Reply to Referee #1 comments

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 deserves publication but some points should be better addressed beforehand.

The improvements suggested were made and detailed here after in our replies to the specific comments. Note that, since a new figure (Fig. 2a&2b) replaces now the former Table 1, figures and tables numeration is now different in this revised manuscript.
Presentation of all the figures has also been slightly improved.

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.

We agree with this comment, and changed, here and further in the whole manuscript, with 'driver'.

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.

We agree and have changed the 'circadian' with 'diurnal'.

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.

Additional detailed information on the biomass (mean diameter, stage of canopy closure and dry leaf production) is now given in section 2.1

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).

Leaves were mature and 3 month old as now mentioned in section 2.3 of the revised manuscript.

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'.

The exact name of PTFE was added, and 'Teflon' was replaced by 'PTFE' all over the manuscript.

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

Indeed, we assessed by ourselves in situ the *Q. Pubescens* LMA values on the O3HP site. It is now explicitly stated in the manuscript (section 2.3), together with the dry mass and area magnitude of the leaves enclosed.

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.

The sampling tube was ¼" diameter and was not heated, as it is now specified in our revised manuscript (section 2.3).

P17240L17: 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.

We have now developed (see end of section 3.3.2) the reasons of the differences of Pn observed among the different branches.

P17241L19: 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.

We agree with these remarks and, as suggested, the statements/conclusions on this point (which is now in the new section 3.4.1) have been changed in the revised manuscript.

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.

As the referee suggested we have added the Guenther et al. (1995) reference which point out a factor of three of uncertainty due to upscaling exercise. The potential reasons for such discrepancy are given in details in the Kalogridis et al. (2014) paper. However a short comment was added.

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.

As also suggested by Referee #2, the use of what we called the 'relative contribution of CL and CT' is no longer made in the new manuscript. Instead, CL, CT and T curves are now presented in former Fig. 4 as suggested, and a new interpretation of former Fig. 4 is made in the (new) section 3.4.2.

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).

As suggested by the referee, we changed to 'reverse sigmoid'.

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

As suggested we have also tested the algorithms with our own Is values. New comments are now made in the (new) section 3.4.3 and corresponding updating was made on Figure 6. Figure 7 was removed since very few information was given; instead a new table (Table 3) summarises all comparisons made using both algorithms, using different values and for both Qp4 and Qp1 trees. New comments were made in the 'Abstract' and the 'Conclusions' sections as well.

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).

As explained on the previous comment (P17244L20), the effect of Is value on isoprene emission assessment is now considered and discussed in the revised manuscript; former Figure 6 was updated accordingly and a new Table 3 is now presented.

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

As suggested by the Referee #2 as well, we are now using 'half' or '50%' instead of 'twice smaller' in all the text.

P17247 L10: 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 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.

As mentioned earlier concerning referee comment P17243L1, we are no more using our so called 'relative CL and CT' contribution. The correlation between Pn and ERiso for the different branches (former Fig. 2) is now better described in section 3.3.2 as required in referee comment P17240L17.

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

As suggested the data presented in the former Table 1 are now plotted in the new Figures 2a and 2b.

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.

Ambient temperatures were given, for comparison, in the original manuscript, in the former Table 1 (which is now Figure 2a). A comment is now made in the former Table 3 (now Table 2) in order to give the reader an estimate of how different enclosure and ambient temperatures are.

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

As mentioned earlier for referee comment P17243L1, CL, CT and T curves were added in the former Fig. 4 (now Fig. 5) and the 'relative contribution' of CL and CT are no more considered.

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

Since the results of all the algorithm comparisons are now compiled in the (new) Table 6, the small figure is no longer presented in this (former) Figure 6 – now Figure 7.

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

As for previous comment, all the results are now complied in a new Table (Table 4); this (former) Figure 7 was thus removed.

P17229L15: ‘radiation’ instead of ‘radiations’?

This change was made as suggested.

P17232L4: ‘PFTE’, should be ‘PTFE’

All ‘PFTE’ was corrected.