Atmos. Chem. Phys. Discuss., 9, C1967–C1971, 2009
www.atmos-chem-phys-discuss.net/9/C1967/2009/

Atmospheric Chemistry and Physics Discussions

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

D. Reidmiller

dreidm@atmos.washington.edu

Received and published: 17 June 2009

Anonymous Referee #1

Received and published: 7 April 2009

Reidmiller, D. R. et al.: The influence of foreign vs. North American emissions on surface ozone in the US, Atmos. Chem. Phys. Discuss., 9, 7927 – 7969, 2009.

General Comments

(1) REVIEWER This paper describes the impact of foreign and North American domestic emissions on surface O3 levels in the United States. It is based on results taken from multi-model ensemble model runs undertaken in the framework of the Taskforce

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

on Hemispheric Transport of Air Pollution. The paper is generally well written and takes advantage of the availability of results from many models. None of the results is really surprising. Rather, results from previous studies using individual models seem to be reproduced in the current paper, probably with a better estimate of the model uncertainty based on the spread between the different models. However, can this spread in the model results be taken as a measure of true uncertainty?

(1) RESPONSE The reviewer is correct in his/her assessment that the results we present corroborate previous investigations. However, our study is novel in that it is the first multi-model assessment with specific insight into how the foreign vs. North American influences differ: (a) by season, (b) by region of the U.S., (c) across the O3 distribution. We point out in the introduction that various modeling studies have used various metrics for uncertainty. Also, we agree that model spread is only one measure of the uncertainty and this is explained in the text in the second to last sentence of Sect 2.2.

(2) REVIEWER The differences to the measurements of individual model results (and even of the multi-model mean) are quite large and I am wondering whether the model-measurement agreement really supports the model results sufficiently to trust the emission response estimates. A comparison of the number of exceedances of certain ozone thresholds is probably insufficient to confirm that the models are providing good estimates of the effect of emission reductions in various regions. Without proper validation of these modeling aspects, the study is a pure model exercise that is not really supported by any measurements.

(2) RESPONSE We devote an entire section (Sect. 3) to "Model evaluation with CASTNet observations," so we feel we have presented sufficient validation of the multi-model mean to move forward in interpreting the perturbation simulations. The evaluation efforts do not merely entail comparisons of exceedance days, but also monthly biases, correlations, etc. These results are not only covered in Sect. 3, but also in Table 3, Fig. 4(A4), 5(A5) and 7(A7). Fig. 7(A7) investigates the presented biases in more detail

ACPD

9, C1967–C1971, 2009

with discussion of the findings in the last paragraph of Sect. 4.

(3) REVIEWER I would have liked to see attempts of using the observation data for identifying periods of foreign influence, for instance, rather than trusting fully the models.

(3) RESPONSE We have added text to the beginning of Sect 3 to address this comment. In brief, it is very difficult to identify periods of foreign influence with observational data alone - especially for low-altitude sites, such as the CASTNet sites we use in this analysis. Rather, this study sought to quantify the more continuous foreign influence as opposed to 'case studies' when the magnitude of the foreign influence may have been greatest.

(4) REVIEWER At least, a critical discussion of these issues is urgently needed before the paper can be published in ACP.

(4) RESPONSE We feel we have adequately addressed the reviewer's concerns regarding model evaluation with the presence of Sect. 3, text at the end of Sect. 4, Table 3, and Figs. 4, 5 and 7. Note, we have added text to the beginning of Sect 3 discussing model evaluation in more detail.

Other major comments

Other major comments:

(a) REVIEWER I find the introduction somewhat unbalanced. It gives a little the impression that North America (or maybe even the U.S. only) is impacted by foreign emission sources but that the North American emissions are not as important for impacting other regions. I suggest a more balanced view on the role of North America as a receptor and source of ozone, the impact of which may be seen over other continents.

(a) RESPONSE We have inserted a paragraph discussing the North American-to-elsewhere long-range transport of pollution. Specifically, we mention the NARE and ICARTT field campaigns with references. Also, we include references for ozone photo-

ACPD

9, C1967–C1971, 2009

chemical tendencies in the region as those are directly relevant to the work we present as well as the Li et al. (2002) reference to air quality impacts in Europe due to North American emissions.

(b) REVIEWER Page 7935, first line: Here, the ozone values are averaged over several sites and subsequently the number of exceedances of an ozone threshold of 75 ppb is determined. However, for an extreme value statistics such as counting days above a certain high value, this averaging procedure does not make much sense. The more values are averaged, the less likely is it that a certain (high) threshold will be exceeded. This is reflected in the results, I think, which indicate fewer exceedances for the areas than for individual stations. It would make much more sense to calculate the exceedances for each site individually and then average the number of cases with exceedances found at each station.

(b) RESPONSE We have changed our method for calculating exceedance days for the "Region" as the reviewer suggests. The text (paragraph 5 of Sect 2.1) has been amended, as has the caption for Table 1.

(c) REVIEWER Fig. 3: The line and symbols for the year 2001 are not highlighted enough in this rather busy graph. Year 2001 should stick out more.

(c) RESPONSE We have increased the symbol size and thickened the (red) line in Fig. 3 (and Fig. A2).

Minor points

(d) REVIEWER Page 7935, line 10: ïĄş has not been previously defined, I think.

(d) RESPONSE We have inserted "...(where ïĄş indicates standard deviation)..." in the last paragraph of Sect. 2.1.

(e) REVIEWER An observational study of the impact of Asian versus North American influence that is probably worth citing is: COOPER, O. R., et al.: JOURNAL OF GEOPHYSICAL RESEARCH, VOL. 110, D05S90, doi:10.1029/2004JD005183, 2005

ACPD

9, C1967–C1971, 2009

(e) RESPONSE We have included this reference in the 4th paragraph of the introduction; we thank the reviewer for bringing our attention to this relevant study.

(f) REVIEWER Page 7937, line 11: Saying that summer is typically not a season of long-range transport is probably a too strong statement. Adding "from Asia to North America" would make this a correct statement.

(f) RESPONSE We have amended the text accordingly.

(g) REVIEWER Page 7937, line 21: the equation in this line is a bit awkward, since it mixes text with equation symbols. I would say "divided by" instead of "/"

(g) RESPONSE We have amended the text accordingly.

(h) REVIEWER Page 7946, line 4: "in most all regions": rewrite

(h) RESPONSE We amended the text to read "in almost all regions", and have added a clarifying sentence afterwards, "The exception to this is in the Northeast where. . ."

** We thank this reviewer for his/her analysis of our manuscript. Their comments enabled us to clarify our work and we are grateful.

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