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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variability of BVOC emissions from a Mediterranean mixed forest in southern France with a focus on Quercus pubescens" *by* A.-C. Genard-Zielinski et al.

Anonymous Referee #1

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General comments

The topic as well as the approach and methodology used by Genard-Zielinski et al. are relevant. There are results and conclusions that give fresh insight on an overlooked branch of BVOC emission research. My main concerns are related to how the methodology is applied and to the analysis of the results. Some details in results and discussion are worthy of more detailed and in-depth handling to avoid underutilizing of high quality data. There are also some minor weaknesses concerning how data is analyzed and results are interpreted; however they do not shake the main findings expressed. Given the fresh approach and scientific value of the study it certainly de-

serves publication but some points should be better addressed beforehand.

Specific comments

P17227L14: I perceive temperature and light as environmental drivers for emissions (synthesis & evaporation), not as triggers. Word 'trigger' suggests that temperature and light only trigger the emissions that are otherwise independent on those factors. Moreover, word 'parameter' is also somewhat confusing in this context, I would suggest 'driver' or 'factor' instead.

P17227L16: Guenther et al. 1991 did not suggest the existence of any circadian clock, which is a central presumption for circadian rhythm. It's true that circadian rhythms are discussed nowadays (for example Hewitt et al. 2011) but in context of this study such concept is neither handled nor needed. Therefore I would suggest 'diurnal' instead of 'circadian'. 'Diurnal' does not take any stand on the factors behind the phenomenon.

P17229L6: Please report also the age of the stand if possible as well as other key characteristic features of the stand (for example mean diameter of the trees, the stage of canopy closure, etc.). These details are potentially important if someone is going to apply your results in modeling or upscaling.

P17230L23: Please report the age of the leaves, at least if they were growing, young or mature, if possible. Leaf expansion can have significant effect on the VOC emissions (Hüve et al. 2007, Aalto et al. 2014).

P17230L24: Please explain the acronym 'PTFE'. Also, mixing trade name 'Teflon' and the exact name of the material (PTFE) is quite confusing. I suggest sticking to the latter and avoiding trade names if possible; at least PFA is also sold under trade name 'Teflon'.

P17232L7: Did you measure the LMA by yourself or is the value for conversion obtained from literature? Reporting the range for the LMA gives an impression that these are your own measurements but it's not stated explicitly. Also reporting the order of

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magnitude for the mass or area of leaves enclosed would make sense.

P17232L15: Please report the inner diameter of the sample tube. Both length and flow are now mentioned, but also i.d. is needed if some wants to get an idea of the tube lag. Also report if any sample tube heating was used.

P17240L17 and onwards: Please discuss the potential reasons for the differences between the branches. You can't be happy with the variation this high (1/3 of the branches not showing exponential relation between Pn and emission rate, and huge variation in parameter values between the branches) without discussing it.

P17241L19 and onwards: You strongly propose that appropriate LMA values are essential in BVOC emission rate and upscaling studies. However, LMA is only a conversion factor between area and mass. If I have understood correct, you measured the leaf area and then estimated the leaf mass applying appropriate LMA values. The estimate for leaf mass was needed because you report the emission rate per leaf mass. You make two interesting conclusions: i) There is no significant difference between the Is for unshaded and shaded leaves (true assuming that your leaf mass estimates are unbiased, which seems to be a justified assumption), and ii) appropriate LMA values for different parts of canopy are needed in upscaling studies (true, and you illustrate this with nice example, but in wider perspective it's obvious that unbiased estimates or conversion factors for emitting mass or area are essential in all kind of inventory studies). The point: No matter if it's LMA, SLA, or any other conversion factor or other estimation method, but any kind of biases should be avoided or at least one should be conscious of those in any kind of study. No needs for major revisions, but I'd like you to clarify the emphasis of the statements and conclusions.

P17242L10: Please add a reference regarding the typical range of uncertainties for upscaling exercises you mention here. And instead of general satisfaction with the factor of 2 discrepancy it would make sense to consider the potential reasons for the discrepancy.

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P17243L1: CL and CT have always equal weights. No separate weight coefficients are included. The value of CL or CT doesn't tell anything about the relative influence of the parameter. For example, when it's dark and CL = 0, it still has equal weight when compared to CT, and in multiplication CTCLIS it actually plays quite important role with that value. Additionally, CL saturates to values close to 1, whereas CT can have values close to 2. This makes comparison between the values even more unthinkable. Then the third problem is with figure 4, where, if I have understood correct, the sum of "relative contributions for CL & CT" should be 1, but it isn't. I suggest that instead of relative contributions you present the values for CL and CT, and include also T to the figure 4. Then you should carefully consider how you interpret the differences between the values of CL and CT.

P17243L20: The idea of using CL x CT is great. I also liked how you interpret it. But after solar noon it's more like reverse "S" or reverse sigmoid instead of "S" (Fig. 5).

P17244L20: In addition to literature value for Is why not to apply also your own measurements?

P17245L9: Correlation or R2 is quite limited measure for the goodness of model. It only describes the precision, and doesn't take any stand on accuracy. I suggest applying also some other measure for goodness of model, for example RMSE. For more details concerning the measures of model validation, see Niinemets et al. 2013).

P17246L16: What do you mean with 'twice smaller'? 50 %?

P17247L10: I suggest you to be careful with conclusion like this if it's based on the concept of relative contributions of CL and CT. Fundamentally, CT is an empirical description for the temperature dependence of enzyme activity, and CL describes the empirical relation between light and electron transport rate. Together they quasi-empirically describe the isoprene synthesis. On the other hand, it really seems that Pn and isoprene emission rate are surprisingly well correlated with the exception of midday when stomatal closure is maybe limiting the carbon assimilation. And this makes the data

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presented in figure 2 more complicated; while stomatal closure is limiting carbon assimilation, isoprene emissions are not reduced by stomatal phenomena to great extent (see Qp1 and Qp6 in fig 2, rainy day -> weak or no stomatal closure). But this is something I suggest you to take a look on, because it potentially explains the variation between the branches in fig. 2, at least partly. After all it's possible that under ideal conditions the relation between Pn and isoprene emission rate is somewhat linear.

Table 1: The table concerning environmental conditions during the period is informative, but figure would be even more illustrative.

Table 3: Please report the difference between temperatures inside the enclosure and ambient; if this approach is not possible to implement please include at least some estimate concerning the temperature difference to the results.

Figure 4: As mentioned earlier, report also T. And also, sum of the "relative contributions" should be 1?

Figure 6: Please include value '100' to the x-axis of the small figure (the one with logarithmic axes).

Figure 7: Why did you ended up using logarithmic axes? At least it doesn't make the readers life any easier.

Technical comments

P17229L15: 'radiation' instead of 'radiations'? P17232L4: 'PFTE', should be 'PTFE'

References

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 17225, 2014.