Atmos. Chem. Phys. Discuss., 9, C2669–C2673, 2009
www.atmos-chem-phys-discuss.net/9/C2669/2009/
© Author(s) 2009. This work is distributed under
the Creative Commons Attribute 3.0 License.

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effect of regional precursor emission controls on long-range ozone transport – Part 1: short-term changes in ozone air quality" *by* J. Jason West et al.

J. West

jasonwest@unc.edu

Received and published: 11 July 2009

Response to Anonymous Referee 1.

We thank Referee 1 for his/her attention and thoughtful comments on the manuscript. Referee 1 does not request broad changes to the paper, and so we respond here to the individual comments. (Referee comments are in italics.)

One thing missing from the paper is an assessment of the uncertainties or limitations inherent in the study. How robust are the results? How sensitive are they to the assumptions made or the approaches taken? While a full quantitative analysis is not expected here, it would be valuable to provide a brief assessment of this that goes

further than the simple warnings provided at the end of the conclusions.

We agree with this comment, and this was a focus of the comments made by Referee 2. We have significantly changed the text in the methods section, to communicate the major sources of uncertainty before showing results. We have also improved our discussion of these uncertainties in the conclusions section. Please see the response to Referee 2 for more details.

page 7039, line 15: How is population weighting done? Are the weighted values greater than spatially-weighted mean O3 over key regions? Table 1 does not allow a direct comparison as population-weighted values are only presented for a 3-month period. Are the differences consistent, and are they meaningful in light of the O3 removal expected in highly polluted populated regions?

We have expanded the last sentence of section 2 to describe the population-weighting further:

"Population-weighting is done by multiplying the concentration and population of each grid cell, summing over all grid cells and dividing by the total (regional or global) population, where the global distribution of population is taken from the LandScan database (ORNL, 2005) for 2003, and mapped onto the MOZART-2 modeling grid."

Table 1 has been updated to include an extra column that reports the population-weighted annual average ozone concentration in each region, from the base simulation. This will allow the reader to draw their own conclusions about how the annual average ozone relates to the 3-month population-weighted ozone – in most cases, using the 3-month versus annual average ozone causes a greater difference than does population-weighting versus spatial-weighting.

page 7043, line 9: "lower sensitivity" needs to be explained more clearly here, as it provides little insight into how the differences arise. Does it reflect differences in chemistry, in boundary layer mixing, or just different emissions It would be valuable to

ACPD

9, C2669–C2673, 2009

explore this more deeply.

We agree that it would be valuable to explore the reasons for different sensitivities of different models more systematically. MOZART-2 showed a relatively low sensitivity of ozone to changes in emissions in the HTAP intercomparison (Fiore et al., 2009). The sources of this difference in sensitivity have not yet been clearly explained in the initial papers on the HTAP intercomparison, but may result from differences in the models themselves, their emissions inputs, their meteorology, and their resolution. The present paper differs from the HTAP simulations in other important ways that are clearly documented in this manuscript, the most important of which is likely the different time period simulated. A different version of MOZART-2 is also used here, compared with that in the HTAP exercise. Because the present manuscript only addresses one model, the difference between the sensitivities of different models is beyond the scope of this paper, and would be best addressed for the HTAP experiments. Research to address the differences in model sensitivities in the HTAP intercomparison is presently under way.

page 7045, line 16: it would be helpful to emphasize the physical scale of these "metropolitan" regions (about 900x900 km square); although the approach taken here is appropriate from a global modeling perspective, the scales remain far larger than those typical of metropolitan regions, even for the largest megacities.

Thank you for this comment. We agree that our use of “metropolitan” is potentially misleading, and have changed our wording in section 4.2 to talk about “populated subregions of interest”.

page 7048, line 9-12: The conclusion here needs to be supported by a more detailed or quantitative analysis. The O3 sensitivity is governed by O3 distribution and therefore lifetime, not just by production. While the statement made here may be true, it should be relatively easy to quantify the relative contributions of the different effects.

The suggestion of Referee 1 is an interesting one, and got us thinking about how we present the lifetime in Table 6. We had presented the ozone lifetime associated

ACPD

9, C2669–C2673, 2009

CC BY

with the regional change in NO_x emissions (dBO_3/dPO_3), and have now added the change in global average ozone lifetime (calculated as BO_3/PO_3, relative to the base simulation) to Table 6. Using this indicator we see that the NO_x reductions increase the average lifetime of ozone, for all source regions. This increase in ozone lifetime would tend to increase ozone, therefore counteracting the direct decrease in ozone production. Consequently, it is clear that the change in production is the dominant influence on ozone, which reinforces our emphasis on production. We thank Referee 1 for encouraging us to think more critically about our assumptions here, and have added one sentence to section 4.3 to communicate this:

"In Table 6, the decrease in regional NO_x emissions in each region decreases ozone production, and increases the global average ozone lifetime. Because the global ozone burden decreases for all source regions, we can infer that the change in production dominates over the increase in lifetime, which would tend to increase ozone."

page 7050, line 2: "...decrease...increased... is unclear and should be rephrased

Done. *Figure 5 would be clearer with the individual frames on each plot removed.*

This is a good suggestion, and we've updated the figure accordingly.

Table S3: the results of the model used here are significantly different from those of the models in the other study cited. The conclusions of this study would be stronger if the reasons for the differences were known. A brief assessment of the cause of the differences would be a valuable addition to the paper (see note for page 7043 above.)

See response to the related comment above (p. 7043).

Table S4: dividing the mean O3 response by mean emissions is not likely to give a good representation of mean sensitivity (which is the mean of the response from each model), particularly as the sensitivity is likely to drop as NOx emissions increase due to greater importance of O3 removal by NO. We agree that this is the more accurate calculation, and have updated Table S4 as suggested.

ACPD
9, C2669–C2673, 2009

CC BY

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 7033, 2009.

ACPD

9, C2669–C2673, 2009