

Interactive comment on “Secondary Organic Aerosol from Atmospheric Photooxidation of Indole” by Julia Montoya et al.

Anonymous Referee #1

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This manuscript describes a laboratory study on the photo-oxidation of indole under low-NO_x conditions. Secondary organic aerosols (SOA) were chemically characterized to determine the importance of indole chemistry in the formation of brown carbon (BrC) constituents. State of art analytical techniques (high-resolution mass spectrometers) used in this study, provide novel and important insights into the understanding of the formation of BrC in the atmosphere. In addition, the authors have evaluated the importance of their findings using a regional model and have highlighted that oxidation of indole could have an important role in SOA formation as well as BrC compounds. Since these results help to provide accurate atmospheric chemistry models for the oxidation of BVOC, they are an important contribution to the literature. While the results are interesting and are appropriate for Atmospheric Chemistry and Physics, few clarifications (c.f. comments below) should be provided and would benefit from clarifying

C1

revisions.

Page 2. Lines 7-15: The authors should consider adding some information on the emissions of indole in the atmosphere and compare them with the emissions of other BVOCs. In addition, what is the contribution of the human activities (e.g. agriculture, pharmaceutical application) in the global emission of indole?

Page 4. Line 5: Could the authors provide more information on the settings of the PTR as well as the time and mass resolutions?

Line 7. The authors mentioned Page 7, line 7 that “The particles had a geometric mean diameter of approximately 0.3 μm ”. What was the mean diameter of the seed aerosol?

Line 9. Hallquist et al. is not an appropriate reference. It is a review and they have not determined any aerosol density. Please provide a better reference(s).

The authors decided to use a density of 1.2 g cm⁻³, could they explain why? Previous studies have reported a density of \sim 1.4 g cm⁻³ for SOA generated from the oxidation of mono- and polyaromatic compounds, such as naphthalene (Ng et al., 2007; Chan et al., 2009; Chen et al., 2016). It is worth noting that such compounds could form oligomers as proposed in the manuscript (Healy et al., 2012). Could the authors further support their choice and discuss the potential impact of using a density of 1.4 g cm⁻³ in their model?

The authors haven't discussed the potential wall losses of organic vapors. Have they considered them in the determination of the SOA yields? If not, by looking at the decay of the main products they should be able to provide an estimation.

Lines 14-16: The authors mentioned page 4 lines 2-3: “In some experiments, mixing was not complete by the time the lamps were turned on as evidenced by the measured indole concentrations continuing to increase”. How did they determine the SOA yields in such experiments?

Page 7

C2

Lines 27-28: The time series of the species presented in Figure S2.5 is not clear. The products are continuously produced over the course of the experiments (c.f. formic acid) disregarding the presence of OH radicals (i.e. lights On or Off). The authors should further discuss the time evolution of the identified products.

In addition, the authors proposed that the oxidation of indole form 3-oxyindole. But according to Figure S2.4, this product is an impurity from the indole and it is not formed during the oxidation process (no increase after the lamps were turned on). The decay of 3-oxydinole is much slower than the decay of indole. Therefore, if the 3-oxydinole was really formed from the oxidation of indole it should have shown up, such as 2-formylformanilide. Please clarify.

Page 10

Lines 19-22: It is not clear if the authors claim that the formation of the oligomers (BrC compounds) occur in the gas phase. The authors used a reference that described similar oxidation process occurring in the liquid phase. If the authors expect that the formation of oligomers happened in the gas phase, they need to provide further evidence and in general better discuss the formation of the identified products. I would suspect this reaction to occur in the particle phase rather than in the gas phase. Have the authors performed any experiments at different RH and/or without seed aerosols? Further experiments are needed if they want to conclude that.

Lines 23-24: With the data presented in Figures S2 it seems difficult to propose that 3-oxyindole is directly formed from the oxidation of indole (c.f. previous comments). Moreover, the authors should consider the mechanism proposed by Healy et al. 2012. In this previous study, they have reported the formation of dimers and oligomers from the photolysis of nitro-naphthalene through gas-phase processes. Could such chemical pathways contribute to the formation of some compounds identified in this manuscript?

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C3