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Discussions

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.

Anonymous Referee #3

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This manuscript presents the findings of an analysis of isoprene emissions from oak trees measured during an extended field campaign in a Mediterranean forest. Trees were sampled from two plots, one covered to intercept rainfall and thus subjected to artificial drought conditions and the second left open and therefore representative of normal water conditions. In this region, even the trees in the control plot experience drought during the summer months. Artificial neural network analysis was used to determine the meteorological and physiological parameters that most strongly influence isoprene emissions throughout the course of the growing season. The network was optimised and tuned for the site, and its skill assessed using reserved measurements

taken from the same trees and sampling period.

With dry regions projected to become drier and experience more prolonged and intense periods of drought in the future, it is important to understand the response of native vegetation to such conditions. If we are to assess the potential impact of changes in vegetated ecosystems on climate, air quality and the Earth system in its entirety, it is equally important to improve the skill of current models in capturing the processes and interactions under present-day conditions. A study that combines observational data with model evaluation and development would therefore seem to represent an ideal approach. However, the study presented here, appears to my mind to have major weaknesses in design and implementation that preclude publication in its current form. Considerable work would be required to sufficiently overcome these limitations.

I also have concerns regarding the presentation of the work, results and conclusions in terms of both language and style. Given the degree of re-working and re-writing required I will limit my comments to suggesting that the authors would do well to have the manuscript proof-read and edited by a native English speaker as there are times when the grammar and choice of word make it hard to follow. In addition, the way in which the results are presented is often non-sequential, jumping from one variable to another rather abruptly and without clear logic again making it hard to follow. There are also times where the authors appear to contradict, or overlook, points they have made earlier in the article but it is hard to judge whether this is a consequence of my misunderstanding them.

My main concerns are outlined below:

Background and citations The authors appear unaware of a large body of work previously conducted into the effects of drought on photosynthesis, cellular processes and emissions of volatile organic compounds such as isoprene. The studies they cite are narrow in focus and scope. While this is of course appropriate when reporting specific emissions factors or observations from similar ecosystems, I would expect to see a far wider discussion and consideration of previous modelling studies particularly those that apply "MEGAN" to simulate isoprene emissions. For example, the authors do not appear to consider the excellent evaluation of the skill of these empirical algorithms in capturing observed seasonal variability of isoprene emissions (Müller et al., 2008) which also investigated whether this was attributable to soil moisture and the implications for estimates of global emissions; nor the assessment of the treatment of wilting in the MEGAN algorithms (Sinderalova et al., 2014).

Measurements 1. It appears that measurements of PAR and temperature were almost exclusively made above the top of the canopy. It is not clear to me that this would be representative of the conditions within the forest canopy, i.e. those experienced by the majority of the foliage. While the authors do state that the enclosed branches were toward the top of the canopy net primary productivity and canopy stomatal conductance reflect conditions experienced by all leaves. Furthermore, photosynthesis, respiration and isoprene emissions are all strongly affected by actual received radiation and leaf temperature yet the authors confine their analysis and model development to air temperature and top of canopy radiation. How have they accounted for shading within the canopy, which will affect leaf temperature as well as available light? Or the occurrence of sunflecks?

2. How exactly are induced drought conditions achieved? How is the roof operated? The overall effect of the deployment of the roof might be the roughly 30% reduction in annual precipitation expected in the area under climate change but how is this reduction distributed? Evenly, i.e. a 30% reduction every day or every month? It is not just total rainfall that affects vegetation physiology and phenology, it is also the temporal pattern. The overall drying of the region is expected to result in more severe prolonged droughts interspersed with periods of increased intense rainfall. Is this reflected in the artificial drought conditions produced at the site?

3. Too little detail of the sampling strategy is given. A full list of the dates of isoprene measurements is required. It would seem to be far from the one week per month from

June 2012 to June 2013 that is stated. It is also unclear whether cartridge samples continued to be made when the PTR was deployed, and if so whether they were also taken at multiple heights. If the techniques were deployed in parallel were the data compared for consistency? How were the data from lower in the canopy included in the analysis, given the issues I have raised in point 1?

4. More details should be given of the use of the data from COOPERATE. Specifically which parameters were selected and on what basis? How exactly were precipitation data from the nearby site used to gap fill the COOPERATE data? Precipitation is highly heterogeneous in both time and space, particularly in mountainous regions such as the Haute Provence. Were data compared from times when both datasets were available?

Statistics 1. "MEGAN" - The algorithms referred to as MEGAN have evolved over time from the initial parameterisations based on leaf-level emissions measured under controlled laboratory experiments presented in 1991 and 1993 by Guenther et al. Over time these have been extended and adapted to represent canopy-scale emissions. The authors appear unaware of the major differences implied by the changes, and are inconsistent in the set of algorithms they choose to apply. The authors begin by back-calculating isoprene emission factors for each month under standard conditions – using the leaf-level parameterisations which is entirely appropriate. (Although, as the authors specifically refer to "CL" and "CT" on numerous occasions I feel that they need to present the equations they are using here in the text rather than referring the reader to the original papers.) It should be noted that other studies have found wide variation in "standard" emission factors between different leaves on the same tree, let alone different trees yet the authors have sampled only a single uppermost branch from each tree as noted above. Later, the authors switch to using the MEGAN algorithms from 2006. However, they appear unaware that firstly these algorithms use different emission factors from the leaf-level ones calculated from the 1991/3 parameterisations as they account for canopy architecture (shade and sun fractions, leaf angle distribution, vertical distribution of foliage) and in-canopy losses. To compare emission rates estimated with MEGAN against the measured single branch emissions does not appear a "fair" assessment (given the different approach taken to assess the estimates from the "G14" algorithms – see below) MEGAN (both v2.0 and 2.1) include a dependence on "historical" PAR and temperature, the average conditions over the previous 24 hours and 10 days. This is similar to the findings of the artificial neural network analysis here: that generally emissions are most sensitive to fluctuations in driving data occurring over a period of 7-14 days, but that at times very short time-scale processes dominated the effect. The authors have not acknowledged this feature of the MEGAN algorithms and in fact appear to state that there is no "memory" in MEGAN.

2. Using an artificial neural network approach to deduce an algorithm for isoprene emissions essentially produces a best-fit parameterisation that is tightly tuned to a single site and single time period. One would expect such a parameterisation to show skill for those specific conditions. Given the sparseness of the data used for this tuning the robustness of using the resulting model to estimate emissions under different conditions is not self-evident. At the very least, data from a much longer time period (and ideally more than one location) is required to give confidence of the capability of the model to capture e.g. emissions in 2100 under RCP8.5.

3. The artificial neural network approach is limited in that while it highlights the variables to which, in this case, isoprene emissions are most sensitive, it does not provide insight into the fundamental processes that influence the emissions most strongly. Therefore, it is hard to use the knowledge gained to improve existing understanding or modelling.

Modelling Which brings me to what is probably my chief concern, the inconsistency in the two approaches to modelling emissions under future conditions. The "G14" algorithm, the optimised statistical parameterisation derived from the artificial neural network analysis, is tuned specifically for this site, this time period and these environmental conditions. and the authors demonstrate that it performs well for this site, this time period and these environmental conditions. By contrast, and in spite of the fact they have site-specific data available, they apply the MEGAN algorithms in the default

form, i.e. with generic emissions factors and wilting point threshold. Why? It is not clear what we gain from this assessment. Müller et al. 2008 and Potosnak et al., 2014 have already demonstrated that when applied in this way, MEGAN does not capture observed seasonality or response to water stress, and Sinderalova et al., 2012 show that changes in soil wilting point threshold can alter estimated global isoprene emissions by up to 50% annually. It would have been far more valuable and far more consistent had the authors taken the same approach as they did with G14 that is to "tune" the model to the site. How does MEGAN perform if the site-specific monthly varying isoprene emission factors are used? Or if the authors experiment with different wilting point thresholds? Given that G14 is not to my mind robust enough to apply under different conditions we are still left with a need for a set of algorithms that can be applied globally and over extended time periods, such as MEGAN. However, I acknowledge that we do need to improve the skill of these global algorithms to replicate observations, particularly under periods of environmental stress that could be anticipated to occur more frequently in the future. The authors spend too much time presenting and discussing the results of future simulations using the G14 algorithms in light of the weakness of applying such a tightly tuned model under future conditions. Better still if the authors were able to leverage the G14 artificial neural network analysis to develop a process-base model of the effect of soil moisture on isoprene emissions, or contribute to efforts to improve such a model, e.g. Gröte et al., 2010.

ORCHIDEE – It's not clear whether the authors used ORCHIDEE to estimate soil moisture content and soil temperature for the present-day as well as under the RCP scenarios, given that these variables were measured on-site (in fact, around the base of each sampled tree) throughout the growing period. What met data were used to drive ORCHIDEE future projections? What downscaling techniques were applied to back out high-resolution precipitation and other meteorological variables for the location of the measurement site?

My recommendation to the authors would be to use the site-specific data to deduce monthly emissions factors and wilting point values to “optimise” the performance of the current (i.e. Guenther et al., 2012) version of MEGAN for this site, and to evaluate the optimised model. But to concentrate their efforts on using the artificial neural network analysis to attempt to gain insight into the fundamental processes that give rise to the observed changes in isoprene emissions. At present they are only able to draw on hypotheses from previous studies to try to explain the variations but cannot support or repudiate the hypotheses. I would suggest they use the same approach to explore the drivers of other physiological parameters: stomatal conductance, sap flow, transpiration, water, carbon and energy fluxes to determine whether the responses of any reflect the same drivers as isoprene emissions (including frequency / speed of response).

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