Responses to Referee #2's Comments

We thank Referee #2 for his/her time and consideration. We closely examined his/her insightful and constructive comments which have considerably helped us to improve the manuscript.

Referee #2's comments are quoted in bold. Authors' answers are in regular font and authors' changes in the manuscript are quoted in italic.

We refer to the marked-up manuscript version for section numbers and pages.

This is an interesting manuscript presenting an alternative global emission model to MEGAN as well as some insights into the mechanisms of such models. It is well suited to the journal and overall well written although grammar and language should be improved in some occasions. There are, however, a couple of open questions/problems that should be clarified along with a number of minor issues before accepting it for ACP.

Author: We checked the text, also following the Referee #1's remarks, in particular the use of article "the", the present and past tenses, some prepositions (in, at, for, with…), the misspelled of needleleaf. We uniform all the text in UK English. The manuscript was read by two English native speakers to reduce, as much as possible, the grammar mistakes.

First, I disagree with the argument that the current available information about Emission Factors is sufficient for statistical significance as stated in the introduction (P33971, L19). As far as I can see this is only valid for very few species while for many others only very few measurements can be found. The question is, however, if the available EFs are sufficient to characterize the representative species for a PFT. Although the authors point out the difficulties of PFT parameterisation (and among these I miss the one that PFTs are of variable species composition) they are obviously of the opinion that they have overcome these difficulties. But how were these PFT specific EFs actually derived? All what is presented is Table 3 showing one EF per PFT and a list of references with varying detail. I would like to illustrate this point: In the ORCHIDEE model description, the authors say they have determined an isoprene emission factor of 0.5 for the boreal needleleaf deciduous PFT (=Larches) based on Levis et al. 03 (EF 0.0), Guenther et al. 06 (EF 0.7), Karl et al. (EF 0.0), Steinbrecher et al. 09 (EF 0.0), and Steinbrecher et al. 13 (only oaks in here). So how does this work out? One of the problems seems to be that only secondary sources are used which in

turn partly use the same original investigations. It would be more logical to fall back on primary literature sources – preferably new ones or at least complemented by new ones (e.g. Ruuskanen et al. 07, Ghirardo et al. 2010). So, which measurements from which species were used to derive which PFT and how is it done? This is probably an issue for a supplement.

Author: We agree with the Referee #2. Our argumentation is overstated considering the methodology used in our paper. Nowadays we can only say that there are more observations and a larger number of compounds measured. We change the text consequently in section 1, page 4 (see §1).

Assigning EFs, especially for the global scale purposes, is a very tricky issue, and the methodology to be used is still under debate within the scientific community. In an ideal case, for each compound emitted, we should consider EFs of all plants belonging to one particular PFT and the land cover of each plant. We could then, for each PFT and compound, make averages weighted on plant land cover, thus getting an average EF for each PFT and emitted compound. Unfortunately, there are not yet enough observations available to use such a methodology.

The approach used to derive EFs is not fully detailed in the manuscript. It represents an important part of the upgrade of the emission module, but it is not the central issue of the work. All values used are available in the papers and listed in Table 3. We do not use a new statistical methodology or consider original measurement that deserves to be particularly detailed. Actually we use a qualitative and comparative method to attribute the EF values. However, we agree with the Referee #2 that it is important to be more precise in the manuscript, in order to better understand the procedure used and the difficulties encountered in assigning the new EFs. We specify more clearly why, at present, in the modelling community, statistically valid methods to assess the EF have not been developed yet. To put more attention on this issue we dedicate the new section 2.2.1 (see §2).

About the isoprene EF related to boreal needleleaf deciduous tree, we consider the following EF values, that we converted (if necessary) into the proper units used in ORCHIDEE:

- EF = 0.44 μgC g^{-1} h^{-1} (in original unit: 0.5 μg g^{-1} h^{-1}) in the supplement of Steinbrecher et al. (2009), (Table 2). The values come from an extensive review.
- EF = 0.44 μgC g^{-1} h^{-1} (in original unit: 0.5 μg g^{-1} h^{-1}) in Karl et al. (2009) (Table 5). They based their values on Steinbrecher et al. (2009).
- EF = 0.0 μgC g^{-1} h^{-1} in Levis et al. (2003). They assign for needleleaf deciduous trees the emission rates recommended for larch species by Guenther et al. (1994).

- EF = 8.0 μgC g^{-1} h^{-1} in Fu and Liao. (2012) taken from the literature, but they do not say from where.
- EF = 1.44 μgC g^{-1} h^{-1} (in original unit: 0.7 mg m^{-2} h^{-1}) in Guenther et al. (2006). They assign an average over all needleleaf deciduous tree.
- EF = 0.09 μgC g^{-1} h^{-1} (in original unit: 0.1 μg g^{-1} h^{-1}) in Smiatek and Steinbrecher (2006). The value is for larch only.
- EF = 0.002 μgC g^{-1} h^{-1} (in original unit: 1 μg m^{-2} h^{-1}) in Guenther et al. (2012). The value comes from a review.
- EF = 0.52 μgC g^{-1} h^{-1}(= 221 μgC m^{-2} h^{-1}) in Klinger et al. (2002). The value comes from measurements.
- EF = 1.44 μgC g^{-1} h^{-1} in Lathière et al., 2010. They use the same value as in Guenther et al. (2006).
- EF = 8.0 μgC g^{-1} h^{-1} in Arneth et al. (2011). They adopt this value for the model inter-comparison.

In these papers the values, which come from a review or measurements, are: Guenther et al. (2006) (EF = 1.44 μgC g^{-1} h^{-1}), Guenther et al. (2012) (EF = 0.002 μgC g^{-1} h^{-1}), Steinbrecher et al. (2009) (EF = 0.44 μgC g^{-1} h^{-1}), and Smiatek and Steinbrecher (2006) (EF = 0.09 μgC g^{-1} h^{-1}) and Klinger et al. (2002) (EF = 0.52 μgC g^{-1} h^{-1}). All these values are much more lower than the ones assigned by Lathière et al. (2006) (EF = 8.0 μgC g^{-1} h^{-1}) and the average is 0.5 μgC g^{-1} h^{-1}, which we set as the new value. In this case we do not consider the other papers because they based their choice only taking into account other previous model setting or the source of information was not clear. We report a part of this discussion, as an example of the procedure used in section 2.2.1 (see §2).

In our review we did not consider the paper of Ruuskanen et al. (2007). They assign a contribution of less than 3% of the VOC emission to isoprene, 2-methyl-3-buten-2-ol (MBO) and 1,8-cineole, for larch. Anyway, they confirm that boreal needleleaf deciduous PFTs, which are mostly composed by larch, are very low isoprene emitters. We mention this work in section 2.2.1 (see §2).

§1 *"Nowadays, a large number of measurements is available for different plants and at various sites and there is an increasing number of field campaigns that investigate, in addition to isoprene and bulk monoterpenes, many other important compounds for atmospheric chemistry, especially regarding the SOA formation, such as speciated monoterpenes and sesquiterpenes. More data and information are therefore available, allowing EF estimates for a wider range of BVOCs, despite the limitations which we will discuss in section 2.2.1."*

§2 *"**2.2.1. Emission Factors update***

EFs represent one of the greatest sources of uncertainty in the quantification of BVOC emissions (Niinemets et al., 2011). Several measurement campaigns were carried out over the last decade, giving important insights and information for re-examining thoroughly the emission factors used in the emission module and correcting them accordingly. Nevertheless the methodology to assess EFs is still under debate within the scientific community.

Assigning EFs, especially on the global scale, is very tricky. In the ideal case, for each compound emitted, we should consider the EFs of all plants belonging to one particular PFT and the land cover of each plant. We could then, for each PFT and compound, make averages weighted on plant land cover, thus obtaining an average EF for each PFT and emitted compound. Unfortunately, there are not yet enough observations available to use such a methodology.

There are several factors that make it difficult to find a good strategy to assign EFs valid for all compounds:

1. depending on the compound and the PFT, the number of measurements available differs considerably, and the statistical accuracy of the EFs may therefore be very variable;

2. in some cases, the most recent measurements contradict the older ones, therefore it is reasonable to consider only the most recent data. However, in other cases the difference between recent and older measurements is not so clear, therefore it is not easy to understand if it is better to consider less recent measurements in the evaluation of EFs;

3. considering the values of EFs that we collected from the literature, we note that they are actually often related to a small number of plant species from mostly the same measurement sites. The values found could not be considered as a significant representative set for the PTFs at the global scale;

4. in many papers focussing on modelling, the EFs presented are either taken directly from previous models, or are based on a review or on measurements available. In this context, it is very difficult to make consistent averages and understand which values found should be taken into account.

Taking all this into account we decided to proceed as follows.

As general rule, and based on an extensive review of publications, we select papers, in which it is possible to convert the EFs into the units and at the standard conditions that are considered in ORCHIDEE (PAR = 1000 μmol m^{-2} s^{-1}, temperature = 30 °C). We do not always perform an *average over all values collected, but we use a qualitative and comparative method to justify the EFs.*

In the case of isoprene, we principally consider the most recent papers, the ones that present new measurements or original review. The review carried out for EFs confirms that the values used in the previous version (Lathière et al., 2006) are consistent with the latest measurements. Only for certain PFTs it is necessary to change the value of EF. Indeed, isoprene has already been widely measured for several years, while other BVOCs have been documented only more recently.

In the case of the other compounds, since there are fewer papers and the information is not so well consolidated, we adopt a similar strategy but we are less restrictive in paper choice. In general, we perform averages considering the different values from all papers collected, and we compare these averages to the older values in ORCHIDEE. Whenever big differences between the new value and the old one were found, we look in detail at the various papers to see if there are some outliers, and if so, we do not consider them in the EF evaluation.

Table 3 show the new and old EFs used in the emission module and Table 4 presents EF values for each speciated monoterpene as a percentage of the bulk monoterpene EF value. As shown in Table 3, the revision leads to the modification of almost all EFs. In some cases, the EF differences in comparison with the previous version are very significant. Regarding isoprene, boreal needleleaf deciduous PFT is now recognized as a less important emitter (EF = 8 μgC g^{-1} h^{-1} in the old version and EF = 0.5 μgC g^{-1} h^{-1} in the new one). We based the choice on papers focussing on reviewed or measured EFs, such as Guenther et al. (2006) (EF = 1.44 μgC g^{-1} h^{-1}), Guenther et al. (2012) (0.002 μgC g^{-1} h^{-1}), Steinbrecher et al. (2009) (EF = 0.44 μgC g^{-1} h^{-1}), and Smiatek and Steinbrecher (2006) (EF = 0.09 μgC g^{-1} h^{-1}) and Klinger et al. (2002) (EF = 0.52 μgC g^{-1} h^{-1}). All these values are much lower than those assigned by Lathière et al. (2006), and their average is 0.5 μgC g^{-1} h^{-1}, which we set as the new value. In this case, we do not consider the other papers where EFs are directly taken from previous models or for which the source of information was not clear. Our choice is confirmed by Ruuskanen et al. (2007), who assign a contribution of less than 3% of the VOC emission to isoprene, 2-methyl-3-buten-2-ol (MBO) and 1,8-cineole, for larch, which is the major component of boreal needleleaf deciduous PFT.

Furthermore, we now consider boreal broadleaved deciduous trees to be a higher emitter of isoprene than in the previous model version (now EF = 18 μgC g^{-1} h^{-1}, while before EF = 8 μgC g^{-1} h^{-1}), since most of the papers collected propose particularly high values, such as Levis *et al. (2003) (24 μgC g^{-1} h^{-1}), Arneth et al. (2011) (45 μgC g^{-1} h^{-1}), Guenther et al. (2006) (42.3 μgC g^{-1} h^{-1}) and Guenther et al. (2012) (22.7 μgC g^{-1} h^{-1}). For monoterpenes, a significantly higher EF (from 0.8 μgC g^{-1} h^{-1} to 2.2 μgC g^{-1} h^{-1}) is now assigned to tropical broadleaf evergreen and deciduous PFTs. For 2–methyl–3–buten–2–ol (hereafter we refer to it simply as MBO) the EF for the temperate needleleaf evergreen PFT is reduced from 20 μgC g^{-1} h^{-1} to 1.4 μgC g^{-1} h^{-1} (Tarvainen et al., 2005; Hakola et al., 2006; Chang et al., 2009; Kim et al., 2010).*

Our review analysis confirms a large variability in EFs, even among plants that are usually represented by one single PFT in global vegetation models (characterized by the same physiognomy, leaf shapes and photosynthesis type). It is therefore a source of high uncertainty to assign one fixed EF value for each PFT in global models, as also pointed out by Kesselmeier and Staudt (1999) and Arneth et al. (2011). Moreover, the procedure used to determine emission factors from field measurements adds an additional source of uncertainty. Indeed EFs are derived by adjusting the measured flux at leaf level at a standard conditions of light photosynthetically active radiation (PAR) and temperature, using algorithms such as Guenther et al. (1995). However, there is no universal agreement on the parameterization of these algorithms (Tarvainen et al., 2005; Duhl et al., 2008; Kim et al., 2010; Bracho-Nunex et al., 2011; Fares et al., 2011).”

Second, I am a bit surprised that LAI is more or less stated to be wrong in ORCHIDEE already in 2011 (P33990, L1) but has not been improved since although the deviation to measurements is very large and it is discussed (and demonstrated) to be a very important driver for emission. There is a bit of discussion about uncertainties in measurements but I feel that the paper doesn’t dare to claim that the ORCHIDEE simulations are as valid as the MODIS derived values. However, if the MODIS data are considered ‘state of the art’, then I see three options to proceed: 1. Improve the LAI simulations, 2. Improve the argumentation to a degree that the reader can accept ORCHIDEE simulations as equally likely as MODIS data, or 3. Run all simulations with MODIS derived values only. Option 3 seems the most feasible to me.

Author: Our current state of knowledge does not allow us to say which of the two methods give more realistic LAI values: the retrieval from the MODIS satellite or ORCHIDEE calculation. The satellite actually measures the *effective* LAI and not the *real* LAI (Pinty et al. 2011; Fang et al. 2012a; Fang et al., 2012b). LAI is obtained from indirect optical methods and strongly determined by the *a priori* assumptions necessary to perform the inversion procedure. We detailed the uncertainties of LAI satellite measurements and ORCHIDEE estimation in the first part of section 3.4, pages 28-29 (see §3).

In addition, we mention that ORCHIDEE is designed to provide future scenarios of emissions from vegetation, studying the links among climate, the plant phenology and emissions. Therefore, the main concern is rather to study weaknesses in LAI modelling and eventually improve it and not to force it with other LAI estimates. A new version of ORCHIDEE model is going to be developed, including a more detailed hydrological scheme, a complete nitrogen cycle and an higher number of forest PFT, where any possible weaknesses of LAI estimates could be solved. We specify it more clearly at the end of section 1 (see §4).

§3 *"The differences between these LAI estimates are significant, but our current state of knowledge does not allow us to say which estimate is correct. Field and satellite data bring very useful and complementary information regarding the order of magnitude, the seasonal and the geographical variability of LAI. Nevertheless, inferring values for LAI on small or large regional scales is particularly challenging, and data available from, either field or satellite measurements also have significant uncertainties. Satellites, for instance, do not measure the real LAI, but the effective LAI obtained from indirect optical methods and strongly determined by the a priori assumptions necessary for the inversion procedure. Even starting from the same input reflectance, diverse retrieval methods can lead to LAI values that are highly different (Garrigues et al., 2008; Fang et al., 2013). The effective LAI can be very dissimilar to the LAI directly measured in situ and relative differences can reach 100% (Fang et al. 2012a, b).*

The transition from effective to real LAI is possible only when additional information about the vegetation structure is available (Pinty et al. 2011), increasing the risk of inaccuracy. The sources of uncertainties are numerous (Garrigues et al., 2008). First, foliage clumping is, in general, not taken into account, leading to underestimates of LAI of up to 70% over the coniferous forest. Second, the forest understory is not systematically taken into account since the satellite LAI product is derived from a vertical integrated radiation signal. Third, in dense canopies, such as broadleaf tropical forests, the optical signal can saturate, leading to an underestimate of the effective LAI in comparison with the true value with a saturation limit of 3.0 m^2 m^{-2} (Pinty et al. 2011). Forth, the presence of ice and snow can strongly upset LAI retrieval, making it very difficult to estimate LAI in boreal and mountain regions.

Conversely, in a validation study using satellite-derived vegetation index time series, Maignan et al. (2011) pointed out some weaknesses in the ability of ORCHIDEE to correctly model the

LAI, especially in the equatorial forest (Amazonia, central Africa, Indonesia) where a poor correlation of model output with satellite data was demonstrated. In general, quite large and comparable incertitude is found when different LAI databases are compared. Krinner et al (2005) found that the difference between ORCHIDEE and MODIS satellite LAI (Myneni et al., 2002) is as much as the difference between the satellite data that they used and an alternative satellite vegetation cover data set (Tucker et al., 2001). Therefore given the many existing limitations, we cannot conclude which LAI estimate is more reliable (LAI obtained from MODIS satellite or calculated by ORCHIDEE). It is likely that the ORCHIDEE LAI could be improved and a possible component to be upgraded is the allocation of the different carbon stocks, but further investigations are needed. Performing a robust evaluation of the model's ability to simulate the LAI, especially at the global scale, still remains challenging, and is beyond the scope of our study.

In this context, model inter-comparison and sensitivity tests give an essential insight to assess the impact of different LAI estimates and their uncertainties on BVOC emissions."

§4 "*ORCHIDEE is designed to provide past, present and future scenarios of emissions from vegetation, studying the links between climate, the plant phenology and emissions. It is therefore essential that the internal variability, weaknesses and inaccuracies of the emission module are extensively investigated."*

Third, I would like to see a bit more model descriptions and information about setups. For ORCHIDEE, the activity factor is mentioned to depend on leaf age but it is not clear how it is derived and how it is different for different PFTs? It is not used in the comparisons of model simulations although it may pose a difference to MEGAN, particularly if it is decreasing the emission of PFTs with high leaf longevity. Furthermore, it is clear that drought and CO2 is changing in the simulations but it is not clear if one or both are considered for emission calculations. Regarding MEGAN, respective functions exist as options because emission is quite sensitive to both (e.g. Seco et al. 15, Acosta-Navarro et al. 14). With respect to the setup, I think that given the large differences in the PFT covered areas between the MEGAN and ORCHIDEE runs it would make sense running the models with each other's land-cover scheme to demonstrate the effect of this issue separately.

Author: At the end of section 2.4 we add more information about L_c and leaf age, telling which values are used, for which species and citing related references (see §5). Even if the absolute values are different, the L_c choice in the two models is quite similar, as both models consider higher emissions for new and young leaves for methanol and lower emissions for isoprene. Considering leaf class, things are more complex. Leaf age classes, in MEGAN, are derived considering the variation between LAI value of the current and preceding month, following an highly parameterised scheme. In ORCHIDEE leaf age classes are calculated considering the plant leaf growth and leaf turnover at each model time step (30 minutes) and are not directly correlated with LAI. The comparison between these two variables and the implementation of sensitivity tests to assess the impact of these different approaches are not straightforward. It would be a very interesting investigation and we mention it in section 5, page 39, as future development of this work (see §6).

Furthermore, we add some more information about the humidity/drought effect and about the CO_2 inhibition factor in influencing emission (see §5), as they are taken into account in the two models.

Considering the last point (running the models with each other's land-cover), the aim of the paper is to compare the two models, putting them under the same forcing conditions, but retaining their own particular characteristics (detailed now in new section 2.5 of paper): the emission scheme, parameterization setting, PFT distribution, radiative scheme. We stress this point in section 1, page 7 (see§7). In addition, PFT distribution is not interchangeable without significantly modifying the models since PFT classification are not the same in the two models. We clearly detail it in the new section 2.5, page 19, point 3 (see §8).

§5 *"In ORCHIDEE, the activity factor (L_c) is kept as in Lathière et al. (2006), considering four leaf age classes (new, young, mature and old leaves). For methanol, L_c is equal to 1 for new and young leaves and equal to 0.5 for mature and old leaves, while for isoprene, L_c is equal to 0.5 for new and old leaves and equal to1.5 for young and mature leaves. In MEGAN, the L_c values are taken from Table 4 in Guenther et al. (2012); in particular, for isoprene, L_c is equal to 0.05, 0.6, 1 and 0.9, and for methanol it is equal to 3.5, 3.0, 1.0, and 1.2 for the four leaf age classes. For both models, no soil moisture activity factor is taken into account. The annual CO_2 concentration varies along the simulation from a value of 368 ppm in 2000 to 385 ppm in 2009. In ORCHIDEE, the variation of CO_2 concentration can indirectly impact on the BVOC emission as it affects leaf growth, while in MEGAN, a CO_2 inhibition factor on isoprene emission based on Heald et al. (2009) is activated. As the CO_2 variation in this 10-year simulation is low, the inhibition effect is considered insignificant (Sinderalova et al. 2014) in this context."*

§6 *"Further analysis will certainly be needed in order to include other important parameters/variables in the investigation, for example: leaf temperature versus air temperature* *usage, leaf age classes, parameters in the Guenther formulation, the soil moisture activity factor."*

§7 *(iii) compare the ORCHIDEE results to the widely used emission model MEGAN, putting the two models under the same forcing conditions, but retaining their particular characteristics (see section 2.5), in particular the emission scheme, classes and distribution of PFTs and LAI processing,…"*

§8 *"…3) the PFTs classes and their distribution are not the same in the two models (Table 1) and they are not interchangeable without significantly modifying the models; "*

In addition, I would recommend avoiding repetitions throughout the manuscript (e.g. P33977 last paragraph, P33983 L18/19, P33996 last paragraph) and re-structure the analysis of LAI impacts, i.e. differentiating more clearly between the effect of size vs. dynamic and between emission area and light (and temperature) modifying impact (see also Keenan et al. 11). In this context, it is perhaps critical to state that some LAI are so large that there 'is no more light available' (P33992, L17). If this would be true, photosynthesis couldn't work and leaves wouldn't make any sense at all.

Author: We reformulate the text in section 2.2, page 11 (see §9), section 3.1, page 21 (see §10) and section 4, page 37 (see §11) to avoid repetition.

We re-structure the LAI analysis differentiating more clearly the discussion on LAI uncertainties, impact of LAI seasonal cycle and size on BVOC emission estimates (see the manuscript, section 3.4), adding two sub-sections: *"3.4.1 LAI seasonal cycle impact"* and *"3.4.2 LAI size impact"*.

About the statement *"there is no more direct light available"*, we omitted the essential word *"direct".* The phrase is indeed incorrect without it. In section 3.4.2, page 32, we change the text accordingly (see §12).

In addition, we mention the very interesting work of Keenan et al. (2011) in section 1 (see §13).

§9 *"As detailed in section 1, most recent field campaigns highlight, for a large number of plants, the dependency of monoterpenes, sesquiterpenes and oxygenated BVOC emissions on radiation as well."*

§10 *"(considering the speciated monoterpenes accounted in this work)"*

§11 *"The LAI calculated by ORCHIDEE is 1.5–2 m^2 m^{-2} higher than the LAI retrieved by MODIS. We examined what these discrepancies can impact on the BVOC estimates. Sensitivity tests are then performed forcing both models with the ORCHIDEE LAI multiplied by a factor of 0.5 and 1.5. ORCHIDEE and MEGAN emissions present a similar response to these LAI variations. Conversely, for monoterpenes, ORCHIDEE is much more sensitive to LAI variations, in comparison to MEGAN. These discrepancies are due to differences in the light-independent emission formulation between the two models. In ORCHIDEE the dependence of emissions on LAI is linear, while in MEGAN for LAI up to 2 m^2 m^{-2} is quasi-linear , then progressively reducing the increase up to become nearly constant for LAI greater than 5 m^2 m^{-2}."*

§12 *"Indeed isoprene is a light-dependent compound thus, beyond a given LAI threshold, the contribution of the highest LAI layers is very low , as there is no more or very little direct light available."*

§13 *"Keenan et al. (2011) investigate the effect of canopy structure using different canopy models and they conclude that larger differences in the final emissions can be attributed to the use of different canopy models, rather than different emission model approaches."*

References from Referee #2:

Ghirardo A, Koch K, Taipale R, Zimmer I, Schnitzler J-P, Rinne J. 2010. Determination of de novo and pool emissions of terpenes from four common boreal/alpine trees by 13CO2 labelling and PTR-MS analysis. Plant, Cell & Environment, 33: 781-792.

Keenan T, Grote R, Sabaté S. 2011. Overlooking the canopy: The importance of canopy structure in scaling isoprenoid emissions from leaf to canopy. Ecological Modelling, 222: 737-747.

Ruuskanen TM, Hakola H, Kajos MK, Hellen H, Tarvainen V, Rinne J. 2007. Volatile organic compound emissions from Siberian larch. Atmospheric Environment, 41: 5807- 5812.

Seco R, Karl T, Guenther A, Hosman KP, Pallardy SG, Gu L, Geron C, Harley P, Kim S. 2015. Ecosystem-scale VOC fluxes during an extreme drought in a broad-leaf temperate forest of the Missouri Ozarks (central USA). Global Change Biology, 21: 3657-

3674.

References in the answers:

Guenther, A., P. Zimmerman, and M. Wildermuth, Natural volatile organic compound emission rate estimates for U.S. woodland landscapes, Atmos. Environ., 28, 1197– 1210, 1994.