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Interactive comment on "Nanoparticle formation by ozonolysis of inducible plant volatiles" *by* J. Joutsensaari et al.

M. Claeys (Referee)

magda.claeys@ua.ac.be

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This is an interesting manuscript presenting the first laboratory evidence for aerosol formation from oxidation of volatile organic species emitted by living plants, namely, cabbage. The concept of using plant chamber experiments is new and offers perspectives for studying the mechanism of new particle formation under atmospheric conditions. The mechanism the authors suggest for new particle formation in the plant chamber under the conditions of the present study is homogeneous nucleation, a mechanism that also holds for smog chamber experiments with terpenes but that appears to be different from the mechanism of new particle formation in the ambient atmosphere. My major comments relate to some statements concerning new particle formation in the

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atmosphere as outlined below. The suggestion is also made to consider isoprene in future experiments.

1) Page 2 (abstract) - line 13. The authors write: “Our results therefore suggest that atmospheric nucleation events proceed via condensation of oxidized organics on pre-existing molecular clusters rather than via their homogeneous or ion-induced nucleation.” This statement is too strong in my opinion: the results obtained in the present study rather suggest that atmospheric nucleation events proceed through a mechanism that is different from homogeneous or ion-induced nucleation of oxidized organics. It cannot be claimed that results have been obtained suggesting that this mechanism involves “condensation” of oxidized organics.

2) Page 3 (introduction) - line 1. Instead of citing the review article by Kanakidou et al., 2004 here, it would be more appropriate to cite an original research article; for example, the study by Kavouras et al. [1], who provided the first field evidence for secondary organic aerosol formation from alpha-pinene, could be cited here.

3) Page 3 (introduction) - line 17. The comment made above under 1) also applies here. I suggest to weaken this statement.

4) Page 4 (experimental) - line 3. Here, I would write: “ initiate an emission spectrum of terpenes that is typical to ”

5) Page 4 (plant material and treatments) - lines 13-14. For the sake of the interested reader, who may not be familiar with the chemical nomenclature of terpenes, I suggest to give some explanation between parentheses here. For example:

monoterpenes (C10 terpenes)

homoterpenes (acyclic terpenes derived from sesqui- or diterpenes)

sesquiterpenes (C15 terpenes)

6) Page 5 (aerosol formation experiments) - line 16. Filtered pressurized air is used in

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the chamber: can it be excluded that this air does not contain SO2, which may oxidize and as such provide sulfuric acid molecular clusters on which new particle formation may occur ? This is relevant to what is written later in the paper where it is stated that no sulfate clusters are present in the plant chamber experiments. Was SO2 measured in the experiments ?

7) Page 7 (plant experiments) - line 5; Table 1. Looking at the results presented in this Table, I wonder whether the data presented for 3-hexen-1-ol + 2-hexenal and 3-hexenyl acetate for cv. Rinda have not been interchanged; it is not logical that the induced levels are lower than the controls.

8) Page 7 (plant experiments) - line 15. The authors write: “Globally the main monoterpenes are alpha-pinene, beta-pinene and limonene ”. Again, it would be appropriate to cite an original research article; I suggest to cite Guenther et al. [2].

9) Page 7 (plant experiments) - line 16. This is a suggestion for future experiments (no action to be taken for the manuscript): the authors argue that the cabbage emission spectrum of terpenes is more relevant to atmospheric conditions than the VOCs or mixtures of VOCs commonly used in smog chamber experiments. I suggest that for future experiments they consider a system that also includes isoprene, which has emissions on a global scale that are higher than those of the monoterpenes and which according to recent field and laboratory studies also serves as a precursor for secondary aerosol formation [3-8]. Furthermore, it has also been demonstrated that there is a co-variance between isoprene emissions and new particle formation above a coniferous forest [9].

10) Page 9 (aerosol formation experiments) - line 18. Here, I would write: “ we believe our experimental results are relevant to atmospheric conditions.” (instead of “are in fact consistent with”)

11) Page 9 (aerosol formation experiments) - line 8. The comment made above under 1) also applies here. I suggest to weaken this statement.

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12) Page 10 (conclusions) - line 23. As already pointed out above, to better assess aerosol forming capacity of vegetation, it is essential to evaluate the whole emission VOC spectrum of different plant species, not only including sesquiterpenes and other inducible terpenes, but also isoprene. As argued under comment 9) there is sufficient field and laboratory evidence that isoprene participates in secondary aerosol formation.

13) Page 10 (conclusions) - line 14. The comment made above under 1) also applies here. I suggest to weaken this statement. The results suggest that homogeneous nucleation that is observed in the plant chamber experiments does not operate in particle formation in the ambient atmosphere. It remains to be demonstrated that the latter mechanism simply involves “condensation” of oxidized organics on pre-existing molecular clusters.

References

[1] Kavouras, I.G., Mihalopoulos, N., and Stephanou, E. G., Formation of atmospheric particles from organic acids produced by forests, Nature, 395, 683-686, 1998.

[2] Guenther A., et al., A global model of natural volatile organic compound emissions, J. Geophys. Res. 100, 8873-8892, 1995.

[3] Claeys M., et al., Formation of secondary organic aerosols through photooxidation of isoprene, Science, 303, 1173-1176, 2004

[4] Matsunaga, S., Mochida, M., and Kawamura, K., Growth of organic aerosols by biogenic semi-volatile carbonyls in the forestal atmosphere, Atmos. Environ. 37, 2045-2050, 2003.

[5] Jang, M. S., and Kamens, R. M., Atmospheric secondary aerosol formation by heterogeneous reaction of aldehydes in the presence of sulfuric acid aerosol catalyst, Environ. Sci. Technol., 35, 4758-4766, 2001.

[6] Jang, M. S., Czoschke, N. M., Lee., S., and Kamens, R. M., Heterogeneous atmospheric aerosol production by acid-catalyzed particle-phase reactions, Science, 298,

814-817, 2002.

[7] Limbeck, A., Kulmala, M., and Puxbaum, H., Secondary organic aerosol formation in the atmosphere via heterogeneous reaction of gaseous isoprene on acidic particles, Geophys. Res. Lett., 30(19), 1996, doi:10.1029/2003GL017738, 2003.

[8] Claeys, M., et al., Formation of secondary organic aerosols from isoprene and its gas-phase oxidation products through reaction with hydrogen peroxide, Atmos. Environ., 38, 4093-4098, 2004.

[9] Leaitch, W. R., et al., A case of gas-to-particle conversion in an eastern Canadian forest, J. Geophys. Res., 104, No. D7, 8095-8111, 1999.

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