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Interactive comment on "Chemical Characterization of Secondary Organic Aerosol at a Rural Site in the Southeastern U.S.: Insights from Simultaneous HR-ToF-AMS and FIGAERO-CIMS Measurements" *by* Yunle Chen et al.

Anonymous Referee #2

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General comments

This paper describes the simultaneous use of an AMS and a FIGAERO-CIMS to measure SOA at a rural site in the southeastern USA. The chemical composition of the SOA detected is analysed via both techniques in terms of general chemical trends and where possible for specific chemical species that are known to be markers for SOA produced via different pathways. PMF based analysis of the AMS data and, most in-

terestingly, of the FIGAERO-CIMS data is used to identify and analyse factors that correspond to SOA produced from different starting VOCs and via different chemical pathways. Correlations between AMS and FIGAERO-CIMS factors are also explored.

This study addresses the topical scientific question of SOA formation mechanisms utilising the fairly novel application of PMF to FIGAERO-CIMS data in order to reach conclusions about the origin of SOA in the studied region in a well structured and clear manner. On this basis I recommend publication after a number of minor issues are addressed which I discuss below.

Specific comments

Page 4 Line 24: A little more detail on the influence on biogenic VOC emissions here would be welcome.

Page 6 Lines 10 - 11: What uniform sensitivity was used? Also how was the sensitivity arrived at? More details are needed here.

Page 6 Lines 16 - 19: I more detailed explanation of the validity and reasoning behind the method used here is needed as (in agreement with the other reviewers comments) I think this section is currently misleading/confusing.

Page 7 Line 13 and 18: The error on the alpha pinene measurement is so large as to make "negative" concentrations possible and this is even more apparent for the NO measurement. As the other reviewer has stated are such values ok?

Page 9 Line 7 - 10: When discussing the FIGAERO-CIMS data in Figure 1 I think the others are overstating when they say it is in "agreement" with the AMS data. Particularly in Figure 1A and 1C the random variation in the signal is larger than any perceived trend. Echoing comments from the other reviewer why is this so noisy?

Page 9 Line 25 - 27: In a similar vein when discussing Figure 2B I think the seemingly random variation in the signal at night makes interpretation problematic. Why is there so much variation? Is it because the overall signal is very low? Would like to see the

"actual" diurnals with the real signal rather than just the proportion diurnals to assess this further.

Page 16 Line 21 - 24: I would like to know more about how the quoted TMAX values were obtained? Looking at Figure S10 (and particularly Figure S10B) it is hard to see how those precise value were chosen, with a seemingly more obvious peak at lower T not identified?

Technical comments

The acronym SOA should be defined where it is first used (like all the others are) rather than later on as it is currently.

Page 10 Line 3: Convert not covert.

Page 20 Line 12: Prevalent not prevalence.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-126, 2020.