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Atmospheric Chemistry and Physics Discussions

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 #1

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This manuscript generally provides an evaluation of the ozone distribution as simulated in GEOS-Chem, relative to in-situ (i.e. ozonesondes) and satellite observations (TES and IASI) over North America, and North Atlantic and coinciding within the BORTAS field campaign time period. In addition, this work tries to quantify the influence of boreal biomass burning to the ozone distribution being evaluated. In particular, it elucidates the impact of NOx and CO emissions from biomass burning, using the adjoint of GEOS-Chem and subsequent inversion of CO sources using MOPITT CO observations. This work is therefore unique in this respect. I recommend the publication of this manuscript with minor revisions following some of my concerns below:

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Interactive Discussion
Discussion Paper

a) I find that the manuscript still lacks cohesiveness. I see at least 3 sub-topics (1) evaluation from PICO-NARE, ozonesondes in Canada, TES and IASI, (2) adjoint sensitivity of ozone profiles in BORTAS domain to model variables such as NOx biomass burning, etc and (3) CO inversion and its impact on ozone distribution. However, I do not see a cohesive section that ties the three sub-topics.

b) On the statistics being used: I was wondering if the reported reduction in mean bias is statistically significant given the reported accuracy and variability of the measurements. I am especially interested on the improvements after CO inversions, which are less than 5 ppbv. Also, the manuscript uses the mean bias as the main metric of performance. Should we be expecting the mean bias to be well within the variability of the measurements, given that the large-scale features are already generally well-captured by the model? Rather, should we be focusing on looking at other metrics?

c) On the CO inversions and section 5.3: From my understanding, only the CO emissions from biomass burning were scaled. Should the other species emitted from biomass burning be scaled as well? How does this impact your results? Does this work assume that the reduction in CO sources is due to an overestimation of carbon emissions (and that the emission factors are fixed)?

Minor Comments:

1) p 25108 line 25. What do you mean by interpolation here? Does this mean that the carbon emissions were not aggregated to the model resolution? What was the resolution of FLAMBE prior to aggregation? Please clarify.

2) P. 25110. Is there an updated emission factor database from Andreae and Merlet (2001). What would be its impact on your emissions?

3) How does the mean bias of GEOS-Chem relate to the accuracy of the measurements being compared? I asked this since TES, IASI have different biases. Are these accounted for when reporting the mean bias of GEOS-Chem?

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Interactive Comment

4) P 25116 line 25. The filament described in the text is hard to see.

5) P 25118 line 5. Is this still consistent with the 8% bias reported in the previous page?

6) P 25122 line 5. What do you mean by “normalized” in this context? Would a simulation of tagged O3 show similar sensitivity?

7) Table 1. Please describe the mean (median) bias column in the caption. I understand that this was describe in later section, but at first glance it can be interpreted to be a bias of ozonesonde.

8) Figure 9. Where is (a) and (b) in the figure?

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