

## ***Interactive comment on “Particle Size Dependence of Biogenic Secondary Organic Aerosol Molecular Composition” by Peijun Tu and Murray V. Johnston***

**Anonymous Referee #2**

Received and published: 7 April 2017

“Particle size dependence of biogenic secondary organic aerosol molecular composition” by Tu and Johnston describes a study in which particles generated from ozone-initiated oxidation of  $\beta$ -pinene are chemically analyzed using offline and online analysis for both size-resolved and polydispersed samples. Understanding size-resolved chemical composition of nanometer-sized particles has important implications for quantifying and, ultimately, modeling new particle growth following nucleation. The current study mostly confirms prior studies that show that condensation of low volatility organics are important for smaller particles, whereas oligomer formation is important for larger particles. Even though it is not clear that a lot of new ground was broken in this study, it appears to be nicely done and the writing is clear and concise. I have only a few

C1

comments that I wish for the authors to address prior to publication, indexed below according to page/line numbers (I will include minor editorial suggestions along with slightly more substantive issues):

1/26: “small ultrafine” – I suggest removing “small”

2/12: I find the distinctions that the authors make between condensation and partitioning somewhat confusing. It has always been my practice to use “partitioning” to describe the most generic process of gases going into and out of particles (e.g., “gas-particle partitioning”). Partitioning can be further broken down into nonreactive- and reactive-partitioning, the former of what I would define as condensation. For the latter, I would include particle phase oligomer formation as well as salt formation as representative mechanisms. The author have their own definitions for these terms; there is a reference to Pankow’s 1994 manuscript so I wonder if this is a distinction that is made therein? If so, it might be of service to others to perhaps be clearer and/or consider what I would think of as more common uses of these terms.

3/9: This sentence implies that the studies cited in the previous paragraph all point to accretion product formation in the gas phase, however this is not clear in that discussion that this was in fact the main conclusions of some of the studies such as that of Kidd et al. Please clarify.

3/24: It may help the reader appreciate this better if the actual SA:Volume ratios are stated for particles at these diameters.

4/9: I am curious as to why the authors chose only to perform these experiments under dry conditions? Surely a more atmospherically relevant RH would be closer to 40%. Can the authors comment on whether or not they feel the RH has an impact on the results of this study?

7/13: This section opens up with a statement regarding the importance of considering the gas phase products of  $\beta$ -pinene oxidation; however, nowhere are measurements

C2

of gas phase products actually presented that I can discern. Most of the discussion seems to be about size-resolved particle composition. Consider modifying this opening sentence to be more representative of the subject of the section (i.e., the section title itself).

10/5: This section presents O/C and OSc vs. Carbon number as a function of mass loading, however the results are not very satisfying because wrapped up in this is the effect of increased particle size that accompanies increased mass loading. In fact, particle size may be the dominating factor leading to this “effect” of mass loading. I am not sure what to suggest here . . . clearly the title of this section is somewhat deceptive because this is really not a study of the impact of mass loading. Hopefully the authors can modify this section to actually say something about the effect of mass loading, as I think this would be truly interesting.

---

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2017-53, 2017.