

Interactive comment on “Formation of secondary organic aerosols from gas–phase emissions of heated cooking oils” by Tengyu Liu et al.

Anonymous Referee #1

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This paper shows the production of SOA from oil vapours associated with cooking using a PAM chamber, which is of strong relevance for the modelling and source apportionment of urban particulate matter. While questions persist regarding how atmospherically representative PAM yields are, this paper focuses on the qualitative mass spectral features, comparisons of different oils and speculations on chemical mechanisms, which will no doubt prove highly useful for further studies, so is well within scope for ACP and represents a decent contribution to the science. The paper is generally well written and I recommend publication after consideration of minor comments.

General comments:

I find myself at odds with the conclusion that some of the ambient reported COA in other studies could be secondary in nature because the SOA spectrum shows incon-

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sistencies with the key features normally reported. Specifically, the $m/z=41$ peak is relatively low compared to $m/z=43$ and the $m/z=44$ peak is much higher. By comparison, the POA spectrum is very consistent with the literature. I should note that R2 is not a good metric to compare spectra in the context of discussing PMF outputs because these spectra are derived according to precision-weighted variance, so factors tend to be dictated by the ‘strong’ variables, i.e. a subset of peaks with the highest signal-to-noise ratios, so it is these features that should really be compared. Furthermore, previously reported diurnal profiles of COA almost universally show a maximum in late evening, which is after dusk in many cases, where oxidation through the OH pathway is not plausible (while ozone and nitrate oxidation of unsaturated bonds will occur, this is not what is investigated here). I think it far more likely that any cooking SOA contribution will be included with one of the OOA factors in a typical urban PMF study. I would recommend that the authors adjust their conclusions accordingly.

All the oils were heated to 220°C. While I can recognise the value in experimental consistency, this is possibly beyond the smoke point of some of the oils used here (depending on their grade). Can the authors verify that the oils did not give off visible smoke during the experiments? If they did, this should be added as an important caveat because this would fundamentally alter the emissions profiles.

Specific comments:

Line 67: Allan et al. (2010) did not strictly find the conclusion that cooking oils were a major contribution; this was a speculative explanation for trends within the data. More conventional marker-based studies (e.g. using chromatography) are probably more of more value to support this notion.

Line 111: Was the Teflon transfer line heated? If not, some condensation onto the tube may have occurred. The authors should comment on whether they consider this to be an issue.

Line 125: How were the flow rate and dilution ratio measured/estimated?

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Line 163: PR has a number of issues, not least of which is the fact that the oxidant concentrations and simulated timescales in PAM are far in excess of those likely experienced in urban atmospheric and chamber studies. While I am not questioning its usefulness, I would put some caveats in concerning its quantitative merit.

Line 222: It should be noted that the S- and IVOCs will most likely evaporate from the filter if they are not at gas phase saturation, although I concede that given that the aerosol is cooling, it is most likely that they are at saturation in this case. However, an experiment where filtration occurs after dilution may produce different results, so this should be noted.

Line 260: The statement about vegetable oil not being used in commercial kitchens needs further explanation because the vegetable oils studied here are almost universal in many countries. Are the authors referring to Hong Kong specifically? What other oils are in use and why weren't these studied here?

Line 284: Statements are made concerning vapour pressures, but it is all very nonspecific and hand-waving. Some typical saturation vapour pressures (either measured or predicted) for some example compounds should be given.

Line 336: The conclusion regarding animal fat yields should be treated as a speculative inference because while a correlation with omega-6 is found here, its role as a determining factor is not conclusively proven.

Line 343: The 'gas phase' measurements referred to should be specified, e.g. GC-MS.

Supplement: I would argue that both graphs in the supplementary material are of sufficient interest to warrant inclusion in the main article.

Technical comments:

The term 'canola' will not be familiar to all readers worldwide. The alternative name 'rapeseed' should be offered somewhere, by way of explanation.

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Line 124: The model of the Dekati dilutor should be given.

Line 139: More information on the diffusion drier must be given. I would note that membranes such as Naphion are known to remove OVOCs. Do the authors have a rough idea what RH the AMS sampled at?

Line 196: It should be noted that the fatty acid vapours studied here are not the specific fats present in the raw oils, but thermal breakdown products of fat lipids.

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