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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Chemical Transport Model Simulations of Organic Aerosol in Southern California: Model Evaluation and Gasoline and Diesel Source Contributions" *by* Shantanu H. Jathar et al.

Anonymous Referee #1

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The manuscript by Jathar et al. summarizes results from a new modeling approach to characterize SOA formation in southern California. In this study, the volatility basis set along with updated VOC emission profiles and speciations are used in a chemical transport model to quantify SOA formation in the region and separate the contributions from diesel and gasoline sources. The topic is of high relevance given the interest in recent years in investigating SOA formation in urban areas from IVOCs and understanding the contribution of gasoline and diesel sources to SOA. The paper is well-written and organized, and the figures are of high quality. The only flaw in the structure of the manuscript is that the conclusion section is missing. I have a few technical and

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minor editorial comments listed below. Once these concerns are addressed, I support publishing the manuscript.

Technical comments: P4, L22: Are the emission profiles of non-road gasoline sources assumed to be the same as on-road vehicles tested on the UC cycle? Also how are their emissions rates defined? Measurements in European cities in recent years have shown high amounts of SOA are formed from small (2- and 4-stroke gasoline engines). How are such emissions characterized for S. Cal?

P5, L 38-39: Why are biogenic SOA not aged similarly to other species?

P6, L2-3: It is mentioned that 10% of POA is shifted to SOA; is that because of POA aging or in addition to that?

Sec 3.1 and Fig 2: How are "episodes" defined? Also are these data limited to the boundary layer? Please clarify.

P6, L29: Isn't OM/OC ratio of 1.6 too low for SOA dominated regions and too high for POA dominated regions? Could uncertainties in this ratio also affect the comparison in Fig. 3a?

Fig 4. It is surprising the total POA from gasoline is more than that of diesel. Based on fuel-based emission factors of POA (Ban Weiss et al. 2008) and fuel use data, one expects the reverse. Are the emission factors used in this study significantly different than Ban Weiss et al.?

P8, L20: Since absolute amounts of the predicted IVOCs are in fact half of the measured values, why the emission ratios of IVOCs were not adjusted to match the measurements? Couldn't this also be the reason why the predicted OA is so much lower than the HR-AMS values in Pasadena (P7, L40)? Related to this, with the additional amounts of IVOCs in runs summarized in Figure 6a, how does the OA comparison with the Pasadena measurements look?

P11, L10-13: Is the low-NOx regime expected to be present downwind, i.e., in Inland

Empire or just over the ocean? Measurements over the land in S. Cal usually show NO levels at ∼100s of pptv which is much higher than the threshold for low-NOx conditions, given typical HO2 mixing ratios. Because of this, I don't think low-NOx conditions are common in S. Cal and therefore applying only the high-NOx SOA yields to VOCs/IVOCs should not change total SOA formation.

Minor comments: P3, line 9 and P5, L31 miss references P6, L28: Mention that HR-AMS measurements were made in Pasadena. Fig 3: Define in the caption what f.b. and f.e refer to. P7, L27-28: remove "in" from "note that in the VBS model. . .." and remove () around Woody et al. P7, L31-33: sentence is unclear and needs to be rephrased P9, L 19: add on: ". . .limited effect ON the SOA burden ".

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