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Interactive Comment

Interactive comment on "Biomass burning influence on high latitude tropospheric ozone and reactive nitrogen in summer 2008: a multi-model analysis based on POLMIP simulations" *by* S. R. Arnold et al.

Anonymous Referee #1

Received and published: 27 October 2014

Title: Biomass burning influence on high latitude tropospheric ozone and reactive nitrogen in summer 2008: a multi-model analysis based on POLMIP simulations

Reviewer Comments: Summary: This paper evaluates the ozone enhancement in biomass burning plumes observed at high latitudes during July 2008 using the suite of chemical transport models involved in the POLMIP comparison. The paper shows that the evolution of plume composition is highly sensitive to the underlying meteorological data used to drive the models. In particular, the efficiency of vertical transport during poleward export from mid-latitude source regions has a large impact on the ΔPAN/ΔCO relationship. Arctic ozone production in the plumes is highly sensitive to the initial PAN abundance.

Overall Comment: Overall this is a well-written and thorough paper. The results are not surprising, but this type of careful comparison is useful for the modeling community. My main concern is the use of $\Delta O_3/\Delta CO$ as a diagnostic for ozone production. Recent work (e.g. see Zhang et al. [2014]) advises against using this diagnostic without verifying the assumption of negligible CO loss. Could the authors do that prior to publication in ACP? That would provide a much stronger foundation for the remainder of the analysis. This is my rational for accept subject to minor revisions. Most of my other comments are technical in nature.

Zhang, B., Owen, R. C., Perlinger, J. A., Kumar, A., Wu, S., Val Martin, M., Kramer, L., Helmig, D., and Honrath, R. E.: A semi-Lagrangian view of ozone production tendency in North American outflow in the summers of 2009 and 2010, Atmos. Chem. Phys., 14, 2267-2287, doi:10.5194/acp-14-2267-2014, 2014.

Minor Comments:

Pg: 24575, lines 12-13: Grammar issue here. Missing “in”

Pg:24584, lines 25 onward: This designation would still be problematic if there was CO loss in the plume.

Pg: 24586, lines 7-9: The authors should describe a) the implementation of and rational for the HO_2 uptake in GEOS-Chem, and 2) how this implementation impacts the abundances of PAN and other species relevant to the paper.

Pg: 24588: lines 19-22: The standard version of GEOS-Chem does not emit NOy with this partitioning. This indicates that the model used in the comparison should probably be better documented. Somewhere in the text should point to a reference for this version with a statement that the model is not a public release version. When

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this chemical partitioning is combined with emitting a fraction of the smoke above the boundary layer, I suspect there are likely to be different results.

Figure 5 and Figure 7: There seem to be excessive significant figures in this set of figures.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 24573, 2014.

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