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Interactive comment on "On the temperature dependence of organic reactivity, nitrogen oxides, ozone production, and the impact of emission controls in San Joaquin Valley California" *by* S. E. Pusede et al.

Anonymous Referee #2

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This manuscript contains an excellent treatment of the relative importance of nitrogen oxides and VOCs to ozone production in the southern San Joaquin Valley of California, an area with a long history of exceedances of the US National Ambient Air Quality Standard for ozone. The authors used in this analysis a very substantial air chemistry data set from a ground site downwind of the city of Bakersfield, CA that was collected during the CalNex campaign of May - June 2010, along with observations from the routine air monitoring sites in this region. They performed detailed calculations of HOx and O3 production rates from the available data, and then made perturbations to the emissions of NOx and VOCs to simulate possible future controls. The results of these calculations show that there are two distinct sets of VOCs present, those with temperature-dependent OH reactivity and those that are temperature independent. The latter have been reduced substantially over the last couple of decades, but the results of this analysis show that further such reductions will do little to further reduce exceedances of the ozone standard. Instead, NOx emission reductions will be much more beneficial.

The authors have produced a very well written paper, and it should send a strong message to air regulators concerning the types of future emission controls would have the most benefit in terms of reducing ozone mixing ratios in this region. I suggest that the manuscript be accepted with minor revisions as I have outlined below.

p. 28513, line 11: Should mention possible sources of these VOCs with temperature-dependent reactivity here in the abstract

p. 28519: Here the authors detail their calculation scheme for PO3. The observed O3 mixing ratios are a function of both PO3 and LO3 (ie: both production and loss). Shouldn't the NET PO3 be used here, considering both production and loss (probably most important would be O3 + OH and O3 photolysis, which could be calculated quite easily). I would think at least a sample calculation of the net production should be included. Perhaps that would be highly correlated with the PO3 during the 10 AM to 2 PM period that is considered. If so, that should be demonstrated.

p. 28520, lines 20-21: I know you need HOx to be in steady state over 10 AM - 2 PM for your calculations. But, having to adjust the photolysis rates by 10% means that HOx was not really in steady state. Can you discuss why this might have been the case?

p. 28522, lines 3-6: I don't understand why you don't use the average temperature over the same interval as the chemistry measurements (10 AM to 2 PM). What is the correlation between daily max T and the average over 10 AM - 2 PM?

p. 28527, line 8: not on weekdays. It looks to be < 10 on weekdays from the figure.

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p. 28530, line 22: Should this be 20 -27 deg., rather than 2 - 20?

Figure 6 caption: Please further clarify the meaning of the wind direction arrow on the right side of the figure. I think it is referring to the fact that the wind is blowing from a high NOx region to a low NOx region, but it took me a while to figure that out.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 28511, 2013.

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