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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climate forcing and air quality change due to regional emissions reductions by economic sector" *by* D. Shindell et al.

D. Shindell et al.

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We thank the reviewer for their constructive suggestions for revisions to our paper. We felt that these suggestions were appropriate and useful for improving the clarity of the presentation, and have revised the paper to take them into account. We realize that thoughtful reviewing takes a substantial amount of time and effort, and would like to emphasize that this is greatly appreciated. A detailed response to each comment is below.

Specific comments:

In section 2, we have added text to clarify that both the models used coupled chemistry and dynamics rather than the composition simulations being driven by offline meteoro-

logical fields in either case. Hence the setup is comparable, and should not be a source of differences between the results of the two models. Meteorological differences will exist of course in the two GCMs, but we would not be able to isolate the effects of those differences from other processes (e.g. chemistry, wet processes, numerical transport schemes) without additional studies such as runs using passive tracers subject only to transport.

We have clarified that the meteorology is intended to represent general present-day conditions rather than any particular years (last line of section 3).

We agree that the use of identical anthropogenic emissions in the two models should be clearly stated earlier, and have revised the first paragraph of section 3 accordingly, also adding that distributions were not changed. The reviewer also notes that assumption of uniform reductions across all species is not necessarily reflective of polices that might target a specific pollutant. We fully agree, and discussed this issue ourselves in the last paragraph of section 1 and in the second-to-last paragraph of section 6.

We have added all the suggested clarifications on using multiple years to average out meteorological variability and holding emissions constant during the 11 year runs.

Following the suggestion of the reviewer, we have examined the changes in OH and H2O2 in the models in addition to the ozone changes already discussed in the paper. The first paragraph of section 4.2 now includes quantification of these changes, though they were so small that it seemed most useful to quantify the range (e.g. <1%) rather than give exact values for each oxidant in each sector perturbation experiment. We believe the oxidant changes played only a small role in the sulfate burden changes for the industry/power sector runs (where the sulfate burden changes were substantial), which instead were driven primarily by SO2 emissions changes. We do note that the only case in which OH levels increase was the Developing Asia domestic sector runs, and that OH increased in both models. The sulfate burden decreased, however, indicating that in this case as well it was responding primarily to SO2 emissions changes.

ACPD

8, S7626–S7629, 2008

Interactive Comment

The base case distributions of pollutants have been published previously. We had discussed this in the last paragraph of section 2, and referred the reader to the model references given earlier in the model description. To make this clearer, we now list those references again in the discussion of the base case distributions so that those fields and the comparisons of pollutants with observations can be more easily looked up if additional information beyond the summary given in this paper is needed.

We have added to section 4.2 a description of the AOD calculation, explaining that both models perform Mie scattering calculations and have prescribed dry particle sizes but include uptake of water on aerosol surfaces. We also provide references for further details. We have added a description to section 4.3 of how the radiative forcing calculation is done and that it is an instantaneous tropopause calculation (this was noted in the figure captions, but not in the text in the ACPD paper).

As suggested, we have clarified that the methane calculation is an offline, steady-state calculation, and we have added the TAR-recommended feedback factor used.

Section 5: We tried several ways of presenting the RF calculations, and decided that in the end it was easiest to follow if we left the rather distinct discussion of offline estimates of RF from long-lived species separate from the long discussion of the short-lived species changes and their RF. However, we provide a guide to the reader in section 3 at the start of the discussion of short-lived species noting that we will consider long-lived species in section 5. Forcing at the 20-year time horizon is the instantaneous forcing at that time rather than integrated forcing, as is conventional (e.g. IPCC TAR and AR4; integrated values are typically evaluated using GWP rather than forcing). The time horizon for RF is only important for long-lived species. For ozone and aerosols, lifetimes are so short that forcing is independent of time horizon when considering years (it would matter only for months or less, which is not terribly relevant for climate, or be important for the response to pulse changes in emissions rather than sustained changes as in this work). For methane, the residence time is 10 years, so the bulk of the adjustment will take place within 20 years, and a steady-state assumption for

ACPD

8, S7626–S7629, 2008

methane should only introduce a small bias in the RF at that time.

Section 6: The reviewer points out several things in the discussion that might be better to include earlier. We agree, and have added discussion of oxidation changes to section 4.2, added that identical emission inventories were used in section 3, and moved the references to earlier sector studies from section 6 to the Introduction (section 1).

Technical comments:

We revised this text to state that the chemical and physical processes represented in the models were generally similar (rather than the generic description: interactions), and then discuss those that differed in more detail.

We changed this from local changes to instead refer to regional changes, for which Figure 1 shows that CAM shows regional ozone decreases in SE and East Asia, though these are not large.

We corrected the reference style in these cases.

We clarified that we meant the former here (similarity in AOD suggests similarity in RF).

We added that these were annual averages to the figure captions, and are glad the reviewer noticed this oversight.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 11609, 2008.