Dear Reviewer,

Thank you for the comments to help improve the quality of the paper. We have revised the manuscript to address your comments and a detailed response to each comment is provided in this file.

The comments are in regular font, the responses are in red, and the changes in the manuscript are in blue.

Anonymous Referee #1

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The authors discussed the concentrations and sources of primary and secondary organic aerosols in PM0.1 over California for 2000-2008 using the source-oriented UCD/CIT model. The article is overall well-written. I will suggest the paper accepted by ACP after the authors address my following questions/suggestions:

1. SOA module

The SOA module used in this study is based on the two-product method. Different SOA formation treatments could result in different results. It would be meaningful if an alternate SOA module (e.g., VBS) is applied in the future study of POA and SOA.

Responses: Thanks for the comment. Atmospheric SOA formation pathways and processes are the focus of intense research which leads to continuous evolution in our understanding about accurate SOA modeling approaches. Part of our research team has recently developed a new statistical oxidation model to simulate SOA (Cappa et al. 2013; Cappa et al. 2016; Jathar et al. 2016) that is able to study the effects of multi-generational chemistry, evaporation of SOA fragments, wall loss effects, etc. Many of these issues are also the focus of VBS modeling efforts, and so we feel that we are capturing the essence of the scientific questions even if we have not directly applied the VBS model itself. The results of the statistical oxidation modeling studies are described in Section 3.3 of the manuscript. Future applications of long-term modeling in California will improve on the 2-product model to capture the latest scientific findings, but this issue is beyond the scope of the current paper. As a result, no changes were made in the current manuscript based on this comment.

2. OC/Mass ratios

The authors discussed the underpredictions of OC/Mass ratios shown in Figure 2, which could be due to the overestimation of dust emissions. Are dust emissions affected by wind speed from WRF? Did the authors evaluate the meteorology provided by WRF? What about seasalt, since some sites are along the coast? Although most of seasalt are coarse particles, they may contribute a little bit to PM2.5?

Responses: In reality the dust emissions are affected by the wind speed and soil moisture. However, the dust emissions in our study were developed by California Air Resources Board based on average wind speeds. Therefore the WRF wind speed was not used in the dust emissions. This point has been clarified on lines 234-236 of the revised manuscript.

The WRF predictions have been evaluated against meteorological observations. The results were described in the Part I paper. We have clarified the sentence on lines 164-165 of the revised manuscript to clarify this point.

The seasalt emissions were included in the simulations. The seasalt emissions were calculated online using the WRF wind speed. The detailed description of the emissions was also provided in the Part I paper. This point has been clarified on lines 154-155 of the revised manuscript.

Some other comments:

1. Page 6, line 78, UCD/CIT has been defined in page 4 line 52

Responses: Corrected.

2. Page 8, line 136-137, do you mean BENZ (i.e., ABNZ1_X1, ABNZ1_X2, ABNZ2_X1, and ABNZ2_X2)?

Responses: Yes, we corrected the sentence.

3. Page 9, line 155, change “meteorology fields” to “meteorological fields”

Responses: Changed.

4. Page 13, line 246, “Condensation of SOA”, do you mean the condensation of volatile VOCs?

Responses: We changed the sentence to “Condensation of the semi-volatile products to form SOA”.

5. Page 15, line 277, “some important sources”, could you please provide some specific sources that for Riverside case?

Responses: We speculate the missing sources are mostly likely some area sources, such as residential and/or agricultural waste emissions. With no solid evidence, we are not sure what exactly the sources are, so we don’t want to give specific names in the manuscript to avoid providing misleading information to readers. No changes were made for this comment.

6. Page 15, line 287, do you mean less POA converted to SOA in ultrafine size range?

Responses: No, we mean PM0.1 OA is more of POA and less of SOA, compared to PM2.5 OA. The sentence has been clarified in the revised manuscript on line 295.

7. Page 16, line 311, You may want to switch the order of supplementary figures.

Responses: Accepted. The order of figures in the supplemental materials was changed to follow when they are mentioned in the paper.

8. Page 31, Figure 4b, why are PM0.1 SOA/TOA ratios very high over the southeastern corner? Is that partly due to boundary conditions?

Responses: That is because very low POA concentrations (as can be seen in Figure 7), i.e., very low anthropogenic emissions, in that region. No changes were made for this comment.

References:

Cappa, C. D., X. Zhang, C. L. Loza, J. S. Craven, L. D. Yee, and J. H. Seinfeld, 2013: Application of the Statistical Oxidation Model (SOM) to Secondary Organic Aerosol formation from photooxidation of

C-12 alkanes. *Atmos Chem Phys*, **13,** 1591-1606.
Cappa, C. D., S. H. Jathar, M. J. Kleeman, K. S. Docherty, J. L. Jimenez, J. H. Seinfeld, and A. S. Wexler, 2016: Simulating secondary organic aerosol in a regional air quality model using the statistical oxidation model - Part 2: Assessing the influence of vapor wall losses. *Atmos Chem Phys*, **16,** 3041-3059.
Jathar, S. H., C. D. Cappa, A. S. Wexler, J. H. Seinfeld, and M. J. Kleeman, 2016: Simulating secondary organic aerosol in a regional air quality model using the statistical oxidation model - Part 1: Assessing the influence of constrained multi-generational ageing. *Atmos Chem Phys*, **16,** 2309-2322.