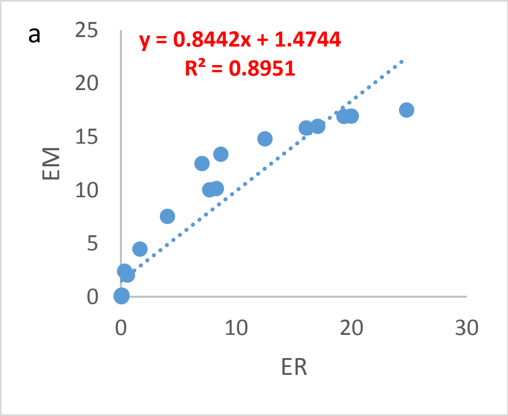
We thank the editor for all the useful comments which help us to improve our MS. We carefully analyzed the new comments, as shown hereafter.

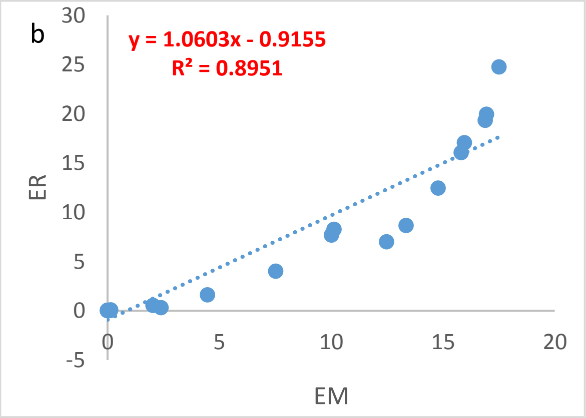
Your modification to the manuscript does not yet fully satisfy. In matter of fact, if I plot the (visually estimated) points in the first panel of Fig 3, I get slope of 1.06 (close to one, (intercept of -0.80, Matlab polyfit function), not 0.84 as in the manuscript. So, this makes the data processing still look a bit suspicious and the one sentence added to the revised manuscript does not fully cover this.

We have tried to understand your results (slope = 1.06 against our slope = 0.84) and realized that differences make sense since:

a) in our manuscript, the fit between modelled (EM) and measured emission rates (ER) are performed this way : EM = a ER + b (graph a). So, slopes and R^2 values shown in Figs 3, 4, 5 and supplementary figures, are calculated from these correlations. According to the example you chose, EM = 0.84 ER + 1.4744.
b) your calculations are obtained when ER = a EM + b (graph b) . So, ER = 1.06 EM - 0.9155. Note that R^2 = 0.8951 are the same in a) and b).

So, either way (EM = a ER + b or ER = a EM + b), the model slightly underestimates the emission rates. However, when plotting ER = a EM + b, this conclusion can only be reached if one accounts for the intercept, which is negative (-0, 9155). If only the slope value is taken into account (1.06), one would conclude that the fit is extremely close to 1. Thanks to your comments, we think that it is very important to clarify that in our manuscript ER was the x axis while EM was the y axis (line 184).





If I understand correctly, the only difference in correlating emission against CTCL, as you do for obtaining EF, and plotting emission against modeled emission, as you do to obtain slopes in Fig. 3-5, is that in the latter one you multiply the x-axis by EF (as E_model=EF*CTCL). Am I correct in this?

Indeed. As explicitly written in the manuscript (lines 167 - 169), the EF is calculated under each treatment as the mean of the emission factors for N= 5 trees/treatment. This approach allows to take into account the variability of emissions observed during our experiment.

Thus, in the case when you have no nigh-time emission (intercept = 0) the slope between modeled emission and measured emission should be close to unity. However, in several cases this is not the case (Fig 3, spring natural drought; Fig 4, several of the plots). Have you looked at how similar are the emission against CTCL plots to the emission against model emission plots? Could these be added as supplementary material?

For isoprene (Fig. 3):

In spring and under natural drought, R² of the correlation between measured emissions and C_l*C_t varies from 0.77 to 0.88 whereas, in summer when no underestimation was observed, R² is a bit better and varies from 0.73 from 0.97 which can explain the discrepancies between modeled emissions and measured emissions observed in spring.

For acetaldehyde (Fig. 4):

On the one hand, the burst of acetaldehyde in the early morning (Fig 4) is not explained by any algorithm (C_l*C_t or C_T), which can explain that slope is different from 1.

On the other hand, it is important to highlight that some trees showed weak correlations between their emissions and C_l*C_t (or C_T), although these correlations were significant. For instance, in summer and under natural drought, R² of the correlations between C_l*C_t and ER varies from 0.34 to 0.90 among the five trees studied (Table S6, new supplementary file), which can explain that the model did not fit well to measured emissions. These relatively weak correlations suggest that light and temperature are not the only factors driving acetaldehyde emissions in all trees and that there were large differences between trees. The discussion considers now this issue (see lines 242 - 247).

In order to highlight that tree BVOC emissions do respond differently to light and temperature (or temperature alone), the new version of the manuscript shows 6 new tables in supplementary file showing R² and p-value of correlations between C_l*C_t (or C_T) and experimental emissions of each compound, tree by tree (tables S1 to S6). This data is separated by seasons and treatments. To our knowledge, this type of information clearly misses in past studies while it is indeed very relevant since it reflects the natural variability among trees growing under the same natural conditions.

Because of the above mentioned possible discrepancy in the data processing, I feel the following sentence, "The modelled emissions were very representative of measured emissions except in spring for natural drought when we obtained a slight underestimation of emissions (sl = 0.84, P < 0.05) maybe, because light and temperature, in spring, were not the only parameters driving isoprene emissions", may be over-interpretation.

According to the editor comments, we changed this sentence in the manuscript as follows:

Modeled emissions were roughly very representative of measured emissions. We note however that in spring, under natural drought, emissions were slightly underestimated (sl = 0.84, R^2=0.90, P < 0.05). This result suggests that although light and temperature remain the main factors driving isoprene emissions in spring, other parameters explain 10 % of isoprene emission variability. In spring, plants likely require to produce more isoprene to protect the establishment of the photosynthetic machinery in the new leaves which could slightly modify the effects of light and temperature on isoprene emissions (lines 222 – 227).

Page 8, lines 283-285: "Predicting emissions rates of these 3 compounds (methanol, acetone and formaldehyde), during the night, seem to require other parameters such as a temperature threshold, below which methanol, acetone and formaldehyde synthesis and so emissions do not occur (Ghirardo et al. 2010)". You should remove citation Ghirardo et al. from this as that paper does not deal with methanol, acetone or formaldehyde.

As suggested, we removed this reference from the manuscript.