

Interactive comment on “Temperature Effects on Optical Properties and Chemical Composition of Secondary Organic Aerosol Derived from *n*-Dodecane” by Junling Li et al.

Anonymous Referee #1

Received and published: 8 March 2020

Li et al. presented a study that examined the effects of temperature on the optical properties and chemical composition of secondary organic aerosols formed from the OH photooxidation of *n*-dodecane. The authors found that oligomers were formed at low temperatures, and these oligomers resulted in higher RI values being measured. This paper is potentially useful to the SOA community. However, there are some important issues that the authors need to address before the manuscript can be considered for publication.

Major comments: 1. Why were experiments conducted under dry conditions? 2. Table 1 showed used of 43 ppb at low temperature vs. 58 ppb at high temperature. Why

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wasn't the same amount of *n*-dodecane used? 3. Do the authors know how the use of different temperatures will affect the loss rates of particles to the chamber walls? Is there a possibility that the observations of the SOA mass, composition and optical properties made by the authors can be explained partly by differences of particle wall loss rates at different temperatures? 4. In page 5 line 144, the authors state that “During this period, the optical properties of the particles tend to be stable and will not change much.” This sentence is ambiguous and needs to be clarified. What optical property is the authors referring to? RI value? Or are they referring to the absorption spectra? 5. Explain the rationale behind tracking the absorption at 532 and 375 nm. 6. The authors did not use seed aerosols in this study to promote gas-to-aerosol partitioning. Hence, I expect substantial vapor wall loss in these experiments, and the extent of vapor wall loss is likely to be different at 5 C vs. 25 C. Is it possible that the authors are not detecting some products (due to their loss to the chamber walls) that can contribute to SOA optical properties? 7. In section 3.5, the authors tried to relate their results to observations made during winter haze episodes in China. I advise the authors to be more circumspect in the extrapolation of their results to ambient observations since NO_x concentrations are likely substantial during winter haze episodes in China. The authors performed a study under low-NO_x conditions. Under high NO_x conditions, I expect the reaction mechanism of *n*-dodecane OH photooxidation to be different. For example, more fragmentation will likely happen, which will result in the formation of more volatile products. If this is the case for both 5 C and 25 C conditions, there may not be significant differences between SOA composition and their RI values under high NO_x conditions, which would imply that temperature does not play a big role in the DRF of SOA formed under areas with significant NO_x concentrations, like China.

Minor comments: 8. Inconsistent tenses. The authors switch between using past and present tenses in some parts of the manuscript. Please correct this.

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