

Interactive comment on “The chemical and microphysical properties of secondary organic aerosols from Holm Oak emissions” by N. Lang-Yona et al.

Anonymous Referee #1

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This is a neat study that examined the effect of three different temperatures (25, 30 and 35°C) on the emission of volatile organic compounds (VOC) from Mediterranean Holm Oak, and the impact this had on the formation of secondary organic aerosol. The work was carried out in the laboratory based plant-aerosol chamber facility at Julich. The temperatures used are suitable and relevant to a Mediterranean tree species. The use of real plant emissions also ensures atmospherically relevant concentrations of VOC were studied. In addition to altering the temperature, two aerosol chamber filling procedures were used to optimise particle growth based on maximising or minimising the presence of OH at the time of flushing with plant chamber air. The paper does address a relevant scientific question – that of SOA formation under changing temperature and

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the methods are clearly outlined with only a few minor omissions. The abstract needs a little work but, the overall presentation is good and there are no extraneous figures or tables.

1. The abstract (line 6) mentions the investigation of varying light intensity as well as temperature. This is referred to again in the results section, (paragraph two of section 3.1 Plant emission patterns of volatile organic compounds). However, these data regarding effect of increasing light intensity on VOC emissions and subsequent SOA formation are not included. Given that the experiments presented in the manuscript are not replicated sufficient times for a biological system, I would like to see this data to: 1. demonstrate that the VOC emissions at a constant temperature and at the light intensity selected for use in the final experiments were not significantly varying with time, and 2. support the statement that “the emission behaviour was typical for Holm Oak” (final paragraph, section 3.1).

2. Figures 4 and 5 appear to indicate that only one experiment at each temperature level was undertaken. I have a number of questions regarding these figures. Do the figures include both methods used to maximise SOA growth (Ox-induced and VOC-induced)? If so it would be useful to indicate these points separately. Does it improve the error terms if one regression line is put through the Ox-induced method, and a second through the VOC-induced method? One experiment on the small Mediterranean stand is not enough to confirm that the emissions and subsequent SOA formation were typical for this species. A minimum of three replicates are required for any biological system. Based on one experiment I am not confident of the comparison to the Holm Oak. Could confidence bands be shown for the regression line?

3. Section 3.2. The authors write that “the SOA formation potential of tree species just depends on the amount of emitted VOC, as long as the emissions mainly consist of monoterpenes”. By this statement I believe the authors to mean that if you removed every other variable, affecting SOA formation not emitted directly by the tree species in question (for example, humidity, isoprene, oxidant levels, pre-existing seed and acidity

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of seed), the only direct influence on SOA formation is the amount and type of VOC emitted. However, this is a virtually impossible situation to find in nature as the authors note in their conclusions. They acknowledge the limitations of the chamber design in not being able to introduce pre-existing seed and mention the potential influence of isoprene on SOA formation. I believe that the authors' statement in section 3.2 is confusing and of low scientific value given the highly restricted limitations it implies. I feel this statement should be removed.

4. Summary (section 4, line 21). The authors suggest that the values for SOA formation obtained from their experiments are relevant to the atmosphere and can be applied to model calculations. I disagree with this statement and suggest that the data can not be used in model calculations due to the absence of pre-existing seed which can have a significant impact on SOA yield and growth rates. I think this sentence should be revised or removed.

5. Summary (section 4, line 24). The authors suggest their measurements show that the SOA formation for Mediterranean species increases more with increasing temperature than for Boreal species. However this is based on the false assumption that there are no stress induced changes in VOC emissions. There are numerous studies that show how VOC emissions change with differing environmental stresses (see for example isoprene emission increasing with ozone (Velikova et al, 2004) and high temperature (Sharkey and Yeh, 2001), drought effects on monoterpenes (Lavoie et al, 2009) and sesquiterpenes (Ormeno et al 2007). Furthermore, as well as being a stress compound, ocimene which the authors highlight, is strongly dependent on instantaneously fixed carbon from photosynthesis (e.g. Noe et al, 2006). Therefore, in the absence of other stresses (i.e. water limitation) which would close the stomata and significantly reduce carbon assimilation, the emission of this compound will increase with photosynthesis. The authors need to better support the conclusion that the increase in VOC emission (and therefore SOA formation) is higher for Mediterranean species than boreal species, given that the majority of this difference is based on ocimene emissions.

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This could be achieved by including gas-exchange data.

Minor corrections:

1. I do not understand the sentence in the abstract "monoterpenes dominated the VOC emissions from Holm Oak and temperature increase enhanced the emission strength under variation of the emission pattern" What do you mean by "under variation of the emission pattern"? Please clarify.

2. Introduction, line 15 "plants exposed to high pollution and CO₂ levels will close their stomata. . ." Do the authors mean ozone when they say high pollution? If so, this is not always true. Low concentrations of ozone may perturb the stomata and impair stomatal functioning, resulting in increased stomatal uptake of ozone (see for example Mills et al, 2009, Global Change Biology and Wilkinson and Davies, 2010, Plant, Cell and Environment).

3. How long are the plants left to adapt for at each temperature level? The acclimation time between experiments should be stated.

4. What were the growth conditions for the Holm Oak? Were these similar to the conditions the specimen was kept in during the experiment?

5. What concentration of ozone is added to the aerosol chamber? This doesn't appear to be mentioned in the methods section.

6. Methods section, line 19 typo "details" should be "detail"

7. Summary section, line 26, page 4769 typo "2 degrees" should be "2 degree"

8. Summary section, line 12, page 4770 typo "independent on emission patterns" should be "independent of emission patterns"

I think this is a neat and very interesting piece of work let down by weakly supported conclusions and a lack of experimental repetition. I would recommend publication with major revisions including additional experimental work. A minimum of three repetitions

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of each method at each temperature must be carried out in order to have confidence in the results. In addition, I would recommend that a boreal tree species is studied with the same experimental and environmental conditions and the same minimum number of repetitions to support the conclusion that SOA formation from Mediterranean species is more temperature dependent than boreal species.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 4753, 2010.

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