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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modeling of trace gases from the 1998 North Central Mexico forest fire smoke plume, as measured over Phoenix" *by* V. R. Kotamarthi et al.

Anonymous Referee #2

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Review of "Modeling of trace gases from the 1998 North Central Mexico forest fire smoke plume, as measured over Phoenix" by Kotamarthi et al.

General Comments

Based on ground and aircraft observations over the Phoenix area in 1998, the authors have performed model calculations to examine the chemical evolution of biomass burning emissions transported from Mexico. Unfortunately, the model results are not well-constrained by observations, nor do they compare well with the conditions observed in Phoenix, either on the ground or from aircraft. Chemical conditions in the modelled plume appear to have been derived entirely from extrapolations based on

CH3Cl and attenuated UV (but no direct aerosol sampling). Plume composition is further influenced by the entrainment of background air based on results from a 3-D CTM (MOZART), but there is no evaluation of whether the MOZART predictions are reasonable. Model results generate significant ozone while observations indicate depleted ozone associated with air of biomass burning origin. I expect that the ground and especially the aircraft observations have not been fully explored. What about CO and NOx on the plane? Any NMHC or aerosol observations? If the measurements reported in this manuscript represent a full accounting of the available data, then they are insufficient to support the scope of modeling or the conclusions being drawn by the authors.

Specific Comments

Line 25, page 3228: The authors state,"However, little is known about the short-term effects of forest fires on air quality in North America, particularly in urban and semi-urban areas (Wong and Li, 2002)." This gives the impression that such work is scarce, but other important references have been overlooked, e.g., McKeen et al., 2002 and DeBell et al., 2004.

Line 19, page 3229: The reference to Peppler et al. is appropriate, but the discussion of Peppler et al. results seems rather pointless given there was absolutely no overlap between the three mentioned variables (ozone, large particles, and single scatter albedo) and the elevated ozone was apparently not related to the fires.

The use of TOMS data offers little of value, especially since the TOMS data do not corroborate a biomass burning influence over Phoenix for the period in question. If TOMS data is of interest, why isn't data for the flight days (21 and 22 May) included?

The url offered to direct the reader to the details of the G-1 operation are useless. All Phoenix 1998 links to either plans or data are dead.

Line 11, Page 3231: The text states that mean values in figure 2 are for "15 May to 12

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June," but the legends in Figures 2a-c state data is for "May 15 to June 15."

Line 20, page 3231: The authors state, "This additional NOx most likely resulted from lower rates of conversion of NOx to HNO3 in the air as a result of decreased photochemical activity in the smoky conditions." By this do the authors mean that the observed NOx is of local origin, but has a longer lifetime? Why isn't the NOx shown as a fourth panel in Figure 2? It is certainly a key player in understanding any variations in ozone.

The interpretation of data from the flights is rather confusing. It looks to me like the lower ozone on 21 May is simply due to clean tropical marine air which is typically very moist and depleted in ozone. H2O2 is higher, but CH3OOH is lower, so the assertion that this air has a biomass burning origin doesn't make sense. I am certain this wasn't all that was measured on the G-1. There had to have been some CO observations. What about NMHCs from whole air samples? Peroxides and ozone should never be considered sufficient to diagnose a biomass burning influence.

The text states that the trajectories originate "close to the site of several observed forest fires," but absolutely no information is offered as to the characteristics or severity of these fires.

The authors try to make the case that the trajectory calculations are well constrained, but almost every aspect of these calculations is based on an assumption:

-initial conditions are based on correlations with CH3Cl from African savannah fires making the claim of a "fairly robust estimate of the initial conditions" rather bold. Andreae and Merlet (2001) show emissions of CH3Cl versus CO, NOx and other gases to be highly uncertain as well as dependent on the type of fire.

-the black carbon used in the model to influence radiation and photochemistry have absolutely no relationship to any observed aerosol or aerosol-related variables in the Phoenix study.

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-background air entrained into the plume are based on MOZART conditions, but no evidence is offered to demonstrate that MOZART results are reasonable.

For airmasses described as having a biomass burning origin, ozone was observed to be 20-30 ppbv on the ground and 40-50 ppbv from the plane. Calculated ozone values are much higher than those observed. Ultimately, there appears to be no serious effort to relate the model results to the observations in the Phoenix area.

References: Andreae, M. O. and P. Merlet, Emission of trace gases and aerosols from biomass burning, Global Biogeochem. Cycles, 15, 955-966, 2001.

McKeen, S. A., et al., Ozone production from Canadian wildfires during June and July of 1995, J. Geophys. Res.,107, doi:10.1029/2001JD000697, 2002.

DeBell L. J. et al, A major regional air pollution event in the northeastern United States caused by extensive forest fires in Quebec, Canada, J. Geophys. Res., 109, doi:10.1029/2004JD004840, 2004.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 3227, 2006.