

***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.***

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Preliminary note: I admit that I might be a little biased in my opinion because as a modeller I have a direct interest in the publication of these data. I have also some contacts to the group and have been provided with the data already. However, I was not involved in the preparation of this manuscript.

The manuscript of Lavoit et al. approaches a currently very important topic. According to climate change scenarios, the Mediterranean region will be certainly exposed to

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changes in temperature and drought thus altering the conditions for BVOC emissions. It is kind of scary to see that more and more regional air chemistry studies appear that are still using the Guenther approach from 1995 without consideration of drought. Although this deficit has been recognized in the new MEGAN model (please check introduction for not making a mistake here), no parameters to consider this effect appropriately are available.

Field measurements of BVOC emissions in the Mediterranean are still very scarce. I am aware of only three recent studies that would fall into this category (see below, unfortunately not mentioned in the manuscript although the first might be too recent to be recognized here). All of these are from Spain and differed partly in measurement techniques and species selection. Therefore, the presented data are very nice additions to the current knowledge base and should potentially be suitable for improving modelling of this topic.

With regard to the comments of referee 2 it is true that some more care should be exercised with the selection of literature references. Also, some descriptions or definitions could be more clearly presented, and the last sentence of the abstract which represents a generalization of the results that is not quite justified should be reworded. With respect to the demand of additional data, I see that internal CO₂ might be of interest for a more detailed analysis although the basic information is already presented with the photosynthesis data given.

Some problems mentioned are definitely not applicable or probably based on misunderstandings. For example the difference between control and dry plots are indeed small - which is really not surprising given the small difference in soil water content. Therefore it is very appropriate to put the results of these sites together and derive the drought effect from the difference to the irrigated site. Secondly, it is possible that a threshold for drought impact exists and this might be recognized by reference to the paper of Brillì et al.. This assumption, however, is not very different from the theory discussed that a lack of carbon supply under extreme drought is responsible for the

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decrease in emission. Overall, the discussion is well in line with the current knowledge. Finally, it is true that the emissions are affected by various environmental conditions - which is the reason why field measurements are important additions to laboratory investigations. In my opinion, these impacts have been sufficiently discussed.

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