Responses to Referee #1's Comments

We thank Referee #1 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 #1'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.

Response to General Comments

(Initial paragraph or section evaluating the overall quality of the discussion paper)

1. This paper describes the updates in the BVOC emission module in the ORCHIDEE model. It further compares the predicted emissions by ORCHIDEE with the emissions predicted by the widely used emission model MEGAN. The paper also provides sensitivity test results to various parameters. The authors find that the spacial patterns of various BVOCs depend mostly on the allocated emission factor, while the seasonal patterns depend mostly on the leaf area index. The scope of the paper fits the journal well. The topic is timely, since there is a continuous need to intercompare models and a strong need to test their sensitivity towards input parameters, though there is also a strong need to evaluate models against measurements. These needs arise as there are great variations in individual plant species and that these species that have very different potential to emit VOCs (both in amounts and in the distribution of individual VOCs) are only covered by a few emission potentials in global models - as is also emphasised by the authors. The authors also highlight that there are very many factors (both physical, but especially biological) that affect the emissions, hence one has to be cautions when making conclusions based on global emission estimates. Unfortunately, the paper is quite messy and there are many mistakes – both in the grammar, but more importantly in the use of symbols. This naturally has to be corrected. My main concerning is how the emission potentials (EF) are allocated to the plant functional types (see also below). The handling of the EF is more detailed in the new emission module of ORCHIDEE (include higher degree of light dependency), but the justification of EF to different PFT has not improved and is also not properly justified. As the authors also specify, the impact of EF is dominant on the predicted emission and I therefore do not understand why the authors did not try to improve this part.

Authors: Referee #1 points out a crucial issue in BVOC modelling: the emission potential (EF) allocation. Following also the Referee #2's comment, we discuss more carefully the inherent difficulties in setting EFs at global scale. We add a new section (2.2.1) where we describe the general approach used to assign new EFs and its limitations.

In addition, accordingly Referee #1's comments, we re-organise some parts of manuscript, seeking a clearer exposition, and we correct the use of notation in equations (1), (2) and (3) (for the details see points 51). The paper was read by two English native speakers to reduce, as much as possible, the grammar mistakes.

*"**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).”

Response to Specific comments:

(Section addressing individual scientific questions/issues)

2. *) P33970L6-9: you could potentially supplement with some references.

Authors: We add at first paragraph of section 1 the references as follows.

“(Laothawornkitkul et al, 2009; Guenther et al., 2012; Penũelas and Staudt 2014)”

3. *) P33970L14: maybe you could also mention some of the papers that first showed the contribution of BVOCs to SOA formation and growth.

Authors: We change the text, in section 1, page 3.

"Additionally, BVOCs and their oxidation products lead to the formation and growth of more than 50% of the Secondary Organic Aerosols (SOA) (Kanakidou et al., 2005; Goldstein and Galbally, 2007; Van Donkelaar et al., 2007; Engelhart et al., 2008; Hallquist et al., 2009; Acosta Navarro, et al., 2014; Tsigaridis et al., 2014)."

4. *) P33970L9-18: Maybe you could also mention the contribution to CCN and clouds and hence climate.

Authors: We add in section 1, page 3:

"Under appropriate atmospheric conditions, BVOCs can contribute to a significant fraction of particles that evolve into cloud condensation nuclei (CCN) (Riipinen et al., 2012), even enhancing the droplet number concentration in clouds (Topping et al., 2013)."

5. *) P33971L15-18: Maybe you could add some references?

Authors: We add in section 1, page 4, two references in which EF is defined.

"The basal EF for instance, defined as the emission at the leaf level under standardized environmental conditions of temperature and solar radiation (Guenther et al., 1995; Steinbrecher et al., 2009),…"

6. *) P33972L1-6: Also mention that phenology is not included in these models (e.g. MEGAN and ORCHIDEE), which is a big lack in order to describe the emission of VOCs.

Authors: Actually, ORCHIDEE includes an explicit description and calculation of plant phenology, as briefly described in section 2.4. MEGAN, on the other hand, does not explicitly calculate it. What is not taken into account in both models is the variation of EF in relation with plant phenology.

We add it in section 1, page 5.

"In addition, the link between EF variation and plant phenology is in general not taken into account, or is roughly described, especially in models that adopt the empirical approach."

7. *) P33972L16-17: I agree, but maybe it is also worth mentioning that the temperature and light dependency not only depends on the VOC of interest, but also the plant species considered (e.g. Ghirardo et al., 2010) due to the different production paths.

Authors: In section 1, page 5, we detail more this important point, also following the Editor remark:

"The Guenther et al. (2012) approach considers only one value per emitted compound, whilst it has been shown the LDF also depends on the plant species. For example, measurements of the diurnal cycle for monoterpenes above Amazonian rainforest (Rinne et al. 2002; Kuhn et al., 2002) suggest that emissions are dependent on both light and temperature, whilst the role of light in influencing monoterpene emissions from boreal Scot pine forest is less clear (Taipale et al., 2011). Moreover, Staudt and Seufert (1995) and Loreto et al. (1996) show that monoterpene emissions from coniferous trees are principally influenced by the temperature, while those from Holm oak are predominantly controlled by a light-dependent mechanism. Owen et al. (2002) find that, in the Mediterranean region, emissions of all compounds from Quercus sp. are light dependent, the ocimene emitted by Pinus pinea is strongly correlated to light and an apparent weak light dependency is exhibited by monoterpene emissions from Cistus incanus. Ghirardo et al. (2010) provide the fraction of light-dependent monoterpene emission, being 58% for Scots pine, 33.5% for Norway spruce, 9.8% for European larch, and 98–100% for both Silver birch and Holm oak. Shao et al. (2001) and Steinbrecher et al. (1999) attribute for Scots pine a value of 20–30% and 25–37%, respectively. Nevertheless, there is no general agreement on the exact value of the temperature- and light-dependent fraction to assign for individual compound and PFT, as it appears also from the works mentioned right above."

8. *) You discuss the sensitivity of the models with respect to LAI and also mention the discrepancy between modelled and measured LAI, but there is no word on biomass. I did a quick test with MEGAN and the emission scales proportionally with the biomass and non-linearly with LAI, and the emission is much more sensitive to the biomass than to the LAI. The effect of biomass has to be discussed in the paper. It would also improve the manuscript if there was at least some discussion on other canopy characteristics (canopy height, depth, age, ...).

Authors: The current version of the MEGAN model, which is the one used in this study, does not use the biomass density as an input parameter. It defines emission potentials (EPs) of the model grid cell (in case of using the emission potential maps) or EFs of each PFT (when using the look up table) as emission per surface (μg m^{-2} h^{-1}). Therefore the amount of emitting biomass is, in fact, accounted in the value of LAI. Even in the ORCHIDEE emission module, the input parameter is LAI, that is obtained by multiplying the leaf biomass per PFT grid cell area (gC m^{-2}) to the PFT specific leaf occupying area (SLA) (m^2 gC^{-1}). The biomass is calculated in the phenology module, while SLA is a parameter vector.

About the other canopy characteristics in MEGAN (i.e canopy height, canopy depth, leaf width, leaf length), they are principally used to determine the leaf temperature. As the leaf temperature is not calculated in ORCHIDEE, there is no correspondence about these parameters between the two models.

In MEGAN leaf age classes, are derived considering the variation between LAI value of the current and preceding month, following a 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 as future development of this work in the new section 5 (see below).

It is worth to mention these differences between both models. We add them in the new section 2.5 related to the differences between ORCHIDEE and MEGAN emission algorithms (see point 10.).

"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."

9. *) P33973L23-29: I do not agree with the authors at all! You cannot asses the correctness of a model by inter-comparing it with another model. This will only tell you how the models differ. As the authors mention earlier in the intro, more and more field measurements are done and it must be those that the models have to be evaluated against in order to evaluate their uncertainty. Unfortunately this has not been done enough, but there is a strong need to do so! A few examples are Tsigaridis et al., 2014, Mann et al., 2014 and Spracklen & Righelato 2014.

Author: The sentence indeed was not properly formulated. We change it in section 1, page 7 as follows:

"The proper way to assess the correctness of a model is to evaluate it against observations, as is done, for example, for organic aerosol by Mann et al. 2014) and Tsigaridis et al. (2014) and for tropical mountain forest carbon store by Spracklen and Righelato (2014). The evaluation of BVOC emission models against observations has already been carried out at a local and regional scale (i.e. Karl et al., 2007; Kunl et al., 2007; Lathière et al., 2009; Smolander et al., 2014),

demonstrating a good performance of the Guenther formulation. Nevertheless, given the ecosystem biodiversity, the huge variability of the parameters involved and the poor spatial and temporal coverage of BVOC emission observations, it is extremely difficult to infer any evaluation at global scale from these tests. In such a context we can rely on model inter-comparison and sensitivity tests in order to assess the limitations and uncertainties of BVOC emission estimates, to relate them to particular key parameters/variables and to investigate their origin."

10. *) It would improve the paper significantly if you included a section that clearly describes what is the difference between your emission module and the MEGAN module. To me it seems that the largest difference is the land cover, which is anyway not predicted by your emission module, but LPJ (or LUH – not clear which). Otherwise it seems to me that you just changed the light dependency and emission factors and this you might as well just have been done in MEGAN.

Authors: We add a new section 2.5, where the differences between the two models and the emission schemes are better explained (see §1). We also change the paragraph where the γ_{LAI} is described (see §2) in section 3.4.2.

We have also noticed that the explanation about the land cover database is ambiguous. Since simulations were performed only for ten years, we do not use a dynamical calculation of vegetation land cover but, rather, land cover fixed data provided by Hurtt et al. (2006). There is, thus, no need to mention this ORCHIDEE module. Therefore we have removed the last sentence about the LPJ model at the end of section 2.1. We also add in section 2.4, page 18, the reference path for the land cover files used (see §3).

§1 *"2.5 Differences between ORCHIDEE and MEGAN emission algorithms*

While starting from a similar approach the ORCHIDEE and MEGAN emission modules differ significantly in their parameterization and variable description. We list below the main differences:

1) in ORCHIDEE, the formulation of CTLD and CL is the same as in Guenther et al. (1995) (see equation 9 and 10), while in MEGAN it is defined by equations (8), (9), and (10) in Guenther et al. (2012). In particular in Guenther et al. (2012) the parameters of the CTLD formulation vary according to the average solar radiation over the past 24h and 240h, and this dependence is different for diffuse and direct radiation. We calculate the CTLD obtained with this formulation considering different incoming solar radiations and we observe that the CTLD for direct light is *around twice that for diffuse light. In ORCHIDEE the CTLD parameters are fixed and are the same for diffuse and direct radiation;*

2) the radiation scheme in ORCHIDEE and MEGAN is based on the same approach (Spitter et al., 1986 a,b), but the parameterization and formulation used are different. For example, the number of vertical layers and their distribution over the LAI significantly differ between the two models: up to 17 in ORCHIDEE and up to 5 in MEGAN. MEGAN also takes into account the infrared radiation in emission calculation;

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;

4) LAI is considered in a different way in the two models. ORCHIDEE calculates the LAI at each model time step for each PFT and grid cell, taking into account a full plant phenology scheme. MEGAN, on the other hand, does not compute the LAI, rather, it has to be provided as an input averaged over the vegetated part of the grid cell;

5) in ORCHIDEE, emissions are calculated for each PFT using the associated EF and LAI. Next, they are averaged over the grid cell, considering the PFT land cover surface, as described in Sect. 2.2. In MEGAN, vegetated emission potential is calculated over the grid cell and multiplied by the average LAI over the vegetated part of the grid cell. In MEGAN, vegetated potential emission maps are provided for isoprene, α-pinene, β-pinene, 3-Carene, limonene, myrcene, t-β-ocimene and sabinene, while for the other compounds EPs are calculated starting from the EFs per PFT and the PFT land cover distribution. This is a significantly different approach. However, for ORCHIDEE, we find that global emissions calculated using the EP and LAI per grid cell (the MEGAN approach) are only 5-12% lower in comparison with the emissions calculated in the standard way. Isoprene presents the lowest differences and monoterpenes the highest;

6) in the ORCHIDEE model, the dependence on LAI of the light independent emission is linear, as shown in the Eq. (1) and (2) of the present work. Whereas in MEGAN, the dependence on LAI is given by the γ_{LAI} *factor that is equal to* $(0.49 \cdot LAI)/(1+0.2 \cdot LAI^2)^{0.5}$ *(Guenther et al., 2006). The implications of this are detailed in section 3.4.2;*

7) in MEGAN, leaf age classes are derived from consideration of the variation between the LAI value of the current and preceding month, following a highly parameterised scheme. In ORCHIDEE, leaf age classes are calculated on-line considering the plant leaf growth and leaf turnover at each model time step (30 minutes);

8) in ORCHIDEE, hydrological processes are explicitly calculated, as briefly described in section 2.1;

9) In ORCHIDEE, the air temperature is used to compute emission, while in MEGAN the leaf temperature is considered."

§2 *"Such differences are detailed in point 6 of section 2.5. In particular, in ORCHIDEE, the light independent emission linearly depends on LAI whereas, in MEGAN it is determined by the γ_{LAI} factor and it varies almost linearly for low LAI ($< 2\ m^2\ m^{-2}$) and then more and more slowly up to become almost constant for LAI higher than $5\ m^2\ m^{-2}$."*

§3 *"The database can be found in:*

http://dods.extra.cea.fr/work/p86ipsl/IGCM/BC/SFR/OL2/PFTmap_1850to2005_AR5_LUHa.rc2."

11. *) P33976L1-2: I guess that this is also the case in MEGAN?

Authors: In MEGAN there are two options:

(1) EFs are set per PFT as in ORCHIDEE using look up table (for the values see Table 2 in Guenther et al., 2012);

(2) high resolution emission potential (EP) maps are used, which do not depend on PTFs. These maps are compiled considering information from different plant species flux measurements and land cover plant species distribution. EPs are provided for isoprene, α-pinene, β-pinene, 3-Carene, limonene, myrcene, t-β-ocimene and sabinene.

It is now more explicit in the text in the new section 2.5 (see §1, point 5).

12. *) P33976L17-19: I guess the real argument is also that these are the compounds that have been measured to be emitted from vegetation in the greatest abundance?

Authors: We definitely agree with the Referee #1. We add the comment in section 2.2, page 10.

"We chose these compounds because measurements have shown that they are emitted from vegetation in the greatest abundance and because of their importance in atmospheric chemistry, in particular regarding secondary organic aerosol formation."

13. *) P33976L25-26: Have you somehow taken the landcover of various species within a PFT into consideration when doing the averaging? If not, I am sceptical.

Authors: We have taken into account the possibility of making an average weighted on land cover of various species within a PFT. But we remarked that the values of EF collected are often related to a small number of species and often come from the same measurement sites. This is one of the major limitation in the allocation of EFs, which cannot be reduced by the weighted average. In theory, if we knew the EF of all plant species belonging to the same PFT and their land cover, the weighted average method would be statistically robust and absolutely preferred. Unfortunately, all this information is not currently available. We agree that it is a very coarse procedure. The low accuracy of the EF parameter used in the model, as well as other parameters/variables, is the principal reason which prompted us to perform a sensitivity study.

We detail the EF limitation in the new section 2.2.1 (see also point 1).

14. *) P33977L3-6: Please also mention the large change in boreal broadleaved deciduous trees, which I somehow doubt.

Authors: We mention in the text (section 2.2.1, page 15) the new EF value, related to boreal broadleaved deciduous trees, and the papers in which particularly high values for this PFT are provided.

"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})."

15. *) P33977L13-15: But one could look into e.g. forest inventories or similar in order to get a better idea.

Authors: We agree with Referee #1, considering anyway the limitations which may arise and that we discuss in the point 13. Following Referee #2's remark too, we detailed in the new section 2.2.1 the limitation in assigning an individual EFs per PFT (see point 1).

16. *) P33977L25-29: Please also mention that the light dependency of a compound also depends on which plant it is emitted from (e.g. Ghirardo et al., 2010).

The Referee #1 raises an important point. Also accordingly with the Editor's comment, we detail in section 1 (page 5) the discussion about the light dependency (see §4) and we mention it in section 2.2 , page 11 (see §5), following the 2 comment, too. The text is reformulated in the following way:

§3 *"The Guenther et al. (2012) approach considers only one value per emitted compound, whilst it has been shown the LDF also depends on the plant species. For example, measurements of the diurnal cycle for monoterpenes above Amazonian rainforest (Rinne et al. 2002; Kuhn et al.,* *2002) suggest that emissions are dependent on both light and temperature, whilst the role of light in influencing monoterpene emissions from boreal Scot pine forest is less clear (Taipale et al., 2011). Moreover, Staudt and Seufert (1995) and Loreto et al. (1996) show that monoterpene emissions from coniferous trees are principally influenced by the temperature, while those from Holm oak are predominantly controlled by a light-dependent mechanism. Owen et al. (2002) find that, in the Mediterranean region, emissions of all compounds from Quercus sp. are light dependent, the ocimene emitted by Pinus pinea is strongly correlated to light and an apparent weak light dependency is exhibited by monoterpene emissions from Cistus incanus. Ghirardo et al. (2010) provide the fraction of light-dependent monoterpene emission, being 58% for Scots pine, 33.5% for Norway spruce and 9.8% for European larch, and 98–100% for both Silver birch and Holm oak. Shao et al. (2001) and Steinbrecher et al. (1999) attribute for Scots pine a value of 20–30% and 25–37%, respectively. Nevertheless, there is no general agreement on the exact value of the temperature- and light-dependent fraction to assign for individual compound and PFT, as it appears also from the works mentioned right above.”*

§4 *“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. To adopt a detailed parameterisation is not yet possible, cause to data lacking at global scale. Therefore, in the new emission module we consider the approach described in Guenther et al. (2012), even if it is rather oversimplified.”*

17. *) P33978L3: I do not agree that light dependency only means “directly released through stomata” and that temperature dependency only means “stored in the leaf pool”. It is much more complicated than that and refers in great part to the production of the compounds. So please reformulate or leave out.

Authors: we removed it from the text.

18. *) P33982L1-5: It would be very very interesting to see what emissions ORCHIDEE and MEGAN would produce if the approach of the other model was used.

Authors: Referee #1 raises a very interesting point. However, doing this in MEGAN would require considerable changes in the code and this is out of the scope of the current paper. In fact, main objective is to characterise how the particular characteristics of the two models impact on the BVOC emission estimates, by putting them under the same forcing conditions (see §5). We specify it in section 1 (page 7).

However, we have performed this test in ORCHIDEE. We found that the annual global emissions calculated with the usual method are 5-12% higher than those calculated adopting the MEGAN approach (using EP and LAI averaged over the grid cell). Isoprene presents the lowest differences and monoterpenes the highest. This test gives indeed an interesting insight and we mention it in section 2.5, point 5 (see §6).

On a general ground, we are interested in performing sensitivity tests on variables/parameters which are likely to be objects of future developments of ORCHIDEE. For example: EFs, LDFs or LAI. In our opinion, this is less likely to be the case for the use of emission potential maps.

§5 *"...(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, ..."*

§6 *"This is a significantly different approach. However, for ORCHIDEE, we find that global emissions calculated using the EP and LAI per grid cell (the MEGAN approach) are only 5-12% lower in comparison with the emissions calculated in the standard way. Isoprene presents the lowest differences and monoterpenes the highest."*

19. *) P33982L11-12: Guess the point is that some areas (e.g. Europe and the US) are covered quite well (though there is definitely a lack of year-round measurements), while there exists no or close to no data in order regions.

Authors: We agree with Referee #1 and we change the text at the beginning of section 3.1 as follows, considering also the point 9:

"As already discussed at the end of the introduction, the validation of BVOC emissions at the global scale is a complex issue because of the poor data coverage in many regions and the general lack of year-round measurements."

20. *) P33983L22-24: Reading this and looking at Fig. 1, it seems to me that what should really be tested/improved is the met forcings, since that seems to have much greater impact than the emission module.

Authors: We are aware that the emission variations resulting from differences in meteorological forcing can be significant and we are also aware that this is a very important issue. However, in the manuscript we focus more on the internal source of variability, on the weakness of the emission module and how to improve it. In fact, the ORCHIDEE model is also designed to be used in future emission scenarios studies. We stress this point at the end of section 1, page 7 (see below).

Arneth et al. (2011) have already provided very interesting and quite exhaustive study on this issue. We, therefore, only mention some results confirming their conclusions and we do not investigate this topic further (section 3.1, page 22).

Regarding the meteorological forcing data, we are not directly involved in the meteorological forcing production. We have used for this study the state of the art forcing files available for the model.

We change the text in section 1 (page 7) as follows:

"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."

21. *) P33989L3-13: Wouldn't it be better to move this to Sec. 3.5? After reading this short paragraph, I am wondering why the emission response is different, since you use the same emission algorithm (Guenther/MEGAN).

Authors: We moved the paragraph to section 3.5.

The two emission algorithms are similar, but there are some differences that can be crucial. In the new section 2.5, we point out the principal discrepancies. In particular, the discrepancy mentioned in the paragraph is extensively explained in section 3.5.

22. *) Table 2: Where does the LDF and Beta values come from? Why is LDF and Beta 0.6 for total monoterpenes, but no values are assigned for the individual monoterpenes? Or is the LDF and Beta values also 0.6 for all the individual monoterpenes? This is not clear. There is information about the light dependency of the individual monoterpenes, which seems to be quite large – e.g. sabinene and ocimene seems to be very light dependent (e.g. Owen et al., 2002). Please indicate what "MBO" is. Compound names should not be in capital.

Authors: LDF and beta are indeed not exhaustively described in the text. We add more details in section 2.2, page 12 (see §5 and §6).

This is the first time that speciated monoterpenes are inserted in the code and, as a first step, we decided not to further detail the modelling of individual monoterpenes and choose a single

LDF/Beta value (LDF=0.6 and Beta=0.1) for all monoterpenes. We are aware that it is a rather crude approximation, but there are currently not enough observations to assign a sufficiently solid parameterization at global scale. We have chosen the Beta value referring to Guenther et al. (2012) and the LDF value relying on Dindorf et al., 2006; Holzke et al., 2006; Guenther et al., 2012; Šimpraga et al., 2013.

MBO is the 2−methyl−3−buten−2−ol and it is often referred as MBO (e.g. Hakola et al., 2006; Baker et al., 1999; Schade and Goldstein, 2001; Tarvainen et al., 2005; Guenther et al., 2012; Hakola et al., 2006; Chang et al., 2009; Kim et al., 2010). We mention more clearly in section 2.2.1 (page 15) that we refer to 2−methyl−3−buten−2−ol as MBO (see §7). Considering the other compounds, we put the name in minuscule, checking throughout the text.

Following the Editor remark too, we add a paragraph in section 1, where the LDF issue is discussed (see §8).

§5: *"To chose the LDF value for monoterpenes, we rely on Dindorf et al. (2006), Holzke et al. (2006), Guenther et al. (2012) and Šimpraga et al. (2013). Other LDF values were based on Guenther et al. (2012)."*

§6: *"β is the empirical coefficient of the exponential temperature response and it is now defined as in Guenther et. al (2012)"*

§7: *"…2-methyl-3-buten-2-ol (hereafter we refer to it simply as MBO)..."*

§8: *"The Guenther et al. (2012) approach considers only one value per emitted compound, whilst it has been shown the LDF also depends on the plant species. For example, measurements of the diurnal cycle for monoterpenes above Amazonian rainforest (Rinne et al. 2002; Kuhn et al., 2002) suggest that emissions are dependent on both light and temperature, whilst the role of light in influencing monoterpene emissions from boreal Scot pine forest is less clear (Taipale et al., 2011). Moreover, Staudt and Seufert (1995) and Loreto et al. (1996) show that monoterpene emissions from coniferous trees are principally influenced by the temperature, while those from Holm oak are predominantly controlled by a light-dependent mechanism. Owen et al. (2002) find that, in the Mediterranean region, emissions of all compounds from Quercus sp. are light dependent, the ocimene emitted by Pinus pinea is strongly correlated to light and an apparent weak light dependency is exhibited by monoterpene emissions from Cistus incanus. Ghirardo et al. (2010) provide the fraction of light-dependent monoterpene emission, being 58% for Scots pine, 33.5% for Norway spruce, 9.8% for European larch, and 98–100% for both Silver birch and Holm oak. Shao et al. (2001) and Steinbrecher et al. (1999) attribute for Scots pine a value* *of 20–30% and 25–37%, respectively. Nevertheless, there is no general agreement on the exact value of the temperature- and light-dependent fraction to assign for individual compound and PFT, as it appears also from the works mentioned right above."*

23. *) Table 3: I understand that you have to limit, but there are much more papers available.

Authors: We have collected many more papers than those included in table 3, but we have rejected many of them as they do not report EFs at standard conditions of PAR and temperature or there are not enough details for reporting EFs at the standard conditions. In addiction we realize that they are often related to the same number of species and come from the same measurement sites. For further details see point 1 and section 2.2.1 in the manuscript.

24. *) Table 4: where do these ratios come from?

Authors: This ratio comes from the paper review carried out for EFs. The only difference is that we represent it as percentage of speciated monoterpene EFs with respect to the PFT bulk monoterpene EF. The references used are those listed for monoterpenes in Table 3. We are now more precise and we add the references per individual monoterpene in Table 4.

25. *) Table 6: Why are there no estimates from MEGAN concerning limonene, myrcene, 3-carene and ocimene?

Authors: Accordingly to Referee #1's remark we add in Table 6 and 7 the limonene, myrcene, 3-carene and ocimene emission estimates.

26. *) I am very sceptical that you predict so high isoprene emissions (especially compared to the monoterpene emissions) in northern temperate and boreal areas. This also seem to be one of your largest differences to the MEGAN model. I fear that this high isoprene emission is due to the fact that you have not considered which northern plants emit isoprene and which do not.

Authors: The largest differences in emissions are actually related to the different choice of the EF values, to the different PFT land cover and to the different PFT categorisation between the two models.

To show this, we compare ORCHIDEE EFs with MEGAN ones (provided in Table 2 by Guenther et al. 2012), after converting them in the ORCHIDEE units. They are not exactly the EFs used to perform simulation in MEGAN, as in the case of isoprene the emission potential map is directly provided. However, by calculating the emission potential using these EFs (Guenther et al. 2012) we get EPs comparable to those of the provided map (differences around 10%).

In the boreal zone the percentage of main PFTs calculated with respect to the surface occupied by vegetation in boreal region and the related EFs are:

In ORCHIDEE

- C3Gr (51.5%) with an EF = 12 μgC g^{-1} h^{-1}
- BoBrDe (21.3%) with an EF = 18 μgC g^{-1} h^{-1}
- BoNeEv (16.5%) with an EF = 8 μgC g^{-1} h^{-1}
- BoNeDe (7%) with an EF = 0.5 μgC g^{-1} h^{-1}

In MEGAN

- BoSbDe (48%) with EF = 9.9 μgC g^{-1} h^{-1},
- BoNeEv (24%) with EF = 6.2 μgC g^{-1} h^{-1}
- C3Cold (16.5%) with EF = 4 μgC g^{-1} h^{-1}
- BoBrDe (2%) with EF = 22.7 μgC g^{-1} h^{-1}

We see that the main differences come from the different choice of EF value for grass and the different PFT classes land cover. For MEGAN in boreal areas there is a strong presence of BoSbDe which is not represented in ORCHIDEE, where it is replaced by BoBrDe and C3Gr.

Considering the EF assigned to C3Gr, we lowered its value with respect to the previous version, from 16 to 12 μgC g^{-1} h^{-1}. These is a compromise value, chosen so that we do not excessively bias the emissions in other areas. C3Gr is, indeed, strongly present in other regions: 13% of northern tropical areas, 22% of southern tropical areas and 32% of the total vegetation surface.

Considering the BoBrDe EF, we set new value relying on Levis et al. (2003), Arneth et al. (2011), Guenther et al. (2006) and Guenther et al. (2012) but the choice is critical as there is no shrub in ORCHIDEE.

In the case of the temperate zones we identify similar problems. The percentage of main PFTs and the related EFs are:

In ORCHIDEE

- TeNeEv (9.4%) with EF = 8 μgC g^{-1} h^{-1}
- TeBrDe (12%) with EF = 45 μgC g^{-1} h^{-1}
- C3Gr (42%) EF = 12 μgC g^{-1} h^{-1}

- C3Ag (18%) with EF = 5 μgC g^{-1} h^{-1}

- BoNeEv (7.8%) EF = 8 μgC g^{-1} h^{-1}

In MEGAN :

- TeNeEv (8.4%)
- BoNeEv (20%) EF = 6.2 μgC g^{-1} h^{-1}
- TeBrDe (7.6%) with EF = 31 μgC g^{-1} h^{-1}
- C3GrCold (6%) with EF = 4 μgC g^{-1} h^{-1}
- C3GrCool (20%) with EF = 2 μgC g^{-1} h^{-1}
- Crop (23.2%) with EF = 0.12 μgC g^{-1} h^{-1}

In this case the discrepancies, between the two models, are mainly linked to the different values of the EFs and the different PFT surface coverage for grass and crop.

In the north temperate zones, there is an additional reason. The global average of bare soil in ORCHIDEE is half compared to MEGAN, while the north temperate average of bare is a third. This can contribute to higher emissions of ORCHIDEE than MEGAN.

From this comparison the limitations in the use of the PFTs come up. The most critical PFTs are the grass and the crop and in boreal region the BoBrDe and BoSbDe. Further studies would be useful to understand if the chosen values should be modified. It appears rather clear that a parameterisation with a larger number of PFTs could lead to more accurate results. We raise this point in the conclusions.

Lowering the ORCHIDEE EF values for the concerned PFTs we could reduce the differences between ORCHIDEE and MEGAN emissions. Nevertheless this is out of the scope of the article, which is focused on quantifying the variability of BVOC related to certain parameters. Moreover similarities of the outcomes of the two models may be accidental. For example, in the paper we show that ORCHIDEE is very sensitive to variations of LAI and with can make its emissions much more similar to those of MEGAN by halving the LAI (ORC_LAI05 simulation, Fig. 12, right column). Thus, if MODIS LAI would eventually turn out to be the correct one, we would have to reduce the LAI ORCHIDEE by a factor 2 (Fig. 10), and we would get similar emissions to those of simulation ORC_LAI05. However, at present, given the high uncertainties on LAI MODIS, we are not allowed to conclude that ORCHIDEE LAI is incorrect. In conclusion, we cannot say, at the moment, whether it is better to correct LAI or the EF or both.

We add in section 3.1 a part of this important issue.

"In particular, in northern temperate region the highest discrepancies are mainly due to the different PFT surface coverage for grass and crop and the higher EFs values in ORCHIDDE in comparison to MEGAN. Actually, in ORCHIDEE C3Gr covers the 42% of vegetated surface with an EF = 12 μgC g^{-1} h^{-1}, C3Ag covers the 18% with an EF = 5 μgC g^{-1} h^{-1}, while in MEGAN the C3GrCool occupies the 20% with an EF = 2 μgC g^{-1} h^{-1}, C3GrCold the 6% with an EF = 4 μgC g^{-1} h^{-1}, C3GrCool the 20% with an EF = 2 μgC g^{-1} h^{-1} and Crop the 23.2% with an EF = 0.12 μgC g^{-1} h^{-1}. This example raises an important issue. Considering the EF assigned to C3Gr, we lowered its value with respect to the previous version, from 16 to 12 μgC g^{-1} h^{-1}. These is a compromise value, chosen so that we do not excessively bias the emissions in other areas. C3Gr is, indeed, strongly present in other regions: 13% of northern tropical areas, 22% of southern tropical areas and 32% of the total vegetation surface. A more detailed description of the different crop and grass (in other words with a larger number of PFTs) could lead to more accurate results. The same consideration could be done for almost all the other PFTs."

27. *) You predict higher sesquiterpene emissions in the tropics than MEGAN – why is this so? Just because the EF is increased in your simulations?

Authors: The largest differences in emissions are actually related to the differences of PFTs distribution and EFs between the two models, in particular the differences that rise from crop and grass. The higher EFs related to these PFTs contribute to higher sesquiterpenes emissions in ORCHIDEE in comparison to MEGAN.

In the Tropics the percentage of main PFTs calculated with respect to the surface occupied by vegetation in that region and the related EFs are:

In ORCHIDEE:

- C3Gr (17.6%) EF = 0.6 μgC g^{-1} h^{-1}
- C4Gr (27%) EF = 0.6 μgC g^{-1} h^{-1}
- C4Ag (8%) EF = 0.08 μgC g^{-1} h^{-1}
- TrBrEv (24%) EF = 0.45 μgC g^{-1} h^{-1}
- TrBrDe (13%) EF = 0.45 μgC g^{-1} h^{-1}

In MEGAN

- C3GrWarm (22.4%) EF = 0.01 μgC g^{-1} h^{-1}
- C3GrCool (11%) EF = 0.01 μgC g^{-1} h^{-1}
- Crop 14% (14%) EF = 0.002 μgC g^{-1} h^{-1}

- TrBrEv (24%) EF = 0.46 μgC g^{-1} h^{-1}

- TrBrDe (13%) EF = 0.46 μgC g^{-1} h^{-1}

The EFs, set in ORCHIDEE, are based on Matsunaga et al. (2009), Steinbrecher et al. (2009), Ortega et al. (2008), Duhl et al. (2008), where the values are considerably higher than in MEGAN.

We do not add the above examples to the manuscript since we consider that those provided in the answer to point 26 already give a sufficient insight in the matter.

28. *) It would be good if you added a section in the end that would also discuss the impact of your findings? And maybe hold this together with previous studies on e.g. meteorology.

Authors: We add a new section in the end of manuscript, discussing the possible developments and the impact of our findings.

"Model inter-comparison and sensitivity tests are extremely useful to define which parameters/variables mainly affect BVOC emissions, which is the cause of this sensitivity, and how estimates can be improved. Previous works have already investigated the impact of different experimental set-ups (climate forcing and vegetation distribution) (Arneth et al., 2011), differences in the canopy structure description (Keenan et al., 2011) and land cover classification (Oderbolz et al., 2013) on emissions.

In the present work we focused on the impact of LAI, LDF, EFs and PFT distribution. Our results underline that the high uncertainties in the involved variables/parameters, and the different choices in modelling processes, result in high variability of BVOC emission estimates. The outcome of this analysis provides some guidelines for future developments of BVOC emission models at the global scale. In particular the following issues should be carefully addressed:

- *LAI uncertainties are still extremely high and have a considerable impact on emissions. Improvements in LAI modelisation or estimation at the global scale are essential;*
- *EF allocation is a big concern because of its high variability. A proper way to assign statistically robust values at a global scale has not yet been found. Significant improvement can be achieved only by increasing the observation data coverage of many regions and performing long-term measurements;*
- *model LDF parameterisation is still oversimplified and has a significant impact on emissions. Future developments should, therefore, improve LDF parameterization accuracy. For example, by including PFT dependency. As for EFs, results can be achieved only by increasing observation coverage;*

- *the rather low number of PFTs is a limiting factor in an accurate emission estimates;*

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.

Finally, it is worth mentioning that, besides model inter-comparison, there is a strong need to evaluate model results against emission observations. This has already been done in other domains, for example in atmospheric chemistry modelling (Mann et al., 2014; Tsigaridis et al., 2014). In the case of BVOC, however, observational data are very challenging to acquire, especially on the long-term scale. Therefore, for BVOC emission modelling, a robust validation of model results against observations, is still lacking."

Response to Technical Corrections:

(Compact listing of purely technical corrections)

29. *) There are many places where the language could be improved (not by fancy words, but just correct English – e.g. the article is sometimes missing.). I have indicated some mistakes, but there are more.

Authors: We thank the Referee #1 for checking the English of manuscript. We examine the text, also following the Referee #2's comment, 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 text has been read by two English native speakers.

30. *) You use the unit "gdm" - it is not clear to me what this means.

Authors: It means gram of dry matter. Actually "gdm" is misleading and can be better replaced by "g". We change in "gdm" in "g" everywhere.

31. *) Please provide the full institutional addresses in the affiliations.

Authors: We change the text in:

"[1]{Laboratoire des Sciences du Climat et de l'Environnement, LSCE-IPSL, CEA/CNRS/OVSQ, Université Paris-Saclay, CEA-Orme des Merisiers, 91191 Gif-sur-Yvette, France}

[2]{Laboratoire Atmosphères, Milieux, Observations Spatiales, LATMOS-IPSL, UPMC/CNRS/OVSQ, UPMC 4 Place Jussieu, 75252, Paris, France}

[3]{Department of Atmospheric Physics, Faculty of Mathematics and Physics, Charles University in Prague, Ke Karlovu 3, 121 16 Prague, Czech Republic}

[4]{Cooperative Institute for Research in Environmental Sciences, University of Colorado, 216 UCB, Boulder, Colorado 80309, USA}

[5]{National Oceanic and Atmospheric Administration (NOAA) Earth System Research Laboratory, Chemical Sciences Division, 325 Broadway R/CSD, Boulder, Colorado, 80305-3337, USA}

[6]{Max Planck Institute for Meteorology, Bundesstraße 53, 20146, Hamburg, Germany}

[7]{Institut Pierre Simon Laplace des sciences de l'environnement, UPMC 4 Place Jussieu, 75252, Paris, France}"

32. *) Please spell out "BVOC" first time this acronym is used (first line in abstract) and not in the intro. Same comment for "PFT" (P33969L21), which is currently first spelled out in the intro.

Authors: Text corrected.

33. *) P33969L27: It took me some time before I realised that you scaled LAI by 0.5 and 1.5. Please write this more clearly.

Authors: We reformulate the text in the Abstract:

"LAI is scaled by a factor of 0.5 and 1.5, changing the isoprene global emission by –21% and +8% for ORCHIDEE and –15% and +7% for MEGAN, and affecting the global emissions of monoterpenes by –43% and +40% for ORCHIDEE and –11% and +3% for MEGAN."

34. *) P33970L2: "to variation of LDF" → "to variation in the LDF".

Authors: Text corrected.

35. *) P33970L26: "*largely*" → "*widely*".

Authors: Text corrected.

36. *) At many places, you mix UK and US English. Please homogenise.

Authors: The text checked and homogenise.

37. *) P33971L11: delete "a" and replace "variation" by "variations".

Authors: Text corrected.

38. *) P33971L15: Don't spell out "EF" again, you have already done so. Same goes for "LAI" on P33972L19. In general: please check the whole manuscript for places where you have spelled out acronyms more than once.

Authors: Text checked and corrected.

39. *) P33971L17: delete "is a key emission driver", since that does not make sense – it IS the actual emission at standard conditions. Also delete "a" in front of "large variability".

Authors: Text corrected.

40. *) P33971L28: "as one PFT can actually correspond to" → "as one PFT is actually corresponding to", since there are always more than one species covered in a PFT.

Authors: Text corrected.

41. *) P33971L27-P33972L1: this sentence sounds broken or that something is missing.

Authors: Text changed in section 1 as follows:

"The choice of one single value for each PFT is especially difficult, as each PFT actually corresponds to several plant species, and EFs show, in general, a wide range of values among different plants (Kesselmeier and Staudt, 1999; Niinemets et al., 2011)."

42. *) P33972L25: Replace "can affect" with "affects" and add "modelled" or "predicted" or "calculated or similar in front of "regional and seasonal distribution".

Authors: Text corrected in section 1 as follows:

"Consequently, the high uncertainty related to LAI affects the predicted regional and seasonal distribution of BVOC emissions."

43. *) Is there no reference to STOMATE available?

Authors: The references Krinner et al., 2005 and Magnan et al., 2011 cover both STOMATA and SECHIBA module. We have shifted their position so that it is clearer (section 2.1).

"ORCHIDEE (Organizing Carbon and Hydrology in Dynamic EcosystEm) is a dynamic global vegetation model (Krinner et al., 2005; Magnan et al., 2011) that consists of two main parts:…"

44. *) P33975L4: What is "LPJ"? Is it the LPJ model that provides the surface areas provided for ORCHIDEE in Table 1?

Authors: For our study (see also point 10), the global vegetation distribution is prescribed for the runs using appropriate forcing (the Land-Use History - LUHa.rc2) related to the year 2000 as described in section 2.4. These files provide the surface areas in Table 1. The module mentioned is a part of ORCHIDEE and related to the dynamical vegetation component (LPJ). We did not activate this part. We therefore delete this paragraph that is misleading and we keep only, in section 2.1:

"For our study, the global vegetation distribution is prescribed for all runs using appropriate forcings, as described in paragraph 2.4."

45. *) P33975L16: Add "The" before "canopy". "Divided in up to 17 LAI layers" - this sounds very weird – I guess you mean that you split the total LAI into different canopy layers – please reformulate.

Authors: We reformulated the text in section 2.2.

"The canopy is considered split vertically into several LAI layers, the number of which (up to 17) depends on the LAI value. Emissions are calculated for each layer through consideration of the sunlit and shaded leaf fractions and the light extinction and light diffusion through canopy. In a second step they are vertically summed, providing a single value for each PFT and grid point."

46. *) P33975L25: Add "the" before "leaf level". Replace "in the" with "at". Now I will try to stop making note on this grammar stuff – please check it yourself. There are many following mistakes.

Authors: Text checked and corrected.

47. *) P33976L3: Add "the" before "emitted". Since CTL depends on the emitted compounds, why is it not CTL*i* instead?

Authors: We changed it. We also changed notation in equation (1) and (2) in order to be clearer (see point 51).

48. *) P33977L1: "in order to take into..."???

Authors: The phrase is deleted because the concept is explained shortly before.

49. *) P33977L3: "needleaf" is misspelled here and later.

Authors: Changed in the text.

50. *) P33977L8: "2-Methyl-3-Buten-2-Ol" → "2-methyl-3-buten-2-ol".

Authors: Changed in the text

51. *) P33978L6-10: I am very confused by your symbols. Is CTLI in Eq. 1 then not supposed to be CTL(l)?

We changed the use of notation in equation (1) and (2) and the text in section 2.2, to be clearer.

"The emission flux F of a specific biogenic compound (c), for a given PFT (i) at a LAI layer (l) is calculated following the equation (1):

$$F_{c,i}(l) = LAI_i(l) \cdot SLW_i \cdot EF_{c,i} \cdot CTL_c(l) \cdot L_c \qquad (2)$$

where $LAI_i(l)$ is the leaf area index expressed in $m^2\ m^{-2}$ at a particular LAI layer and PFT, SLW_i is the specific PFT leaf weight in $g\ m^{-2}$, $EF_{c,i}$ is the basal emissions at the leaf level for an individual compound and PFT…"

"BVOCs are now modelled to consider both light-dependent and light-independent emission processes, and the response to temperature and light (CTL) is calculated for individual compounds at each LAI layer (l):

$$CTL_c(l) = (1 - LDF_c) \cdot CTLI_c + LDF_c \cdot CTLD \cdot CL(l) \qquad (1)$$

LDF_c is the light-dependent fraction of the emission, specified for each compound emitted (Table 2)."

52. *) P33978L13-18: Either you write out the meaning of the symbols, or you leave out the equations and only refer to Guenther et al., 1995.

Authors: We deleted the equations.

53. *) P33980L20+L24: "2" and "1" → "two" and "one". Other places also with same mistake.

Authors: We changed the text.

54. *) P33981L12: Any reference or website to CRU-NCEP?

Authors: We inserted the website in section 2.4 (page 16).

"(http://dods.extra.cea.fr/data/p529viov/cruncep)"

55. *) P33982: "Ls"??? You have used Lc before – is that now the same?

Authors: The Referee #1 is right, we write now *"L_c"*

56. *) P33983L19-20: compounds does not start with a capital letter – please change it here and also other places in the text where you wrote it like that.

Authors: We changed it in the text.

57. *) P33988L19: Sure it's not "western Brazil"?

Authors: The Referee #1 is right. We changed the text.

58. *) P33989L1: "...can observe comparing..."?

Authors: The sentence does not exist anymore.

59. *) P33989L6-9: You start and end the line with emphasising that this is important for light dependent emission – I think you don't have to add "in the case of BVOCs that are strongly light dependent".

Authors: We deleted it in the text.

60. *) P33989L21-22: Don't write about the "solid black line" and "red line", since you confuse the reader, cause there are no such lines in Fig. 4.

Authors: We deleted it in the text.

61. *) P33990L20: "for each" → "with each".

Authors: Text corrected.

62. *) P33990L24-27: Please add reference to the figures that shows this.

Authors: The sentence does not exist anymore.

63. *) P33994L9: "non light-dependent" = "light independent".

Authors: Text corrected

64. *) P33995L14-20: maybe past tense works better.

Authors: We changed the text in:

"The main objectives of this study were to (i) present the new version of the BVOC emission module embedded in the ORCHIDEE model, (ii) provide BVOC emission estimates for the 2000–2009 period for a large diversity of compounds, (iii) compare the ORCHIDEE model results to emissions calculated by MEGAN in terms of global, regional and seasonal patterns, and (iv) investigate how

the uncertainty linked to some key variables or parameters such as the LAI and the LDF could affect the BVOC emission estimate in the two models."

65. *) Table 5: "Modis Lai" = "MODIS LAI"? You must have a mistake in the LAI info column for the simulations where you multiply LAI with 1.5! Is it really so that you used the air temperature for MEG_LDF and not the leaf temperature?

Authors: The LAI provided by MODIS is used as forcing in the MEG_CRU simulation. We corrected the mistakes related to ORC_LAI15, MEG_LAI15 in LAI column. We indeed use the leaf temperature in MEG_LDF simulation. We put "*T leaf*" in the T column.

66. *) Figure 11: The unit is not supposed to be in italic? From your figure text "The thick and thin dashed line represent..." → "The thick and thin dashed lines represent...". Maybe also worth to mention that the LAI peaks at different times in ORCHIDEE and MEGAN and why this is so. Also, Fig. 11 should be listed before Fig. 10, since it is mentioned in the text before Fig. 10.

Authors: We corrected the figure and related text and we reversed the order of Fig. 10 and 11.

We mention in section 3.4 (page 27) in the text that LAI peaks at different times in ORCHIDEE and MEGAN, giving a possible explanation.

"In addition the LAI peaks at different times throughout the year in ORCHIDEE and MEGAN. We investigate the contribution of different areas and we observe that, whilst in northern temperate region the MODIS LAI peaks in July and afterwards decreases quite fast, the ORCHIDEE LAI peak in both July and August. Furthermore, in the boreal region, the ORCHIDEE LAI peaks one month later (August) than the MODIS LAI (July). Therefore, the time shift observed globally is most likely due to the greater persistence of the growing season provided by ORCHIDEE in the northern temperate area and its delay in the northern boreal region compared with what is detected by MODIS."

67. *) Figure 10: Please include the results from MEG_CRU in this figure too. It helps for the comparison.

Authors: Figure changed.

References from Referee #1:

Ghirardo et al., Plant, Cell and Environment, 33, 781, 2010.

Mann et al., Atmos. Chem. Phys, 14, 4679, 2014.

Owen et al., Atmos. Environ., 36, 3147, 2002.

Spracklen and Righelato, Biogeosciences, 11, 2741, 2014.

Tsigaridis et al., Atmos. Chem. Phys., 14, 10845, 2014.

References in the answers:

Emmons, L. K., Walters, S., Hess, P. G., Lamarque, J.-F., Pfister, G. G., Fillmore, D., Granier, C., Guenther, A., Kinnison, D., Laepple, T., Orlando, J., Tie, X., Tyndall, G., Wiedinmyer, C., Baughcum, S. L., and Kloster, S.: Description and evaluation of the Model for Ozone and Related chemical Tracers, version 4 (MOZART-4), Geosci. Model Dev., 3, 43–67, doi:10.5194/gmd-3-43-2010, 2010.