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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effect of regional precursor emission controls on long-range ozone transport – Part 2: steady-state changes in ozone air quality and impacts on human mortality" *by* J. J. West et al.

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Response to Anonymous Referee 1.

We thank Referee 1 for his/her attention and thoughtful comments on the manuscript. Referee 1 made only minor changes and suggestions on the manuscript, which we respond to below. (Referee comments are in italics.)

p.7083 - a brief summary of the origin of the emissions data would be valuable here.

We agree that our description of the emissions is short. In this case, we decided to add

detail on the origin of our emissions estimates to the companion paper, so that this is clear in that paper as well. From the methods section of the part 1 paper, the sentence reads:

"Emissions for the early 1990s were compiled by Horowitz et al. (2003) based on several global emissions sources, including EDGAR2.0 (Olivier et al., 1996)."

While Horowitz et al. (2003) compile emissions from several sources, we now add the reference to EDGAR2.0 as the single most important source of emissions information. With this added detail in the part 1 paper, we didn't feel that additional detail would be necessary in part 2.

p.7089 - the ozone-mortality relationship is assumed to be linear above a given threshold, and is based on 24-hour average ozone. How sensitive are the results likely to be to differences between the diurnal variation in ozone in the model and in reality? Are differences in diurnal patterns in different regions likely to influence the results?

This is an interesting question. The epidemiological study we use (Bell et al., 2004) regressed mortalities separately against three different indicators of ozone concentration (1-hr. daily maximum, 8-hr. daily maximum, and 24-hr.), and report the results for all three cases. One can convert between the three reported concentration-mortality relationships, using the relationships between 1-hr., 8-hr. and 24-hr. ozone in the US. As it is not clear in the epidemiologic literature which indicator is best for ozone and mortality, we would be justified in using any of these indicators, and we use the 24-hr. average. In general, we do not expect large differences in the diurnal patterns of ozone in polluted regions of the world. Changes in emissions from different regions may affect the diurnal patterns of the changes in ozone differently, but we do not expect that this will be a major influence on the results. Had we estimated mortalities using 1-hr. or 8-hr. concentration indicators, it is not clear which of these would provide the best estimate.

p.7089 - a full discussion of baseline mortality rates is not required in this paper, but *some indication of regional mortality rates would be valuable so that the reader can judge how significant the calculated changes are.*

We agree with the reviewer that this manuscript contained insufficient detail on the baseline mortality rates. We have given more detail in the text of section 3:

"Non-accidental baseline mortality rates are taken from the WHO (2004) for each of 14 world regions, which are also mapped onto the modeling grid, and vary between 0.416% per year (percentage of the population that dies of non-accidental causes in a year) for the Eastern Mediterranean-B region (which includes Saudi Arabia) and 1.554% per year for the Africa-E region (which includes Ethiopia) (see West et al., 2006)."

We refer the reader to our previous work (West et al., 2006) where the 14 world regions are mapped and baseline mortality rates for all regions are reported.

p.7096 - it would be valuable to estimate the effects of PM changes so that the reader can understand the significance of the calculated ozone effects. Are they of the same order of magnitude, or are the effects of PM likely to be much greater?

As mentioned in the response to a referee comment in part 1, we do not actually model aerosols in these simulations. We are very interested in how these actions would affect PM, although not all of the relevant processes are currently modeled in global chemical transport models, such as the effects of changing OH on the lifetime of organic aerosols. We plan to include this influence in future studies using more updated versions of MOZART that include aerosols. For now, we have alerted the reader to this limitation in the conclusions section:

"This analysis is limited to considering ozone-related mortality; including changes in particulate matter (PM) concentrations due to these precursor reductions may significantly change the results, as PM has been strongly linked with mortality (Pope et al., 2002)."

Direct numerical comparison of the tables would be easier if some of them were combined, particularly tables 2 and 4, and tables 6 and 7.

This is a good suggestion, but in this case we choose to keep Tables 2 and 4 separate, so as to avoid confusing results in the same table, and as similar results are presented separate tables in the companion manuscript. Table 5 is also the basis for comparison with Tables 6, 7, and 8, and so we prefer to keep these separate.

Figures 1 and 2 are small and somewhat difficult to read. I recommend that they are redrafted, perhaps in color to emphasize key elements. The caption in figure 1 should note the change of scales between the plots.

The figures have been redrafted in color, making the axes labels and legend larger. We have also noted the change of scales in Figure 1.

In Table S3, the SA(IN) column should be labeled IN for consistency with the rest of the paper.

We have changed this as suggested, and the corresponding tables in the supporting information for the companion paper.

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