# Reply to Referee #2 comments

## General comments

This article describes the branch-level measurements of BVOC (mainly isoprene) emissions from Quercus pubescens trees. The data seems interesting and is a good complement of other papers resulting from the same campaign (e.g. canopy-level fluxes), so it is relevant for the scope of the journal. However the presentation and discussion of the results should be made clearer to the reader before this manuscript is ready for publication. Also, the use of CL and CT requires some further discussion and clarification. In addition, which data was obtained with PTRMS and which with adsorption cartridges should be clearly indicated in the text and the Figures to avoid confusion.
See the specific comments below for more detail. I will complement what referee #1 already commented.

As for the referee #1, the different general improvement suggested were made and detailed here after in our reply to the specific comments. In particular we made clearer in the revised manuscript when PTR-MS and when cartridges were employed in our measurements (‘sections 2.2’ and ‘2.3’). The structure was slightly changed, as suggested, with a re-organised section ‘3.2’ new section ‘3.4’.

## Specific comments

P17227L27: delete the "-" sign after "14 _ 7".
This change was made as suggested

P17227L28: should the "high" range of emissions should have the ">" symbol in front of the number "70"?
This change was made as suggested

P17232L10: between the words «biomass" and «LMA", isn’t there some punctuation mark missing?
Referee #2 was right, a punctuation mark was added as suggested between ‘biomass’ and ‘LMA’.

P17232L23: please define IMBE and LSCE. I guess these are different laboratories where the samples were analyzed. Is there any reason for the samples to be analysed in different labs? If so, please clarify.
The laboratories IMBE and LSCE are already defined in the author list; they were not defined again here to prevent any tediousness. But as recommended, we have explained why this partnership was created, essentially due to the number of samples (section 2.3).

P17232L26: change "closed" for an appropriate word. May be "close"?
As recommended we have change by ‘close’.

P17233L18: change "chromatography" for "chromatograph".

As recommended we have change by ‘chromatograph’.

P17234L4: Do the sunlit branches and the shaded branches have a different limit of detection? Please describe the difference in methods (sampling flow, time, etc) that justify the difference. Also specify the detection limit for shaded branches.

The isoprene limit of detection expressed in µgC gDM$^{-1}$ h$^{-1}$ is the same for all samples taken from all branches (sunlit or shaded) during this study, since similar sampling and analytical methods were used for all our samples. However, because LMA was found to be different for sunlit and shaded branches respectively, we decided, in the initial manuscript, to express this value in µgC m$^{-2}$ h$^{-1}$ too: one for the sunlit branch and one for the shaded branch. In order to prevent any further confusion the isoprene limit of detection is now expressed in the new manuscript only in µgC gDM$^{-1}$ h$^{-1}$. Sunlit and shaded LMA values being explicitly given in section 2.3, readers can, if needed, easily convert the µgC gDM$^{-1}$ h$^{-1}$ into µgC m$^{-2}$ h$^{-1}$.

P17236: section 3.2 is hard to follow. Maybe you could do subsection for each VOC or VOC group to clarify...

As recommended, section 3.2 was re-organised. It is now divided into 2 sections, ‘section 3.2.1’ and ‘3.2.2’ dedicated to *Q. pubescens* and *A. monspessulanum* species respectively.

*Q. pubescens* BVOC emissions are now structured into different paragraphs. After general discussion, BVOC emissions are now presented according to their relative contribution (MeOH, total MT, Acetone, and MVK+MACR+aldehyde) into 4 different paragraphs.

BVOC observations made on Acer are also better structured (general findings are now pooled together and presented at the beginning, and a different paragraph was made for every new ‘idea’ (fraction of assimilated C, light and T influence …).

We do hope this improves the reading and understanding of this former section 3.2

P17237L1-2: Acetaldehyde and methanol are correlated to isoprene or to each other? Please clarify the text.

We apologize, but the correlation mentioned between acetaldehyde and methanol was a mistake from a former copy/paste version of the manuscript, and is now deleted.

P17238L16: in the morning the authors state that the fraction was greater than in the afternoon, but the percentage for the morning is 33% and for the afternoon is 66%. Please correct.

P17237L16 is not correct it should be: “P17238L16”.

It was indeed a mistake: the remaining fraction of BVOC was lower in the morning than in the afternoon as it is now stated in the revised version.

P17239L1-8: Isoprene emission has been suggested to not be constrained by stomatal closure because of its high vapor pressure (Niinemets and Reichstein, 2003).

We agree with this point and we have added a comment and the Niinemets and Reichstein (2003) reference.

P17239L7: by "twice weaker" you mean "half"? Similar expressions are found throughout the manuscript.

We have changed this point as mentioned.

P17239L8: This implies that all treated and control trees will be pooled and analysed together without regard to their control/drought status, right? Please clarify this in the text.

Clarification was made end of section 3.3 according to referee comment.

P17239: section 3.3.2 is somewhat difficult to follow and may be helped by a Figure showing the differences between sunlit and shaded branches, etc instead or in support of Table 3.

Indeed, the former Table 3 presents a large number of parameters, obtained at different dates, on different time scales, for different trees. After many tries, we ended up by selecting the 'Table' format to present them all, rather than numerous different figures which did not make easier to follow the 'actual story'. We thus preferred to keep the Table 3 as it was presented in the initial version, without any additional figure. Note that former Table 3 is now Table 2 in the revised version.

P17240L14-16: these lines should be situated right after the sentence that ends in line 10.

This change was made as mentioned

P17240L24 and onward: In my opinion, sections 3.3.3 and 3.4 should be in some way pooled together with section 3.5 into a new section, because all the data analysis described in these sections uses an emission algorithm for interpretation (Is, CL, and CT, in this case). So not only the current section 3.5 title should include an explicit reference to emission algorithms.

As suggested, sections 3.3., 3.4 and 3.5 belong now to a same new section 3.4 entitled: "Capturing *Q. pubescens* isoprene emission variability and providing estimates ". Former sections 3.3, 3.4 and 3.5 are now sections 3.4.1, 3.4.2 and 3.4.3 respectively.

P17242L12: Please clarify the last sentence.

Referee 1 also pointed out this point. We do hope that the changes made clarify the last sentences.

P17243: In agreement with Referee #1, the conclusions drawn from CL and CT parameters must be revised. For example, CL - with the parameterization used in the manuscript - practically saturates at PAR>1000 umol $m^{-2}$ $s^{-1}$. As shown in Fig. 4, PAR is above this threshold during most part of the day (Temperature is missing in Fig4 and this hinders a better judgment of the relative role of PAR and T). Did the authors check with their own data if isoprene ER saturated at PAR>1000 or instead isoprene ER continued to increase with increasing PAR?

As mentioned previously, the relative CL and CT parameters are no longer used and conclusions on that point were changed. In addition, light – or PAR – and temperature are employed in the revised manuscript rather than the, somehow ambiguous, CL and CT terms.

P17244 section 3.5: Can the authors calculate which Is factor provides agreement between the G93 and MEGAN algorithms and their measured isoprene ER?

We apologize, but, unfortunately, we are not sure we have understood correctly the reviewer comment: indeed, our Is factors were already the best agreement between G93 and measured ERiso since it is the best fit curve of measured ERiso vs CL×CT.

P17245L9-12: With the G93 and MEGAN models the diurnal cycle of emission is more or less captured, that is why the correlations obtained between measured and modelled values are so high. But the slope of those correlation is far from unity (e.g. <0.45, Figure 6) because the magnitudes of the emissions are really different as stated in the text. Please discuss further this fact when interpreting your data.

We agree with the referee #2. Consequently we added some further discussion which also replies to referee #1 comment concerning this section (RMSE is now considered for G93 and MEGAN performance comparison, and all comparison results are gathered in the new Table 3.

P17245L14-28: Do the authors have a measurement of the soil water content at deeper levels of the soil? Maybe just taking into account the first 10 cm is not valid for use with the current soil moisture component of MEGAN -used in this part of the analysis- which was designed basically to shut down emissions of isoprene when plants were under an exceptional drought stress. The information in the manuscript did not give me the impression that the plants were hydrically in such a bad shape, were they? This could be one reason for the poor results of the application of this mechanism to model the emissions from the oak branches in this study.

The depth of our water probe was the same as described in the Pegoraro et al., 2004 study (10 cm). This study was used to develop the drought parameterisation in MEGAN. Concerning the drought impact, we agreed that our tree were not hydrically in such bad shape, as mentioned in beginning section 3.3. Moreover, we had already a comment on the weakness of MEGAN in the previous version of the manuscript concerning the drought parameterisation in our conclusion and in the last section but one.

P172247L11: Change "contract" by "contrast"

Changes requested were made.

P172247L13-14: When the authors refer to PAR and Temperature remaining stable, are they referring instead to CL and CT? Please redo the discussion and interpretation about the relative role of PAR and T on the emissions of isoprene, as indicated previously in this review.

Indeed, PAR and T referred to CL and CL respectively, but we have changed by light and temperature effect as mentioned for previous comments. The relative role of CL and CT is now no longer considered as suggested in previous comments.

P172247L20-22: Information about soil which water content depth (-0.1m) used for calculations belongs to the discussion section (i.e. P17245L14-28) and not in the conclusions.

This section was moved to appropriate discussion section.

Figure 3: Temperature and PAR should be included in the figure.

T and PAR were not included in the former figure 3 as it made it too difficult to read; instead we have added a figure 3b. Former Figure 3 became then Figure 3a.

Figure 4: CL and CT should be shown, as well as Temperature, to allow the reader to interpret the data. If I understood correctly, Fig4 shows data obtained with a PTRMS. Can the authors prepare similar Figures showing data for other branches (measured with adsorption cartridges)?

As suggested by referee #1 as well, CL and CT relative contribution has been removed and only CL and CT are now presented and discussed in the revised manuscript. Concerning branches other than Qp4; since samples were manually collected using cartridges, we had not enough frequent data to produce a useful and accurate figure as for Qp4.

Figure 6: Temperature and PAR should be included in the figure.

As also suggested by referee #1, T and PAR were added in the (former) Figure 6 (now, Figure 7) of the revised manuscript.