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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Overview of the Focused Isoprene eXperiments at California Institute of Technology (FIXCIT): mechanistic chamber studies on the oxidation of biogenic compounds" *by* T. B. Nguyen et al.

Anonymous Referee #1

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General comments

This manuscript describes the design of atmospheric chamber experiments aimed at better simulating the atmospheric conditions under which biogenic compounds undergo photooxidation. The simultaneous use of many different experimental approaches and synthesized standards is expected to allow for better controlled and measured conditions in the chamber experiments, as well as the opportunity to do cross-calibration experiments for instruments used in both field and chamber studies

with overlapping detection capabilities.

I was genuinely surprised, given that the various experimental conditions that are exquisitely described and tabulated (i.e., Table 2), that there are very few specific results given for these experiments in this manuscript. I can suppose that the authors thought their achievement of chamber conditions that are more representative of the atmosphere is publication worthy, but without directly demonstrating that useful results actually came out of these experiments, I'm not sure what audience will find this paper immediately useful. The "Preliminary results and atmospheric implications" section is not much more than just a teaser (for example, air quality modelers cannot do much with the qualitative information that "the high-NO isoprene hydroxy nitrate yield is closer to the high end of the spectrum," even though this is a provocative result). While the manuscript provides interesting details on the challenges of designing the experiments, it doesn't seem appropriate as an ACP research article. Perhaps it would be appropriate as a technical note in ACP or as an article in another journal?

That being said, I look forward to a full report of the results of this study, as it does appear that many new important insights have been (or will be) gained. Therefore, rather than publishing this methods-centered article now, it might be more impactful to integrate the methods descriptions that make up this manuscript into the series of papers that seem likely to result when the full outcomes of the experiments are available.

Specific comment

I very much appreciate the effort described to minimize RO2 + RO2 reactions in the present study, but since virtually none of the rate coefficients are known, I would contend that it is quite difficult to know whether the experimental conditions described actually minimized the influence of RO2 + RO2 reactions. I don't think the secondary photooxidation chemistry of the isoprene system is really well known enough to be sure that the observation of C5 diols can be exclusively attributed to RO2 + RO2 reactions,

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and the authors themselves mention the recent finding that ISOPN (high NO) can lead to IEPOX (low NO), which also confuses the interpretation of the dominant fate of RO2 radicals in their experiments.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 21611, 2014.