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Interactive Comment

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

Anonymous Referee #1

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This paper presents results of two models, in which emissions from different sectors in North America and East Asia are reduced in order to evaluate changes in surface air quality and radiative climate forcing. Because the effects of changes in emissions from a single sector on multiple forcing agents have not received great attention in the past, this paper is a unique and relevant contribution to the literature. However, I find that there are a few too many places in the paper where the methods used are unclear. Therefore, I ask that the authors give attention to the issues raised below, most of which are intended to clarify the work that has been completed.

Specific Comments

In Section 2, there is little discussion of the meteorology. For the GISS model, I infer that the authors use meteorological inputs from ModelE that are archived and therefore external to the chemistry model. In contrast, I infer that the CAM model is a model of atmospheric dynamics with coupled chemistry. The authors should clarify if this is correct, and if so, then the differences between the models (one with offline meteorology, one with coupled chemistry-meteorology) should be discussed further as a possible difference between the model results. Are the differences between the models, or differences in the meteorology in the models, possible causes of differences in the results?

Is the meteorology intended to be for a specific year or period of years? Or just representative of present-day conditions?

The authors clarify that the emissions are identical in the different models in the conclusions, but that should be clear at the beginning of Section 3. Also clarify that all emissions from different source categories are reduced uniformly by 30% (that is, for all species, and keeping the same temporal and spatial profiles). Note that doing this is not necessarily a good reflection of the effects of policies that can target one pollutant or another (for example, scrubbers to remove SO2 from power plants), which is one of the authors' motivations.

Why is the model run for 11 years? It's not clear to me that this is better than just running for a few years, other than to avoid meteorological variability. The statement about 8 years (p. 11615, line 14) is a little vague (maybe clarify that it is "negligible differences from the 10-year averages"). Also clarify that the emissions are held constant over the 11 years of simulation (that you're not trying to model changes in emissions through time).

Can Section 4 give more discussion (with quantification) about the changes in oxidants causing changes in sulfate?

I think it would be appropriate to show the base case global distributions of pollutants

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(SO4, BC, O3) in the two models, before changes in emissions are conducted. Or refer to another paper if these have been published previously.

Section 4.2, 4.3 - How is AOD and RF calculated? Can the authors refer to other papers where these methods are used? For AOD, are the same methods used for the two models? Is RF the instantaneous or stratospherically-adjusted RF?

p. 11621, lines 6-23 - Clarify that the change in methane concentration is the steady-state change, which is calculated offline using the changes in oxidizing agents in the model. Report the feedback factor that is used.

Section 5 - Should some of this discussion appear earlier when presenting RF results for the first time? Also, is it the 20-year GWP (integrated over 20 years) that is used, or the forcing from the fraction remaining in 20 years? Is the 20-year horizon only used for CO2, or also for CH4 or something else? For CH4, you are calculating the steady-state change which will take ˜30 years to achieve. Can you discuss this in light of the 20 years for CO2?

Section 6 includes some things that I think could have been discussed earlier in the paper for greater clarity. This includes p. 11626, line 9-10 (the results section mentions changes in oxidants, but more details are given here), p. 11627, line 10, and p. 11628, lines 22-24.

Technical Comments

p. 11614, line 1 - "Interactions are similar..." is vague and should be clarified.

p. 11615, line 18 - It doesn't look like O3 in CAM decreases locally.

I believe some references are referred to incorrectly: "scavenging of aerosols in (Koch et al., 2007)" should instead be "scavenging of aerosols in Koch et al. (2007)." This occurs several times in the paper.

p. 11623, line 6 - Are you just saying that the AOD is similar, so that the RF should be

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similar? Or are you referring to other model results (from previous papers)?

Fig. 1 caption - are these annual average concentrations?

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

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