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# *Interactive comment on* "Simulating precipitation decline under a Mediterranean deciduous Oak forest: effects on isoprene seasonal emissions and predictions under climatic scenarios" *by* Anne-Cyrielle Genard-Zielinski et al.

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Responses to Referee #2

We do thank Referee #2 for her/his careful reading and useful comments and suggestions.

a. General comments

- Use of literature values instead of site-observed data: G14 was tuned using O3HP



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data, not literature data; this point is now more clearly explained in the new section 2.5.

- No discussion of potential uncertainties from G14 and ANN in general: details are now given in the revised section 2.6 and discussed in the new section 4.3. (Among the other available statistical methods, ANNs present the advantage of being the most parsimonious (e.g. giving the smallest error for a same number of descriptors; see for instance Dreyfus et al., 2002). Moreover, ANN approach, as the other non-linear regression methods, is not, or not very, sensitive to regressors' co-linearity (Bishop, 1995; Dreyfus et al., 2002). One of the ANNs limitations is that they can be used only for interpolation, not extrapolation exercises. For this reason, our future RCP projections we made using only xi values that did fit into the range of variation of xi obtained during the training phase; in total 21% of data were thus rejected. ANN optimization during the training phase was based on the reduction of the root mean square error (RMSE) between calculated and measured values. Our final optimized RMSE (validation data) was 8.5 $\mu$g gDW-1 h-1 for our ER values ranging between 0.06 and 113 $\mu$g gDW-1 h-1, and represents 35% of the mean (22.7 $\mu$g gDW-1 h-1)).

- No comparison with process based model: as mentioned in the first paragraph, p3 of the original document, process based model require a complex set of data to be ran; such a dataset is not available for this study. This is the reason why an 'empirical' model (MEGAN) was solely tested.

- No conclusion and no clear key messages: a new conclusion has been added, and key messages are now given.

- Abstract:

Too many abbreviations: we cannot use less than the 4 remaining abbreviations (ER, ND, AD, G14) if we want to keep the abstract as concise as possible.

Sentences were added at the beginning of the abstract in order to better explain 'why we need this study'.

- Method:

No description of MEGAN: a complete description of the MEGAN2.1 model and how it was tuned in our study is now given in a new specific section 2.5.

Better description on how ORCHIDEE data were used and how G14 was tuned: the purpose of running ORCHIDEE and how the ISI-MIP and ORHCIDEE derived data were used in our G14 algorithm is now detailed in sections 2.6, 2.7, 2.8 and 3.4.

A more detailed description on how AD roof was operated is now given in section 2.1.

- Results:

Use of on-site data to apply MEGAN rather than 'literature' data: detailed information is now given in the new section 2.5.

- Discussion:

Long text on future ER although no consideration on C source + training period for G14 is too short: C source was indeed not directly considered since the hypothesis we made in applying a neuronal approach to statistically analyze our data was to consider 'simple' integrative environmental parameters; C source, nor vegetation adaptation, were thus not considered, but in fine we hypothesized that they are, more or less indirectly, driven by PAR, T, SW and ST fluctuations over a range of different frequencies.

As vegetation adaptation is concerned, we did discuss this point at the end of the original section 4.3 (p 17) since we are aware that this aspect is of importance. This point is also mentioned in our revised and new section 4.3.

Thank you: we do agree that our database was not large enough to really assess impacts under future climates such as RCP2.6 and RCP8.5. In the revised manuscript, the future climatic data are now used only to test the sensibility of isoprene emissions to T, P and T+P changes.

No conclusion: As mentioned earlier a proper conclusion is now given at the end of the





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manuscript.

b. Detailed comments

- ‘gaz’ has been changed to ‘gas’.

- P3, L5, References for empirical-based model: Ashworth et al. (2013) is actually a review where both types of models (empirical and process based) are described.

- P3, L31 and P3, L32: this part of the introduction has been changed, but we made sure that to explain an abbreviation when used for the first time.

- P4, L3-8: thank you; the scientific questions considered in our work are now better explained at the end of the introduction, and in the “results” and “discussion”.

- P4, L26: this sentence has been rewritten at the beginning of the revised section 2.2.

- P4, L27: ‘except from’ has been changed as requested in the revised section 2.2.

- P5, L22: only a small fraction (<5%) of the data was missing ; of course, the bias between Pcum curves at both sites was assessed and considered to ‘extrapolated’ the missing values at the O3HP site. As the precipitations were cumulated over 7, 14, 21 days, the bias was negligible (around 1%) and no adjustment was made. This information has been added in the revised section 2.7.

- P6, L18: AF is dry leaf mass per area conversion factor, called LMA in the previous line. AF was hence changed by LMA in the revised section 2.3 (former section 2.4).

All the abbreviations cited were homogenized, in the text and in the figures.

- P6, L23: Cout and Cin order has been changed in the revised section 2.3.

- P8, L1: How Krustal-Wallis test detects seasonal variation? Kruskal-Wallis tests allowed to check for different median values among months. ANOVA tests could not be used since data did not follow the requirements of parametrical tests. This information has been added in the revised section 2.4 (former section 2.5).

- P9, L31: we clarified in the revised section 2.1 how the rain exclusion of 30% was achieved along the seasons.

- P10, L2-5: although, in the absolute, the gap between Pcum(AD) and Pcum(ND) curves increases toward the end of 2012, from July 2012 the relative difference between both curves was maintained at around 30 % (fig. 1c: 510 and 760 mm respectively, thus a 33 % reduction) .

- P10, L11/P5 L17-19: we clarified in the revised section 3.1, why, although PAR measured in the COOPERATE database was made on one point above the canopy, the PAR actually received by the enclosed branch in the chamber could be different from one branch to another, especially in September 2012,during the autumnal sun setting.

- P10, L22-23: this line has been removed.

- P11, L2: 'P=3.9' was a mistake and was removed.

- P11, L5-9: because the measurements were made right during the beginning of the isoprene onset period, some of the sampled branches started to emit isoprene significantly, while others emitted only at a very low level; this led to a large variability that could not be significantly related to the relative position of the branches in the AN or ND plots. Further precision is now given in the revised section 3.2.

- P11, L13-14: these points have been already mentioned in our general responses and are now detailed in the new section2.5 dedicated to MEGAN 2.1 description.

- P11, L18: specific regression lines/$R^2$ are now given when needed in the document and/or in the Fig or Tables.

- P12, L6-7: thank you, our text was indeed not precise enough; we meant that under AD 'the contribution of the two lowest frequencies (-14 and -21 d.), was, RELATIVELY to the contribution of the two highest frequencies (instantaneous and -7d.), higher in April (48%) and June (700 and 40 % in 2012 and 2013 respectively) than during the summer (22, 8.5 and 16 % in July, Aug. and Sept. respectively)'. The text has been

changed accordingly in the revised section 3.3.

- P12, L9: the contribution to each frequency has been converted to ratio and is now presented in the revised fig. 6 (former Fig. 4).

Note that the discussion concerning the relative contribution of each environmental parameter is no longer made: indeed, SW appeared as the 'major' driving regressors tested in G14 . . . since this was the ONLY regressor that differed between both sample sets (ND and AD).

- P12, L30: this section has been completely re-written, but we made sure to use correct number of RCPs in the revised text.

- P12, L31; P13, L17: these parts of the text have been completely re-written.

- P14, L5: thank you, '$\mu$gC gDM-1 h-1' is now the only unit used for emission rates throughout in the revised text.

- P14 L28-31 and P15, L21-24: as explained earlier, and in our General Responses, the 'MEGAN adjustment' to our local data is now explained in the new section 2.5.

- P16 L2-3: 'degree day' unit was changed to 'Dd' and explained when it appeared for the first time in the revised section 4.2.

- P16, L12: soil water description by land models is no more addressed in the revised manuscript.

- Section 4.3: as mentioned in our General Responses, this section has been completely re-written. As suggested, sensitivity tests are now presented.

Concerning the comparison with other literature data, in the new conclusion we are now insisting on the fact that there is actually no in situ isoprene data comparable to our O3HP database; however, in the new section 4.3 we are now giving more details on the findings obtained during a seasonal study carried out on isoprene emission from Q. pub. saplings by Genard-Zielinski et al. (2015). In order to extend our discussion

on species emitting other BVOC than isoprene and compare our results to other in situ drought study in the Mediterranean area, we are now reporting in the revised section 4.1 the Lavoir et al. (2009) study.

- Figure1: an x axis legend has been added also on the top of the (a) graph in order to make the reading easier.

- Figure 2: letter 'c' and 'd' meaning is now added in the revised figure caption of figure2.

Indeed, differences between ND and AD using Mann-Whitney tests should be denoted with asterisks at the top of each figure. Differences among months were tested using Kruskal Wallis tests (W) followed by followed by Newman-Kruels post-hoc test with a<b<c<d. These corrections have been made in the revised Figure caption.

Note that the Gw unit should be 'mmol cm-2 s-1' as indicated in the revised document and not 'mol cm-2 s-1' as previously written.

- Figure 3: former Fig. 3 was removed and replaced by a new one.

However, on this figure and others, log scale was not always suitable since, as explained in the discussion, many ER were set to zero by MEGAN2.1.

- Figure 5: Former Fig. 5 was removed and replaced by a new one.

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