

## ***Interactive comment on “Atmospheric Chemistry, Sources, and Sinks of Carbon Suboxide, C<sub>3</sub>O<sub>2</sub>” by Stephan Keßel et al.***

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This paper represents a comprehensive study on the fate of C<sub>3</sub>O<sub>2</sub> in the atmosphere. The research has been carried out to a very high standard and is generally clearly presented. There are two areas where I feel some additional input would be useful to enhance the impact of the paper and then there are a number of minor technical points and suggested corrections.

1. Ambient and PTR measurements - The high resolution spectra clearly shows the differentiation between species of protonated mass 69. Is there any possibility that the signal could originate from isotopomers of protonated mass 68, for example <sup>13</sup>C or <sup>15</sup>N pyrrole? For the ambient measurements in the Mainz region, the only data shown is the spectrum in Fig 1, which I believe was collected over a hour period. Presumably

C1

other data were collected - was C<sub>3</sub>O<sub>2</sub> always present? Was there any evidence of a diurnal cycle or any variability in the C<sub>3</sub>O<sub>2</sub> signal. Perhaps a time series could be included in the supplementary. It would be helpful to state the isoprene concentration in the ambient spectrum component of Fig 1 and the ratio of the peak areas.

2. Given the comprehensive nature of the study, it would be good to provide some more definitive conclusions. Given the relatively short lifetime (n.b. can you check the overall lifetime? I get ~3.4 days not 3.2), the low concentrations and the products formed, are further studies required?

Minor points: Abstract - O<sub>3</sub> reaction not measured using relative rate method. Qualify comment on products - the sole carbon products are CO and CO<sub>2</sub>. HO<sub>2</sub> is produced in the OH reaction. p5 line 17. Pathlength was 880 cm on previous page. 880 cm seems to give the value of the cross section reported on p6. Line 30 264.8 nm is presumably lambda max. I don't think this is stated. p6 line 4 multiplier of 10<sup>-19</sup> missing. Line 8 'data' are plural. Line 20 Please include amount of additional NO added, purity etc. Relative rate studies - I was interested in the choice of ethene as a reference compound, the pressure dependence of the reference adds an additional level of complexity to the analysis. Also given the uncertainty in the reference and hence the total rate coefficient, it is not clear that there is any justification in ignoring the low pressure data point. A simple average would give 2.5e-12. Finally the comments on the comparison with Faubel are slightly contradictory - on p7 and 8, the conclusion is that the data are in the same ball park, but not in quantitative agreement and that the relative rate data are likely to be more accurate (I would agree with these conclusions). However, in the conclusions, the data are reported to be in good agreement with Faubel (p14, line 24). p9 line 13 The theoretical predictions of da Silva have been confirmed by experimental studies from our group (Lockhart - JPCA 2013) p10 For the O<sub>3</sub> experiments would it be possible to run with an OH radical trap? p13 line 21 - the model input has a rate coefficient of 2.5e-12, not 2.6 References - Need some subscripts in some references. Fig 1 - More details of sampling times, concentrations in the caption. Fig 2 not sure

C2

where the 'nm' comes from Fig 4. Were the fits constrained to go through the origin? The data looks excellent, so don't expect an unconstrained fit would give a significantly different gradient, but should be checked.

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