coal-fired power plant emissions, and that for the constant emissions cases forcing is calculated for emissions from 2000 onward only.

P 21264, L 8-9 – Revised as suggested.

P 21264, L 18 – We've clarified this point, though we note that we did explicitly point out that temperature changes would be different a few lines later.

Figure 2 – We've added to the caption that all values are relative to the year 2000, not to a control run. As this figure shows forcing due to additional plants, the appropriate control would be no additional plants and hence zero forcing. The reference run of constant current emissions is not a control, but a reference to allow comparison of the effects of new plants with those of existing plants and to see how forcing from a particular plant changes with time.

P 21265, L 7-27 – Thank you. We've tried to highlight these points about the regional

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structure more clearly throughout the paper.

Figure 3 – We now note in the caption that: All forcing calculations were done internally in the GISS climate model other than the aerosol indirect effects which were estimated based on the sulfate forcing as described in the text.

We thank the reviewer for pointing out that it was worth considering the difference between instantaneous and adjusted forcing. While there is no real difference for aerosols, adjusted is a better indicator of climate response for CO2. As we had calculated both instantaneous and adjusted values for CO2, we now use adjusted forcing everywhere in the paper.

We have added discussion of the actual RF values to the text instead of only mentioning them in the figure caption. Values are now presented for both 2040, when aerosol forcing maximizes, and 2046, when the global mean net forcing is zero.

The reviewer suggested adding a table with normalized RF values by burden change in order to allow comparison with other models. We point out instead here that the radiative forcing per unit burden change in this model has been discussed and compared with other models previously (Shindell et al., 2008b; Schulz et al., 2006), and add the suggested quantitative comparison with other models in section 6 (2nd to last paragraph) in the context of uncertainty in our results.

P 21265, L 24-26 – We've added discussion of how this work related to the 2003 Boer and Yu paper as suggested.

P 21266, L 1 – We agree that the order of the sections would be better if 5 and 6 were switched, and have done so in the revisions.

P 21266, L 4 – Added suggested caveat.

Section 5 (now section 6) – We've added a paragraph on the uncertainty associated with the models, including the suggested comment about resolution and model components.

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P 21267, L 10-12 – We're revised to state this explicitly, as suggested.

We agree that this discussion was not exactly an uncertainty, but felt it was important to include here so have revised the section title to be: Uncertainties and Limitations.

P 21267, L 21 – We had said g/kg fuel, but not everywhere. We've now clarified that, and have also clarified in the text and tables the exact units for NOx , SO2, etc.

As in the comment above (on RF per burden change), we've added a discussion in this section that describes uncertainties in emissions to concentration to burden, with ranges given from and comparisons to the literature values.

P 21268, L 14-27 – We initially had thought to be brief in explaining the response calculation so as not to distract from the RF results, but we agree that its important to describe our methodology better. As suggested, we've therefore added the equations used in the response calculation, including a description of what the lag in temperature response represents (different timescales of ocean response) and how it is calculated. In preparing the revision, we discovered that very similar results for the climate response had already been presented in the literature. As they yielded fairly consistent results with those from our model, we decided to simply use the already established technique/results so that we could focus our attention on the extension that we have made to regional scales. We added a table (Table 3) showing the coefficients of regional response per unit forcing that we used for that extension, so with the equations presented and the values in the table the methodology should be fully reproducible. We hope it may prove useful to others.

We also added an explanation of why we did not want to use one of the standard energy balance one, two or four box models as one of the most important aspects of our results was the spatial pattern of RF (and a model such as MAGICC has one land and one ocean box in each hemisphere, so cannot provide the regional response to regional forcing we wanted to investigate). We also revised the response calculations to include AIE in all cases, in accord with the RF calculations, and now state this explicitly.

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We shortened the explanation for why we did not perform full GCM calculations as suggested.

As suggested, we added text discussing that surface temperature was only one aspect of climate response, and referencing relevant studies on the response of precipitation to inhomogeneous forcing at the end of section 6.

Figure 5 – As we now explain in the description of the equations used to calculate response, the relationships between regional forcing and both local and non-local responses were taken from Shindell and Faluvegi, Nature Geoscience, 2009. That work did not examine Southern Hemisphere mid-latitudes and high-latitudes separately (as there are no particularly large regional forcings from tropospheric aerosols or ozone to separate out there), so that the responses can only be calculated for the regions shown. We have, however, added Arctic temperatures so that the reader could compare the two entire extratropical regions if they wanted to (with the appropriate area-weighting, meaning that the NH extratropical mean is roughly 3/4*NHml+1/4*Arctic).

P 21269, L 10 – We added the suggested explanation of why climate sensitivity was greater in some regions.

P 21269, L 16 – Reference added to a recent paper on the importance of rate of change, as suggested.

P 21269, L 19 – We've refined this discussion and clarified that the first papagraphs in this section were simply qualitative descriptions, with historical calculations for 1940-2000 defined and explicitly presented in just the one paragraph.

P 21270 – While the temperature responses are only rough estimates, we believe that is appropriate given that the scenarios are only illustrative (with real emissions not predictable) and that the temperature response are important to present since RF is on its own not terribly informative to most people. In the revised text, following the reviewer's previous suggestion, this is not an add-on at the end of the paper but is

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within the main results section before the discussion of uncertainties and limitations and the conclusions. Also in accord with the earlier suggestions, the methodology is now presented in considerable detail so that it is now much clearer how the temperature responses were derived and how these results, which we agree are in principle more relevant, should be interpreted.

P 21270, L 1-5: We addressed earlier in the revisions that AIE is always included in the temperature response calculations.

P 21272, L 1-25 (accidentally given as p 21270 in review) – This seems to us a useful way to present this, so we've followed this suggestion and added this reference and phrasing.

P 21272, L 1-3 – Revised to refer purely to physical science here (. . .does not imply that warming will not eventually take place).

P 21272, L 8-14 – We've added a similar reference to Charlson et al., 1992 in the Introduction following the suggestion of the other reviewer to put this work into context with the known aerosol offset of CO2 forcing.

The suggestion to discuss the limitations of using a global mean metric was a good one in our opinion, and we've added a paragraph on this in section 7 (3rd paragraph).

P 21273 – As suggested, we've rearranged the paragraphs to now mention the additional impacts not fully explored in this paper at the beginning of section 7, and then close with the results from our study as these are the primary conclusions we obtained.

All suggested technical corrections made.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 21257, 2009.