

Interactive comment on “Impacts of soil moisture on de-novo monoterpene emissions from European beech, Holm oak, Scots pine, and Norway spruce” by C. Wu et al.

Anonymous Referee #1

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This paper investigates the effect of soil moisture (SM) on de novo monoterpene (MT) emission of seedlings of 4 tree species. The paper is generally clearly written; the ideas are logically introduced and discussed. The methodology is sound, at least in what concerns MT measurements, because the experimental design and the application and evaluation of drought are less rigorous. The main merit of the paper is the valuable attempt toward modelling the effect of soil water availability on MT emission of 4 widespread tree species. The main flaws are: 1) the election of soil moisture as a reference parameter of soil water availability, 2) the uncertainty authors acknowledge in the measurement of SM (which precludes differentiating the sensibility of MT emission to SM among species), and 3), the confusing writing of an important aspect as is the

validity of a factorial approach to model the effect of several abiotic factors on MT emission. I understand using plants cultivated in pots is almost inevitable to study abiotic factors in controlled experiments. Although I initially saw the use of potted plants as a flaw, there are two questions that made me change my mind: the large volume of pots to grow 2 year-old seedlings and the similar response of MT emission to soil moisture in two experiments with different rate of soil desiccation.

1) I don't share authors' opinion that SM is the best parameter to study and model plant responses to drought; I think soil water potential or relative water content (RWC) are better indicators. For example, RWC is useful for meta-analyses and comparison among experiments because it is independent on the nature of the soil.

2) Authors cannot reliably estimate soil moisture (see P 12993 Line 5 and P 12996 Line 23; 15% error is very high relative to the range of soil moisture they monitor 0-40%), and yet soil moisture has a central role in this paper. On the other hand, authors leave it clear throughout the text, so that readers can decide whether these summed errors in estimating SM invalidate their conclusions or not.

3) I have found it hard to follow the description of the experiment 3 with holm oak, as in the Material and methods as in the Results. I advise authors to be clearer in this aspect, as the question of how to model temperature and SM is also important in the paper. From the Material and methods I don't understand if temperature sensitivity of MT emission is assessed along the whole gradient of soil water availability. Then in the Results section, I don't find it clear whether there is an interaction between temperature and SM; it seems so in Lines 20-28 (P 13000), but not from figure 6. Finally, I don't understand why using a correction factor for MT emission in experiments 3 and 4. First, why leaving temperature constant below 0.02 SM? Cannot you use previous relationships of SM and MT emission at constant temperature from the previous experiments 1 or 2? Or more simply, why not just simply measure MT temperature sensitivity along the whole gradient of SM? Please, can you try to clarify these questions? I would expect net CO₂ assimilation does not change or even decline with increasing tempera-

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ture and thus de novo MT emission would be less sensitive to temperature as seedlings are more drought-stressed.

Other aspects that I highlight from the review follow.

P 12992 Line 2. Why did you decide to set CO₂ concentration at 350 ppm? Can you indicate the deviation from 350 along the experiment?

P 12993 Line 5 How much the 20-30 g error is in proportion to total weight?

P 12994. Line5. It is dangerous to select “representative” leaves. We can subjectively over/under estimate leaf area by non-randomly sampling bigger or smaller than average leaves.

Somewhere in the Material and methods, it should be said how many plants per species were used in the study.

P 12997 Line 4. Why the standard emission rate of MT is considered as the average around the maximum values and not at maximum SM?

P 13997 Lines 19-24. I find it surprising that severe droughts do not cause a generalized metabolic “disorder” preceding mortality that is reflected in the VOC spectrum.

P 12998 Lines 6-21. Due to differences in the composition of MT emission, would not it be more practical to demonstrate drought effects on the sum of all MT instead of the dominant compound (which may change among studies)?

P 13002 Lines 19-27. It would be nice to see the data of MT emission versus SM, as for holm oak and beech in figures 5b and 4, respectively.

P 13003 Line 16. Plant water potential between -2 and -8 MPa is not mild drought stress; it is a severe stress even for Mediterranean species.

P 13009 Line 15. Please say how you estimated the fraction of photosynthetic electron transport destined to MT synthesis. The increasing fraction of photosynthetic electron

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transport destined to MT synthesis between 0.15 -0.10 soil moisture (Fig 8) does not appear to support a negative effect of drought in MT emission at this range of soil moisture (Figs 4 and 5b).

I miss a figure or a mention of the correlation between net CO₂ assimilation and MT emission, for every species.

Similarly as for Fig 6, showing “beta” dependency on SM (useful for modelling through equation 1), I would show “alfa” dependency on SM (to model through equation 2).

Other minor appreciations are:

P 12987 Line 13. “as” is missing.

P 12990 Line 27. You previously say MT emission in conifers depends on diffusion from pools. However, you measure two conifers, and say here that you are going to study de novo MT emissions. This is later explained, but at first sight is intriguing.

P 12991 Line 18. “respectively” is missing.

P 12994 Lines 25-28. It seems that pine, beech and spruce seedlings were grown outside and holm oak in a growth room during acclimation. If this is correct, why setting different cultivation conditions among species?

P 12996 Line 16. Please indicate how you separated the weights of the pot and the plant from total weight to estimate soil weight.

Figure 1 legend. Separate “hand” and “y”.

Figures 3 and 4. Are not values of net CO₂ assimilation of beech low? Even for plants in the shade?

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