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Atmospheric Chemistry and Physics Discussions

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 #1

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General Comments

This paper describes results from a series of simulation chamber experiments to study the formation of gaseous products and secondary organic aerosol (SOA) resulting from photo-oxidation of isoprene and its major oxidation product, methacrolein. A range of on-line techniques have been used to probe the gaseous species and characteristics of the SOA.

The key feature of this work is the relatively long timescale (7-8 hours) over which the species are monitored which enables investigation of the chemical evolution of the various products and also the aerosol. The concentration-time profiles for the gaseous species are dynamic and nicely reflect the primary and secondary chemistry in detail. In contrast, the composition of the SOA appears to change only for the first 2 hours or so.

The article is, in general, well written and the results are presented in a clear and logical manner. The experimental data are of high quality and the interpretation and discussion of the results is generally appropriate. There are a few minor issues that should be discussed, but overall this is a good piece of work that is of interest to the atmospheric chemistry community. I recommend publication following revision of the manuscript in line with the following comments.

Major Comments

1. One of the key results of this work is that the yields of SOA for both isoprene and methacrolein photo-oxidation are smaller than those observed in most previous studies. The authors attribute this to different light sources used in the various studies. However, there are of course many other factors (discussed in the manuscript) which influence SOA yield. A key factor is the level of NOx in the chamber and in this work, as well as the initial introduction of NOx or HONO, NO is continuously added throughout the experiments. As pointed out by the authors themselves (p 22510, lines 3-10), this is expected to produce less SOA because under these conditions the RO2 + HO2 reactions are less favourable than RO2 + NO reactions. Could this also be a contributory factor to the observed lower yields found in this study? Did the authors perform any experiments without the continuous addition of NO to see if the yields increased?

2. The NO2 and O3 concentration-time profiles shown in Figure 1b are a little unusual. After about 4 hours the NO2 mixing ratio starts to increase. Why is this? Interestingly this occurs during the period where particle formation begins. Are these observations connected? The NO2 signal continues to increase for a further hour or so and then falls. Meanwhile, the ozone signal also shows a corresponding increase. This interesting

behaviour should be discussed and explained if possible.

Minor Comments

1. Page 22508, Abstract: The abstract is not very well written. The English could be improved, e.g., rephrase “general dispersion” and “the solar one”. There is no need to use numbers to list the two main findings.

2. Page 22511, line 3: Rephrase “. . .could also contribute to the observed variability in SOA yields, including. . ..”

3. Page 22515, line 6: Should be “BFSP”?

4. Page 22515, line 14: Rephrase “In all our experiments. . ...”

5. Page 22515, line 23: “photolyzes”

6. Page 22515, line 24: delete “an”

7. Page 22515, line 25 and throughout the rest of manuscript: “ppbv” with the “v” not as subscript.

8. Page 22516, 16: Rephrase “. . ..due to sampling and was found to be around . . .”

9. Page 22517, lines 6-7: Referring to Table 2, the authors state that their measured yields are in good agreement with those in the literature. They certainly do agree within experimental error, but it should also be noted in the text that they have the highest yields for formaldehyde and methacrolein, as well as the lowest yields for methyl vinyl ketone and 3-methylfuran. In addition, all other studies report higher yields for methyl vinyl ketone than methacrolein, whereas the opposite is true in this study. Is there an explanation for this? Maybe some further comment is required here.

10. Page 22517, line 16: “. . .can be made.”

11. Page 22518, lines 13-14: This sentence is unclear – rephrase.

12. Page 22518, line 22: a closing bracket is missing.

13. Page 22519, line 13: “. . ..semi-volatile species. . ..”

14. Page 22520, line 10: Rephrase “. . ..exhibit some variation, our yields. . .”

15. Page 22520, line 23: Rephrase “. . ..induces and increase in SOA yields. . .”

16. Page 22521, line 18: Rephrase “. . ..different from that of alkenes. . .”

17. Page 22523, line 25 and several other places in the manuscript: units for density should be g cm-3.

18. Page 22524, line 8: Rephrase “. . .used: the extent of semi-volatile wall losses could be. . ..”

19. Page 22524, line 18: Rephrase”. . .exhibit a fairly large variation.”

20. Page 22524, lines 18-22: In this work the use of HONO produced higher SOA yields, whereas the opposite was observed in the study of Chan et al. (2010). Is there an explanation for this?

21. Page 22525, line 11: Rephrase “. . ..were very close. . .”

22. Page 22525, line 16: Delete “very”. . ..in line with comment 9 above.

23. Page 22525 and 22526, Section 4: Parts of this Conclusions section are not very well written. The English could be improved.

24. Page 22535, Table 2: rephrase caption “Yields of first-generation. . ...”

25. Page 22535, Table 4: rephrase caption “Yields of first-generation. . ...”

26. Page 22535, Table 4: The yields are reported as a range of values and also without errors. Is there a reason for this? Why not list them in the same way as in Table 2?

27. Page 22539, Figure 2: Typo “particle”. Units of density should be changed.

28. Page 22543, Figure 6: Typo “particle”. Units of density should be changed.

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