

Interactive
Comment

***Interactive comment on “Drought reduced
monoterpene emissions from *Quercus ilex* trees:
results from a throughfall displacement
experiment within a forest ecosystem” by
A. V. Lavoit et al.***

Anonymous Referee #1

Received and published: 1 April 2009

A better understanding and quantification of drought impacts on biogenic emissions from Mediterranean forests is very much needed for assessing the role of BVOCs in atmospheric chemistry in general and under future climate conditions; especially field observations including manipulations are needed by the modeling community; the paper has utility with the readership of Biogeochemistry.

The study investigates the impact of enhanced drought on emissions of BVOCs from a holm oak forest at Puechabon, making use of the large scale Mediterranean throughfall displacement experiment MIND to assess drought vulnerability. Excellent idea of using

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experimental resources from another project: field studies on BVOC emissions hardly find any funding in the European research scene of today.

My general impression of the paper: writing is fluent and easy to understand, the methods applied appear to be state of the art, presentation and discussion of results is concise and well balanced. By measuring the seasonal course of emission rates, physiological and biochemical parameters in parallel, the French/German collaboration achieves an experimental level, which most research group working in the field would have difficulty to follow.

Especially laudable is the approach to describe the plant water status with the predawn water potential as core information, which appears to be the only reliable parameter to indicate eventual water shortage, especially of trees like the deep rooting holm oaks, but is not often monitored because working "pre-dawn" is not so comfortable. I'm missing one parameter surely measured but not reported in the paper: transpiration and stomatal conductance may not be relevant as a factor limiting the emissions, but would be interesting to confront with the other physiological parameters like CO₂ assimilation and PSII photochemical trapping efficiency.

Like most field experiments, the study has some weak points in the experimental design, which is not strictly factorial: it is based on the comparison of ambient to 27% reduced rain at Puechabon, which is a very minor variation compared to common interannual variability's of Mediterranean drought exposure. Consequently, there was no significant treatment effect on BVOC emissions; instead, the typical polyfactorial field design included the impact of Gypsy moths, the comparison of current vs. one year old leaves and of 10years old vs. some 60 years old trees, growing on different soils, etc.

In consequence, comparative statements like in the abstract: (i) "due to a more pronounced summer drought"; (line 11) or (ii) "irrigated trees emitted 82% more than trees of the other treatments"; (l. 13) are not fully correct. The

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results may also be affected by (i) current leaves in 2006 versus one year old leaves in 2005 (fully expanded new leaves need days up to weeks before emitting at mature level), and (ii) irrigated trees have different age and grow in different soils etc.

I would not be so much worried about comparisons and student-tests; its ok to look at, but the story to tell is convincing by its own: (1) drought is a factor to be considered for estimating BVOC emissions under Mediterranean or future climate conditions; (2) several questions are arising from the study to be followed up in order to come to a more quantitative understanding of drought impacts, e.g., is the relatively sharp control of predawn water potential at -2MPa a primary driver or an indicator for resource depletion? (3) we need much more field studies of this type, including other species and other experimental set-ups (e.g., at drought exposed sites, adding rain instead of excluding rain), to get enough datasets of driving parameters (like the ones shown in Fig. 5) for achieving realistic estimates.

Specific comments:

I agree with the authors in all points of their reply to comments of referee 2 and look forward to see improvements of the paper in reaction to these comments, e.g., to get the title sharper, or to see some definitions being used more clear and consistent.

Abstract: In addition to the two points mentioned above (lines 10 and 13), I consider a bit surprising the concluding statement (l.23) "due to a sustained inhibition of photosynthetic carbon assimilation"; I would not exclude this, but in my view this is not backed up by the data. I would prefer asking questions like (2) above.

Introduction: The proof of the statement in line 20ff "the literature is still inconclusive on this subject;..BVOC emissions were reduced, enhanced or unchanged in response to water stress"; is not convincing. Emissions of monoterpenes from species like Cistus, Rosemary or Pine with storage organs for aromatic oils (Ormeno et al. 2007) has nothing to do with the situation of holm oak without any stor-

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age organs but emitting monoterpenes from current synthesis. Also the comparison with isoprene emissions from poplar is not helpful because poplar is so different from holm oak.

I propose to focus both Introduction and Discussion on studies really relevant in the context Q.ilex/BVOCs/drought; a quick shot to my literature database delivers the following studies, which should be considered in addition: - Bertin & Staudt (1996) *Oecologia* 107: 456-462 - Bertin et al. (1997) *Atm.Env.* 31-S1: 135-144 - Nunez et al (2002) *Atmospheric Environment* 36: 4441-4452 - Llusia et al. (2009) *Russian Journal of Plant Physiology*, No. 1, pp 29-37

The paper of Niinemets et al (2002) *New Phytol* 153: 257-275 describes a coupled photosynthesis/BVOC emission model developed from Q.ilex field measurements and may also deserve attention.

No comments to the part Results and Discussion in addition to the general comments above. Just one point related to soil water storage measured and modeled in Fig. 3: the lines show continuity during transition 2005-06 despite the gap of first 90 days; nothing happened between Jan and Apr?

PS: The interesting paper of Lavoie and colleagues would have deserved more rapid reaction and publication. My excuse for late delivery of this review because of an ski accident.

Interactive comment on *Biogeosciences Discuss.*, 6, 863, 2009.

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