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***Interactive comment on* “Gaseous products and Secondary Organic Aerosol formation during long term oxidation of isoprene and methacrolein” by L. Brégonzio-Rozier et al.**

Anonymous Referee #2

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Photooxidation experiments of isoprene and methacrolein in the presence of NO_x have been performed in the CESAM chamber. The paper presents the yields of some small gaseous oxidation products from FTIR and PTRMS measurements. They mostly agree with already published values. In the case of methacrolein oxidation they claim to observe the primary production of acetaldehyde by PTR-MS. This m/z could also be a fragment from higher masses or eventually even be released from the walls. The authors provide a speculative mechanism as to how it could form. According to Figure 5 acetaldehyde is continuously increasing reaching about 15 ppb at the end of the experiment. It is not clear to me why it should not reach levels above the detection

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limit of FTIR (20ppb) in experiments with doubled precursor concentrations. If this was not the case, such an experiment should be performed. The authors also report SOA yields from both precursors. In general they find smaller yields compared to literature. They speculate, that this could be due to the light source (xenon lamps), which produces a light spectrum similar to sun light. Most studies are done using black lights, which lack the emission of longer wavelengths. This speculation may be true or not, but it cannot be drawn based on the analysis given. The authors calculate the SOA yield from the measured aerosol mass at maximum concentration. They consider neither wall loss of particles, dilution flow nor wall losses of gaseous condensable species. It has to be admitted, that the latter point is not considered in most yield determinations either, but it seems to be quite large in this chamber. When the chamber was manually cleaned almost no SOA production occurred anymore. The question is therefore, what contributed to SOA formation in an aged chamber and how much. No blank experiment with seed aerosol was performed determining the blank production of SOA in the chamber. Therefore, there exist enough possible processes to explain the lower yields. The analysis of the experiments needs to be done more carefully. Somehow peculiar is the increase of O/C from AMS measurements in the beginning of the experiment (Figures 4 and 8), which is attributed to “oxidation processing during particle formation” (p22522, line1). It is generally assumed that condensable gases need to be of low volatility. This implies highly oxygenated compounds. How can the authors explain the condensation of compounds with such low O/C? Could this be due to impurities in the chamber (needs blank experiment) or an issue of data analysis? Such an effect has not been observed so far. More often a slight decrease of O/C is seen because of condensation of less volatile and thus less oxygenated species with increasing aerosol mass concentration. It is observed that the use of HONO as OH source leads to higher SOA yields. This is attributed to higher OH concentrations at the beginning of the experiments (page 22524, line 3). However, Figure S4 shows quite the opposite, lower OH for HONO experiments. This paper has some serious deficits and this reviewer cannot recommend publication of this paper.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 22507, 2014.

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