Review of *Characteristics and degradation of organic aerosols from cooking sources based on hourly observation of organic molecular markers in urban environment* by Rui Li et al.

This manuscript presents measurements of fatty acids and their oxidation products, which are used as a proxy for cooking emissions, at an urban site in Changzhou, China. Aerosol gas chromotography-mass spectrometry (TAG) was used to detect concentrations of these compounds every two hours during three monitoring periods in January – March 2021. The authors explore changes in concentration throughout the diurnal cycle and under the influence of different air masses. Estimates of the effective rate constant are used to suggest that the key night-time oxidant for these fatty acids is ozone.

This paper's specific focus on cooking emissions provides a valuable contribution towards a growing literature base on the chemical composition of urban aerosol. The overall goal of the paper – to quantify the contribution of cooking aerosol and examine its influence under different regimes – is important. However, I found the conclusions in some parts of the paper to be unclear or not fully supported by the data presented. I would also recommend a check through the paper for typos and to improve the clarity of the writing in some places. I would suggest that this paper is suitable for publication in ACP, subject to some edits. Additional comments are provided below.

Major comments

1. Line 95: This would benefit from more information about the process of how sample collection takes place with TAG.
2. Lines 181-184 and Fig. 3: The authors discuss the influence of boundary layer changes on the concentration of fatty acids, suggesting that this is the reason there is no lunchtime increase in fatty acids. I agree that the diurnal pattern shown in Fig. 3 is very characteristic of a parameter influenced by boundary layer dynamics. However, this makes it difficult to discern which changes are related to boundary layer changing and which to fresh emissions. I might suggest that plotting the diurnal of each compound as a fraction of PM2.5 or OC would give more insight into the chemical changes. In fact, this can be seen later in Fig. 12, where there is a more obvious midday peak in the fractional contribution of the cooking factor than in the absolute concentrations.
3. The authors assume that all fatty acids observed are the result of cooking emissions. This may be a reasonable assumption in this environment; however, I would like to see more justification for this and discussion of other potential sources. For example, palmitic, stearic and oleic acids can all be associated with biomass burning (eg Bertrand et al., 2018; Fujii et al., 2015); and palmitic and stearic acids are released from marine biota in the ocean surface (eg Bikkina et al., 2019), which could be an influence here in CL#2 and CL#4. It seems to me that fatty acids being released at the ocean surface and then oxidised during transport is a more likely explanation for the chemical composition in CL#4.
4. Lines 190-193 and Fig. 4a: If I have understood correctly, palmitic acid/stearic acid (on the x-axis) is taken to be a rough indicator of the source of the emissions. If this is the case, it is difficult for the reader to interpret the x-axis here. Could it be more illustrative to label the ratios for a selection of sources on the x-axis? Would you expect to see a relationship between the y-axis and the x-axis?
5. Section 3.3 and Fig. 7: From Fig. 7, there does not appear to be a relationship at all between the concentration of fatty acids and that of PM2.5, which makes it difficult to discern the key

scientific conclusion from this analysis. In the text, the authors point to a link between the air mass cluster and the behaviour of TFAs vs PM2.5. I would suggest clarifying the main conclusion from this section and ensuring that it is clearly supported by the figure.

6. Fig. 8: The correlations shown here appear to be quite weak; conclusions taken from them would be strengthened with further statistical analysis. I might suggest:
   a. Considering a log-log axis,
   b. Carrying out a t-test in each case to establish whether the correlations are statistically significant,
   c. Stating correlation co-efficients in the plots.
7. The purpose of the correlation plots (Fig. 8b-d) is not clearly explained in the text.

Minor comments

1. Line 30: The meaning of the term ‘rising period’ is unclear.
2. Line 56-8: Quantifying the concentration of fatty acids will not reduce their impact, though it may inform policy. The phrasing needs to be reconsidered here.
3. Sometimes ‘fatty acids’ is written out fully (eg line 176), and sometimes it’s abbreviated to ‘FA’ (line 174). I would recommend using the same approach each time.
4. Fig. 4b: I don’t completely follow where the source profiles come from here. It would be good to outline the origin of these more clearly in the text.
5. Fig. 5: This figure shows the chemical influences of the different clusters very clearly. However, I would recommend rearranging slightly so that the pie charts do not block the back trajectories, and so that the text is slightly larger in some places. It is currently very difficult to read the percentages on CL#1 and CL#4.
6. Lines 235-239: I might suggest rephrasing both of these reasons, as they are currently quite difficult to understand. What is meant by the “CL#4 air mass [being] under the influence of CL#4”, for example?
7. Lines 281-2: It could be helpful to remind the reader what the conclusion referenced here was.
8. Line 351: What does Ox refer to, in this context? How was it measured/observed?
9. Fig. 12: It’s difficult to interpret the relative size of sections on a 3D pie chart. I would strongly advise making this 2D.
10. Lines 416-8: The authors state what was observed; it would be good to state the conclusions drawn from these observations as well.
11. Line 422: It would help here to remind the reader that CL#3 was the slower-moving, more local cluster. The same goes for the other clusters when they are mentioned in the conclusion
12. Line 425-6: Why would this be the case? Is it because these clusters provide more ozone?
13. Line 430-1: The statement here, that “O3 and NO3 are the main nighttime oxidants for uFAs in Changzhou City” contradicts the statement in Lines 373-4 that “O3 was the most likely oxidant for the nighttime uFAs oxidation in the urban area of Changzhou”.

Bertrand, A. et al., Atmos. Chem. Phys., 2018, https://doi.org/10.5194/acp-18-7607-2018.

Bikkina, P. et al., ACS Earth Space Chem., 2019, https://doi.org/10.1021/acsearthspacechem.8b00161.

Fujii, Y. et al., Atmos. Environ., 2015, https://doi.org/10.1016/j.atmosenv.2015.03.042.