**Atmospheric
Chemistry
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Discussions**

Interactive
Comment

Interactive comment on "The influence of boreal biomass burning emissions on the distribution of tropospheric ozone over North America and the North Atlantic during 2010" *by* M. Parrington et al.

Anonymous Referee #2

Received and published: 9 November 2011

This paper presents a number of analyses related to boreal biomass burning in summer 2010, associated with the first phase of the BORTAS field experiment. First, a summary of the boreal fire activity for that summer is shown, based on the FLAMBE inventory. Then, GEOS-chem global model results are evaluated with a variety of ozone observations (TES and IASI satellite retrievals, ozonesondes and ground-based data). Next, an analysis of the sensitivity of the ozone distribution to various NO and CO sources is shown using the adjoint of GEOS-chem. As part of the adjoint analysis, CO emissions are optimized. The optimized CO emissions are then used in a forward model run and compared to the original run.

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Interactive Discussion

Discussion Paper

I feel the paper is worth publishing, after addressing the comments below.

Each section of this paper is written well, and the techniques used are generally clearly explained. However, the sections do not follow one from the other very smoothly, and it is difficult to see what the overall goal of the paper is. Additional text in the transition between sections would be helpful.

The Abstract and Conclusions should emphasize the significance of the results. I find it hard to get excited about 1-2 ppbv changes in mean ozone.

I feel a few things are missing from the analysis. Evaluating ozone alone does not say very much about how well the model is performing, or how accurate the emissions are. Comparison of the model to ozone precursors would be very valuable. Many models show good agreement with ozone observations while not representing CO, NOx, NOy and VOCs well. An "analysis" of the BORTAS surface measurements at Dalhousie and PICO-NARE (which the authors say is beyond the scope of this paper) is certainly not required, but using the observations of CO, NO, NOy, etc., to evaluate the model results at those sites would be valuable.

The adjoint results shown in Fig. 9 show that ozone is more sensitive to the NO emissions than CO, but the CO emissions were optimized. The text (Section 5.3, firs sentence) implies only the CO biomass burning emissions were changed for the final sensitivity run in Fig. 12. However, the results look like NO emissions must also have been changed? And the Conclusions imply they have. Please clarify. If the FLAMBE inventory needs to be adjusted for CO, it follows that all species ought to be scaled. Though, there is also uncertainty in the fire emission factors correlating CO and NO emissions, for example.

A comparison of the model to satellite NO2 observations, particularly over the fire regions, would be a valuable addition to this paper, and provide some confidence in the ozone results and other conclusions of the paper.

Specific comments:

Abstract, 1st sentence: Do you mean "We analyse *the sensitivity of* the tropospheric ozone distribution . . . "

Section 2.1, line 9-11: I don't follow this sentence. What are the MODIS fire emissions? Do you mean FLAMBE or MODIS fire counts? MODIS has 1 overpass per day, which gives fire counts for a single local time (or maybe 2 for day and night) each day. What are the 6-hourly observations?

Section 3: For which species are MEGAN biogenic emissions calculated? Is this online in GEOS-chem, or have you used pre-calculated emissions?

Section 4.3.2, line 25: The sensitivity of the retrieval does not depend on the vertical grid used in the retrieval. Rewrite this sentence.

p. 25124, line 11-12: Are these all of the CO sources included in the model? What are "monoterpene sources"? These should be explained in section 3. How is the CO produced from oxidation of methane and VOCS treated in the inversion?

p. 25128, line 14: You should clarify that PAN is emitted directly by fires in GEOS-chem (I guess that is the case), but there is no evidence of direct emissions in reality.

Figures 2 and 3: Too many significant figures given. Integers or tenths are sufficient.

Fig. 7: Clarify the caption to indicate the model results have been transformed by the satellite retrieval averaging kernel and a priori.

Fig. 9: "a" and "b" labels are missing.

Fig. 13: Do these profiles show the mean of the differences for each observed profile, or is it the difference of the mean? It should be the first, and please show error bars. And clarify in the text what is shown.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 25099, 2011.