Interactive comment on "Direct measurement of ozone production rates in Houston in 2009 and comparison with two estimation methods" *by* M. Cazorla et al.

Anonymous Referee #2

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This paper capitalizes upon the rich array of observations during the SHARP campaign to present the first evaluation of the Measurement of Ozone Production Sensor (MOPS) against modeled and calculated ozone production rates (P(O3)). Given the novelty of the MOPS approach and the value of intercomparing the three methods during ozone pollution events, this paper certainly merits publication. However, since all three methods of estimating P(O3) are prone to significant uncertainties, greater caution is needed in diagnosing the cause of the discrepancies as discussed below. Several specific comments and minor edits should also be addressed prior to publication. Major comments: The conclusion that discrepancies among the approaches indicates underprediction of P(O3) by the model has not been proven. The authors appropriately

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note that all three approaches have significant uncertainties, and acknowledge that the uncertainties of the MOPS are poorly understood given the newness of the technique. However, at several points the paper suggests without clear justification that "missing radical sources" in the model are the likely cause of the discrepancies. The focus on error in the model seems to be driven by the claim that the calculated results have "better quantitative agreement" with measured P(O3) than the model does. However, that depends on the statistic used. The model was closer to measured for R2, RMSE, and the shape of the P(O3) vs NO response, whereas the calculated was closer to measured for IA, MBE, and the magnitude of P(O3). For the model to be showing P(O3) peaking at the right NO level, it is surprising that there would be a major missing radical source. A limitation of all of these comparisons is that P(O3) is a highly uncertain quantity. The authors should first directly compare the measurements of HO2 and OH to those predicted by the model. Also, it may be possible to infer an approximate "true" P(O3) from the time series of ozone mixing ratios. The MOPS and calculated predictions of average P(O3) $\sim$ 20 ppbv/h in the morning seem quite high given the relatively moderate daily rise in O3 mixing ratios on most days in Figure 1. That makes it possible that the relatively low predictions of P(O3) in the model are closer to reality. Specific comments: p. 27525, lines 19-28: The rationale behind the difference in PSS in the MOPS chambers providing a measure of P(O3) should be justified, and key uncertainties noted. p. 27527, lines 15-16: Clarify how the model was constrained. For example, were NO and NO2 both specified? p. 27530, lines 18-21: The logic of this statement is unclear. Photolysis of NO2 to NO just maintains the null cycle if followed by NO+O3 reaction. Minor edits: p. 27522, line 18: change the phrase "holding a debate" p. 27522, line 24: correct "designed" to "designated" various lines: Define NOx, VOCs, RO2, and HOx on first use p. 27523, lines 20-21: unjustified to claim that MOPS provides basis for designing regulations and controls, or contributes to the "efficacy of air pollution management" (p. 27537, line 3). Strategy development requires understanding of ambient responsiveness to control measures, which is not provided by MOPS. p. 27253, lines 25 and 29: clarify that you're referring to tropospheric ozone production p. 27525, line

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24: define "sample" and reference" chambers p. 27529, line 15: change "considerably" to "considerable" Fig 1: Is the y-axis scale also for O3 (ppb)?