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Interactive comment on "Secondary organic aerosol formation from biomass burning emissions" *by* Christopher Y. Lim et al.

Anonymous Referee #2

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The Lim et al. manuscripts reports on SOA formation and aging in a series of chamber studies, conducted as part of FIREX. The chamber experiments were conducted at the Fire Sciences Laboratory, and biomass burning emissions provided the precursors for SOA formation; aging proceeded largely by exposure to OH. Gases and particles were characterized throughout the batch-mode experiments using a suite of instrumentation. Mass loadings, OA enhancement ratios, aging time (OH only), and bulk elemental composition are also reported for each experiment. One of the most significant findings of this work is that that AMS collection efficiencies may have been significantly underestimated in a wide range of published works, which affects interpretation of results and ultimately our understanding of SOA formation as gained from those works. The authors report that while OA enhancement ratios initially appeared

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Discussion paper

to be significantly higher than those reported in other publications, once aging time and collection efficiency were taken into account, the results were broadly consistent. The authors present a parameterization for SOA formation from biomass burning as a function of total NMOG and aging time that may prove useful when insufficient data regarding NMOG speciation and associated SOA yields are available. The paper is very well written, and the methodologies and results are clearly presented. It is an interesting and thought provoking paper, and likely will create much discussion and future research within the atmospheric community. Some specific comments and questions are provided below. Only minor edits are suggested before publication.

Scientific On p. 4 potential loses in the sampling line are discussed, and a figure is provided in the supplement illustrating the difference between gaseous emissions sampled in the stack directly, to those in the community inlet, binned by saturation concentration. The binned comparison does not suggest a systematic loss in the community inlet either across bins or as a function of volatility. However, there is a significant difference in one of the bins (C* 107-108). Is this difference well understood (e.g., likely due to a specific class of compounds)? And, how might this difference affect the results and analysis?

On p. 7, line 4 the authors state that the dilution factor prior to oxidation influenced observed initial aerosol mass and reference Table 1 in the supplement. It is not clear how the dilution factor is represented in the table. Is it a function of sampling time? This needs a bit more clarification/explanation.

The relationship between reported enhancement ratios in this work with previously published ratios is discussed on p. 7, first paragraph. The authors suggest that once aging and collection efficiency are taken into account, the results are broadly consistent with other results, and not overestimates. However, based on Fig. S6 panel b, where OA enhancement ratios are plotted as a function of aging time, the reported enhancements are still a factor of 2+ higher than Ortega et al. Are the Lim et al. enhancement ratios in the right panel actually for CE =1? They seem significantly higher than what is pre-

sented in the left panel (and it is assumed that both are for CE = 1). In addition, the average appears to be ~3, which is the CE corrected-value reported in the main text.

In the discussion of correlation of measured SOA formation with different parameters (pp. 8-9), it is suggested that the reasonable correlations exist between SOA and total NMOG, with some relationship to POA, and when corrected for aging time. On p. 9, line 2, the authors state that the NMOG correlated well with POA (not shown), and thus POA is also well correlated with SOA. The correlation with POA, at least based on r2 values, actually appears to be better than the correlation with total NMOG. If the goal is a simplified parameterization, why not just use POA?

In the discussion of the results presented in Figs. 4 and 5, it is suggested that the relative insensitivity of SOA to NMOG m/z cutoff below ~m/z 135 is either due to the contribution of higher volatility/lower molecular weight species, or other compounds which are not measured by the PTRMS, but correlated with these smaller NMOG molecules. It seems like there may be sufficient data available to look at the ratio of likely SOA precursors to total NMOG to test this hypothesis. Also, while it is not discussed, to the observed carbon yields themselves lend some insight to the likely precursors?

Editorial p. 8, line 20: Recommend changing "common SOA precursors" to "monoterpenes", since that is the only precursors show in S.7. p.9, line 23: It would be interesting to see the correlation plots of the two different temperature NMOG factors with SOA. Could these be added to the supplement?