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Geoscientific Model Development Discussions

Interactive comment on "Impact of a new condensed toluene mechanism on air quality model predictions in the US" *by* G. Sarwar et al.

Anonymous Referee #1

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Comments: Impact of a new condensed toluene mechanism on air quality model predictions in the US

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General comments:

This manuscript describes the sensitivities of ozone and several PM species to CB05 and CB05 with a new toluene mechanism for western and eastern US. The impact of the new mechanism on RRF and OPE is discussed as well. Since aromatic chemistry is important to ozone and secondary PM formation, this work is an important step and relevant to this journal. The manuscript is generally well written on a scientific base. However, the authors should make at least moderate revisions before it can be

considered for publication. I list my specific comments below.

Specific comments.

Section 2.1, line 27. Why two different years were selected for the simulations for eastern US and western US? Since the two domains give readers a big picture of whole continental US. It will be more interesting to see model result for the two model domains over same time period. Will your conclusions change if you run July 2001 for western US or July 2002 for eastern US?

Section 3.2. In the second paragraph of this section, the authors show day-to-day variation of the increases in daily 8-hr maximum O3 in 6 cities. However, the description in the text is rather simple and the information I get from this paragraph is no more than that from the first paragraph. For this part, I would expect to see more detailed explanation to the day-to-day variation in at least one city in each model domain. For example, in Los Angeles, why the change in 8-hr O3 is about 6.5 ppb in day 7 while in day 24, the change is less then 0.5? What's the difference between these two days? This additional information will provide more insights into how the new mechanism will impact on air quality under different conditions.

Section 3.4. OPE is defined as the slope of a regression between O3 and NOz. So the correlation coefficient between O3 and NOz is critical. I guess at both Los Angeles and Chicago, the correlation coefficient should be low which will make the OPE value less meaningful. So please provide the correlation coefficients where OPE values are discussed. With new mechanism, the OPE value is slightly lower at LA and keeps same at Chicago. But if we look at figure 4(b) and figure 5(b), monthly mean NOz at both LA and Chicago are increased by about 25% while monthly mean daily 8-h O3 are increased by less than 2.5% at both sites when new mechanism is used. So that suggests the OPE is actually lower at both sites. I would recommend the authors to replace Figure 4a,b with same plots using mean NOz from 10:00am-5:00pm. If the new plots are very different to the original ones, this will also indicate that the impact

on NOz is different during daytime than during nighttime.

Section 3.7. The predictions for PM species from base mechanism vary spatially (e.g. western domain vs. eastern domain), so does the difference between the two mechanisms. A discussion/explanation with respect to these spatial variations should make this section more complete and provide more insights. O3 problem usually only exists during summer time. But PM problem could be more significant during winter month especially in western US, like California. Do the authors have any ideas with regard to the impact of the new mechanism on PM predictions during winter time?

Interactive comment on Geosci. Model Dev. Discuss., 3, 2291, 2010.