

Interactive comment on “Isoprene emission and photosynthesis during heat waves and drought in black locust” by Ines Bamberger et al.

Anonymous Referee #3

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Summary:

This work examines the changes in foliar isoprene emission and photosynthesis of isoprene emitting black locust to periods of drought and drought plus heat stress. The isoprene and photosynthesis responses are compared to existing literature and the response of isoprene emission was then compared to that which would be calculated using the Guenther et al algorithm under the same stress conditions. In general the study is well written, although some aspects could be clearer (outlined in the comments sections below). There are a few typographical errors and the within text references need to be looked at as they should be presented either alphabetically or by date order. This study does appear to contain a solid body of work which is worth publishing to add to our understanding of isoprene responses to complex stresses and to help improve modelled emission estimates. However, as the manuscript is currently written I struggle

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to find the novelty in the work. I have a few concerns and believe the manuscript should be improved as outlined below before it could be accepted for publication.

Major comments:

1) Materials and Methods, Experimental set up, line 96. I am concerned that the trees in the stress treatments had previously been exposed to two experimental heat waves and were showing a difference in basal area. Previous work has shown that VOC emissions differ based on exposure to previous environmental conditions (e.g. Sharkey et al , 1999 and citing references). Could the authors provide some reassurance that after pruning and over wintering the development and growth rates were then equivalent and could be fairly compared to one another? If they were not equivalent as suggested in the results section 3.1, were the data normalised?

2) Could the authors give an explanation as to why the trees were not randomly selected for the work included in the current study? This would have given a mixture of previously stressed and unstressed trees in each treatment group and removed any concern that the prior treatment of these trees was affecting the current results.

3) I would also like to see a clear description of the growth conditions and number of trees used per treatment and per measurement. Could the authors give a full description of the growth conditions of the trees (temperature, light, CO₂, RH) in the description of the experimental setup? Did the greenhouse have supplemental lighting, where was average PAR recorded, what was the day length? How many replicates were used per measurement? At the moment it is not clear to me how many replicates were used for what.

4) In general I cannot currently see the novelty of this work. However, this might be improved if the authors could use their data to suggest a new algorithm or an amendment to the existing algorithm to bring modelled isoprene emissions more in line with that which is observed. At the moment the authors highlight the difference between the observed and modelled emissions but don't go any further.

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Minor comments

- 1) Abstract line 12 – mentions assessing the impact of stress on BVOC emissions but only isoprene is presented in the manuscript. Either remove the reference to general BVOC or include other emitted compounds.
- 2) Intro, line 38 – include ref to more recent Wyche et al, ACP 2014 which gives positive and negative effects of isoprene emission on secondary aerosol formation.
- 3) Intro, line 65 and line 71 – include ref to more recent Ryan et al, New Phyt 2014 and remove older references unless they are seminal /original work.
- 4) Mat and Methods, Paragraph starting line 155 – description is not clear. Is the automatic switching of the measurements or the air flow? If air flow does this mean the chambers were clamped on the plants with no air flow for a period of time?
- 5) Section VOC Line 200 – the PTR-MS only counts set masses and cannot give compound identification. Could the authors include information on any mass identification that was performed (e.g. GC-MS) to confirm that it was only isoprene at m/z 69
- 6) Line 231 – 500 PAR seems quite low for trees in the summer. Top of canopy PAR in northern Europe during the summer is more likely to be between 1000 and 2000 PAR. Could the authors give a reason for choosing 500 PAR.
- 7) Mat & Methods Line 267 - Formatting error
- 8) Results 3.1 line 295. Could the authors include a description of how midday leaf water potential was measured?
- 9) Results 3.1 line 299 – typo “relative” should be “relatively”
- 10) Results 3.2 line 307 – I don’t understand why “ (PAR > 50 $\mu\text{mol m}^{-2} \text{s}^{-1}$)” is included in this sentence, when the sentence is referencing stomatal conductance – please clarify

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- 11) Line 316 Daytime (PAR > 50 $\mu\text{mol m}^{-2} \text{s}^{-1}$) – I am assuming this means the authors collated any data collected when PAR readings were over this value to be “daytime” values. If this is correct please include a clarification at first use to make it easier for the reader to understand.
- 12) Line 322 – It may be over-stretching the results to include “marginally significant (p value around 0.1)” results as significant differences. This is not common practice but is perhaps personal preference.
- 13) Results 3.3, line 338 “significantly different to control trees” and “no significant differences. . .” please give p values.
- 14) Discussion Line 380 – references you should include more recent ref e.g. Ryan et al New Phyt 2014 who used genetically modified tobacco specifically to study the impact of drought on isoprene emission and protection.
- 15) Line 385 “A quick recovery of isoprene emissions after periods of drought stress seems to emerge as a 385 common feature that has also been observed in previous studies (Brilli et al., 2013; Pegoraro et al., 2004; Velikova and Loreto, 2005)” and line 288 “The observed faster recovery of isoprene emissions than photosynthesis may be a common pattern following stress release (Brilli et al., 2013; Pegoraro et al., 2004).” This appears to be a repeated point – please remove one of the sentences.
- 16) Line 390 – “this is the first study that considers dynamics of isoprene emissions during and following combined heat–drought stress. . .” Unfortunately this claim is untrue – please remove and see Vanzo et al, 2015 and references therein.
- 17) Paragraph beginning line 415 – including reference to Ryan et al, 2014, New Phytologist, who studied isoprene emitting and non-emitting plant responses to drought, would be appropriate here. Most likely with the Vickers et al, 2009 reference. 18) Table 2 – could the authors explain why there is such a variation in group sizes (n values from 0 – 49)?

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References

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Ryan, A. C., Hewitt, C. N., Possell, M., Vickers, C. E., Purnell, A., Mullineaux, P. M., Davies, W. J. and Dodd, I. C. (2014), Isoprene emission protects photosynthesis but reduces plant productivity during drought in transgenic tobacco (*Nicotiana tabacum*) plants. *New Phytol*, 201: 205–216. doi:10.1111/nph.12477

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