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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Urban influence on the concentration and composition of submicron particulate matter in central Amazonia" *by* Suzane S. de Sá et al.

Anonymous Referee #2

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his manuscript provides an overview of particle mass and chemical composition, with a focus on organic species in the Amazon. The authors strive to understand the anthropogenic contribution(s) to mass and influence on speciation and approach this with 2 different statistical approaches applied to online measurements. The work is interesting but seems premature. The paper relies on and cites several manuscripts that are "in preparation" to justify some arguments and conclusions and this is problematic. For example, comparison among different data sets (and presented in Figure 1) includes data not previously published, nor fully explained here.: data from ATTO sampling location "T0a-2015" is from Carbone et al., in preparation and "T2-2014" is from IOP1 Brito in preparation. Organic mass variability in relation to meteorology seems to be

an important finding and necessary to the arguments in this manuscript but Cirino et al. 'in prep' is the provided proof and readers are not left with sufficient information to understand the reasonableness of the argument.

The authors also cite de Sá et al 'in prep' to explain why a certain analysis is beyond the scope of this paper and not presented here and explain that the analysis is currently underway (e.g., biomass burning influence (presumably screened here?)) will be discussed in the literature later and I think that is ok.

Many journals would not even accept 'in prep' References at all. To use such References for conclusions seems unreasonable to me. Prior to acceptance for publication I think the 'prior' work must first be published or properly backed up here.

specific comments: Line 19/20: The choice to cite Weber et al., 2007 and Goldstein et al., 2009 here is curious. Weber et al. state in that paper: "Although NOx may be another precursor that could be influencing this system, NOx-WSOC . . . was weakly correlated" The R2 is <0.2 I acknowledge time scales for complex chemistry matter and correlation for instantaneous values can be low even though there is a dependence, however the work by Weber does not provide support for NOx or SO2 dependence as suggested by the authors here. The work by Weber et al does demonstrate a link to CO. The Goldstein analysis for particles is limited to satellite-AOT. Seasonal and spatial patterns have found these AOT observations are not due to organic fine particle mass (Ford and Heald, ACP 2013; Nguyen et al. GRL, 2016) The authors cite Xu et al., 2015 later in the manuscript and that would be a good citation here. Because the authors are talking in the manuscript here about the Southeast US, citing recent findings from the Southeast field campaigns (e.g., SOAS as in the Xu paper) and making a link with the context of those field campaigns would improve the paper.

I have no idea what "V" and "W" mode mean. The authors should provide an explanation if the distinction is important as they suggest.

Line 165: When talking about Figure 1 the authors state 'concentrations at the T2 site were more than three times higher on average' All of the presented averages in Figure 1b overlap within the uncertainty. Can it be stated that there is statistically significance to the difference? Figure 2 suggests a factor of 2, not 3.

Figure 2 caption: Please correct the text: "Error! Reference source not found." The panels of Figure 2 have different y-axes and this should be mentioned explicitly.

Figure 4 is nice, but it's hard to read and digest.

Figure 9 is excellent!

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