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## ***Interactive comment on* “Characterizing ozone production in the Mexico City Metropolitan Area: a case study using a chemical transport model” by W. Lei et al.**

### **Anonymous Referee #2**

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Review of “Characterizing ozone production in the Mexico City Metropolitan Area: a case study using a chemical transport model” by Lei et al.

This is another paper of a large number investigating smog dynamics in Mexico City. Here, they use CAMX, a widely used air quality model, to simulate gas phase dynamics in the region. After the initial application, they evaluate the model using their available data. The results are plotted against the average measured concentrations, which is a non-standard approach. Using the model they then investigate photochemical formation rates and use that to suggest that the core of the MCMA is VOC limited, and that there is a heterogeneous source of HONO. They also do a couple of emission

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reduction cases.

My first comment in regards to this paper is that it struggles to distinguish itself from the many other studies of Mexico City and air quality model applications in general. What have we learned that has not been pointed out before? You can look back at prior modeling of Mexico City, and just analysis of the measurements, that suggest Mexico City is currently radical-limited due to the high NO<sub>x</sub> levels. That a heterogeneous HONO source is out there is known.

A second comment is that their domain, shown in Fig. 1, is really small, and actually does not look right. We do not need to see the old CIT domain. That is history. We do need to see the 36, 12 and 3 km domain boundaries. If the 36 km model domain is really as shown, they need to go back and use a larger domain such or do a variety of other tests taht concretely show that the results are not dependent on the boundary conditions, at least over the region of interest. The one shown is just too small.

While they claim to have evaluated the model, they use non(sub)-standard procedures. In Figure 4, they compare their averaged results to averaged concentrations from the observations. This can disguise many problems dealing with spatial variation. Indeed, Figure 3 shows that this is likely the case. At a minimum, they should have a table of standard performance measures. Even this would be a bit deceptive because they have also adjusted the emissions inventory. They next show their VOC results, having modified the inventory to match. Their choice of how to present model results makes the reader wonder what is really going on. They should provide the results when the standard inputs are used.

Fig. 8 is very reminiscent of work done in the '90s on VOC reactivity, but you would never know it from the literature cited. This should be corrected.

A final comment is that it would be nice for the group to be a bit more sophisticated in their assessment of emission reductions. Why do this without looking at specific sources and likely levels of controls on those sources?

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In general, while many aspects of the model application are of interest, the small domain (if that is the case), the fact that it is “another model application in Mexico City” with little new to offer and the substandard model evaluation approach weaken the manuscript significantly.

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Interactive comment on Atmos. Chem. Phys. Discuss., 6, 7959, 2006.

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6, S3564–S3566, 2006

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