Atmos. Chem. Phys. Discuss., 9, C225–C232, 2009
www.atmos-chem-phys-discuss.net/9/C225/2009/

Interactive comment on "The influence of foreign vs. North American emissions on surface ozone in the US" *by* D. R. Reidmiller et al.

Anonymous Referee #2

Received and published: 16 April 2009

1. General comments

The manuscript presents results from an intercomparison of 16 global-scale models for tropospheric chemistry (HTAP). The results are detailed and thorough. As noted by another referee, the results are not very surprising. Nonetheless, this definitely belongs in the public record. By providing a detailed composite view from many models, the paper provides a summary of the current consensus view and the level of agreement or disagreement between models.

The biggest reason for doubt is the poor agreement between model and measured O_3 in polluted regions of the U.S. A systematic bias of +25 % in monthly average O_3 is really quite poor. However, this is shown very clearly in the manuscript, both in terms of monthly averages and in terms of the frequency distribution of O_3 (Figures 6 and

10). The discussion addresses reasons for model error (p. 7936-8, especially p. 7937, l. 4 and p. 7838, l. 8-20). Readers can draw their own conclusions about the reliability of the predicted sensitivity. The relatively poor agreement for O_3 should not prevent publication.

I would not give these modeling studies much credibility if the evaluations were based solely on the comparisons with measured O_3 shown here. However, most of the individual models have been extensively evaluated against measurements for many species, including both surface and aircraft measurements. These previous model-measurement comparisons, along with the many publications in the literature, give the models credibility. This can be clarified by adding a brief description of previous model-measurement evaluations.

It is surprising that the model intercomparison showed a large variation (+/- 25$\%$) among models. This variation suggests that there are still major uncertainties in the current understanding of O_3. Despite this variation, it appears that there is a broad agreement among models with regard to the relative impact of the four source regions (North America, Europe, East Asia and South Asia) on O_3 in North America. This is the main result of the paper: despite the differences in O_3, all these models show agreement on the main policy-relevant result.

I support publication of this manuscript with only minor revisions. The specific comments offer suggestions for added discussion, but these are mainly for the authors' consideration and are not review requirements.

2. Specific comments

1. Model-measurement comparisons: It might help this paper if the authors could add a brief summary of the extensive model-measurement comparisons that have been done previously. Otherwise, readers may get the wrong impression that the ozone comparison shown here is the only model evaluation.

It would also help if the paper could include a brief discussion of previous evaluations of the individual models in comparison with measured global O_3. The results in Figure 4 and 4a suggest than some models systematically overestimate O_3 throughout North America, while others do not. Are there similar overestimates for O_3 at the global scale? My guess is that all the participating models have shown reasonable agreement for O_3 in comparison with global measurement networks.

This last issue is briefly discussed on p. 7937 (line 4), but the discussion leaves the impression that the different models systematically overestimate or underestimate $O_(3)$ throughout the troposphere (at least relative to each other). Perhaps this could be clarified.

2. As stated above, it appears that the models are all in agreement on several important issues. The impact of emissions from EA, SA and EU combined has roughly 10% of the impact of NA emissions for events with MDA8 O_3 above 65 ppb, though it can be as high as 50% for MDA8 O_3 between 55 and 65 ppb. EA and EU have roughly equal impact on NA (with slightly higher for EA), while SA has less impact. This is based on multi-model averages and standard deviations shown in Figures 6, 9 and 10.

I suggest that the authors check whether these broad conclusions hold true for each individual model, rather than just for the multi-model average. There might be some graphical method to show this – for example, an equivalent to Figures 6 and 10 but showing values for each individual model. (This is not suggested as a substitute for Figures 6 and 10, which are useful and clear just as they are.) An additional plot is not necessary, but it would be useful if the text could identify some conclusions about foreign impacts that are supported by all 16 models.

3. Abstract, l. 19, : "East Asia is the largest contributor.... the exception is in the NorthEastern U.S.", and p. 7942, l.1 "The influence from EA is greater than that from EU".

This is a bit misleading. The relative contributions of the three regions is described

more accurately in the conclusion (p. 7946, l. 5): " EA emissions have the greatest effect on US air quality... followed closely by EU emissions... both of which have a far greater impact than SA emissions." However, both the abstract and the main body on p. 7942 are written as though EA impacts are much larger than EU impacts. (The contribution from Europe is 20$\%$ less than the contributions from East Asia in most regions of the US.)

Also, the abstract implies that the foreign impact is very different in the northeast relative to the rest of the US. In fact, the difference between regions is relatively small: EU has a slightly larger impact than EA in the northeast, while EA has a slightly greater impact elsewhere.

4. Page 7933 and Figure 1: There is some confusion about the regions and what they mean. The text describes how "regionally representative" sites were chosen for each of 10 EPA regions. However, the site locations in Figure 1 do not always correspond to the geographic description. This is only a problem for the "Plains" region. Both the name and the map suggest that the region consists mainly of the far-midwest and Great Plains. But the three representative sites are two in Illinois and one in Arkansas, each of which is affected by emissions from large urban areas (St. Louis, Dallas). This is apparently because the CASTNET, though rural, has fewer sites in sparsely populated regions.

It would be helpful to clarify this "Plains" oddity in the CASTNET description.

Also (p.7935-6): Presumably, the model values are determined in a way directly analogous to the measurements: daily regional model values represent the average of the model values at each of the regionally representative CASTNET locations, and monthly averages, number of exceedence days, etc. are based on those model values. This should be stated in the text to avoid any possible confusion.

5. Page 7943, l. 11: "Figure 10 shows... the impact from NA emissions... the interseasonal difference (summer vs. spring/autumn) is 25$\%$..."

I believe this is incorrect. Figure 10 does show a 25% difference between summer and spring/autumn, but only when the comparison is done between days with the same MDA8 O_3. If the comparison were made for all days, then the inter-seasonal difference would be a factor of 2 or more.

For example: model results from the southeast region show a 5-6 ppb decline in MDA8 O_3 for summer, compared to a 4-5 ppb decline in spring and fall, both for days with >55 ppb MDA8 O_3. (Presumably this is the 25% difference.) But for summer the days with >55 ppb represent 100% of the season (in the model), while in spring and fall they represent 35% of the season. The remaining days in spring and fall have lower MDA8 O_3 and much lower decline (<2 ppb). The season average decline is approximately 3 ppb for spring and fall compared to 5.5 ppb for summer.

6. Page 7944-5: I believe that previous results from Fiore et al. also concluded that the impact of local emissions is larger and that of foreign emissions is lower during events with elevated O_3. The authors might consider adding an appropriate reference to this result.

7. The conclusion (p. 7946, l. 13, also p. 7943, l.7) warns that the model may overestimate the impact of NA emissions (and therefore may overestimate the impact of NA relative to the rest of the world) because the model overestimates O_3 in comparison with measurements. It is good that they have included this warning, especially since the NA impact is strongly correlated with ambient O_3 (Figure 12).

However, the conclusion also suggests that Figures 7c and 7d (showing little correlation between model-measurement bias for O_3 and model response to NA emissions reductions) provides counter-evidence. I think they are mistaken here. The model-measurement bias shows little correlation with the response to NA emissions (Figure 7) and the response to NA emissions strongly correlates with model O_3 (Figure 12). This implies that the model-measurement bias is also poorly correlated with model O_3. However, the model-measurement bias is likely to show a strong correlation if plotted

against the **measured** MDA8 O_3.

To oversimplify: the model in the southeast during summer is reporting elevated O_3 on every day, with high NA influence on every day, and little day-to-day variation. By contrast, the measurements show days with both high and low O_3. The days with high measured O_3 show good model-measurement agreement, while the days with low measured O_3 show model overestimates. In this situation it is likely that the days with low measured O_3 also correspond with model overestimates in NA influence.

No change to the text is necessary since the authors have already included a warning that the model may overestimate the impact of NA emissions. I am skeptical about their interpretation of Figure 7(c) and 7(d), however.

3. Technical corrections

This section also includes minor issues that can be corrected with small changes in wording.

Abstract, line 15: The grammar is awkward in the sentence containing "... in each of the source regions:".

Abstract, line 29: " growth in emissions upwind of the U.S." It would be better to say "outside the U.S.". It is not strictly accurate to describe Asia as "upwind of the U.S.", since it is half a world away.

Page 7935, line 1, and Table 1: The calculation of the number of exceedence days in the "Region" is confusing. On p. 7934-5 it states that "exceedence days... are determined by averaging the MDA8 O_3 values from each "representative" site in the region. However in Table 1 it states that exceedence days are determined by averaging MDA8 O_3 values from all sites in the region, not just the " representative" sites.

Also, in both places the process is described as follows: "then analyzing the multi-site regional mean MDA8 O_3 value". My guess is that this means that they classify the number of exceedence days and identify the 4th highest MDA8 day based on the multi-site mean for each day, regarding the mean value as though it were a single site. Please clarify the writing.

Page 7937, line 10: It is reasonable to exclude winter, but the explanation is a bit strained. Long-range transport is not necessarily less in winter than in spring and fall. It would be more valid to state that O_3 is at its seasonal minimum value in almost every region in the U.S., and exceedences are rare.

Page 7939 line 19: "... the bias is present in all airmasses, regardless of the degree of local O_3 buildup."

This is awkwardly put. Technically, the bias is not present in all airmasses, because the bias in Figure 7(d) ranges from -2 to +30 for individual days.

Page 7941, line 23: "If O3-precursor emissions continue to grow abroad (particularly in the EA and SA regions)... "

The study results do not suggest that emissions in EA and SA have a greater impact on the U.S. than emissions elsewhere. The reference to EA and SA is probably based on the authors' expectation that emissions in these regions will increase. I suggest cutting the reference to EA and SA.

Page 7943, line 19: "The effect of NA emissions reductions is almost twice as great in the Eastern US because most anthropogenic O3 precursor emissions are east of the Mississippi River."

I think the region east of Mississippi River accounts for half of precursor emissions in the US. (These track very closely with population). It would be more accurate to state that the density of anthropogenic precursor emissions (per unit surface area) is much higher east of the Mississippi. The effect of emissions reductions in California is nearly as large as in the eastern U.S.

Figure 4: It is hard to identify the individual models in this plot, because the colors are very similar for many of the models. It would help if different point symbols were used

in addition to different colors.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 7927, 2009.