Reply to Referee#2, Dr. Palmira Messina

We would like to thank the reviewer for the positive evaluation of the manuscript, the careful reading and for the useful comments and suggestions. Below we address the raised concerns. The reviewer's comments are *italicized*.

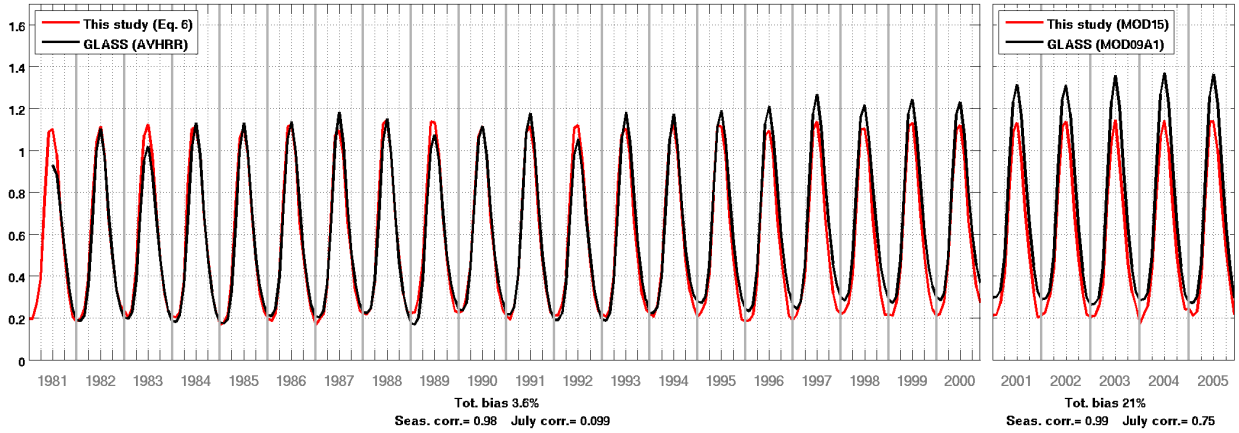
The paper presents a critical overview of the isoprene emission estimates at European scale for present and end-of-century period, considering many of the most important factors that can drive isoprene emission changes such as CO2 inhibition effect, CO_2 fertilizing effect and climate changes. It is remarkable the analysis on the different meteorological fields (ALARO and ERA-Interim), that greatly enriches the discussion. The paper is, in general, well written and organized. The results are clearly presented and discussed. I therefore recommend the publication of the present manuscript in Biogeosciences after having clarified the following points:

1) In section 2.2, the approximation used to derive the LAI before2003 seems to me quite crude. Where does equation (6) come from? Could you please detail more the scientific basis of this formula? Please explain why you do not directly use other LAI databases that better cover the period of your analysis like GLASS Leaf Area Index product (http://glcf.umd.edu/data/lai/) or GIMMS (Zhu et al. 2009). I would suggest to compare the LAI that you calculate with at least one of these databases to discuss the quality of your formula.

Zhu, Z.; Bi, J.; Pan, Y.; Ganguly, S.; Anav, A.; Xu, L.; Samanta, A.; Piao, S.; Nemani, R.R.; Myneni, R.B. Global Data Sets of Vegetation Leaf Area Index (LAI)3g and Fraction of Photosynthetically Active Radiation (FPAR)3g Derived from Global Inventory Modeling and Mapping Studies (GIMMS) Normalized Difference Vegetation Index (NDVI3g) for the Period 1981 to 2011. Remote Sens. 2013, 5, 927-948

Note that we could have used the MODIS climatological average LAI over 2003-2014; however, the relationship expressed by Equation (7) of the revised manuscript has the merit of capturing (part of) the LAI interannual variability associated with temperature variations. As indicated in the text, the relationship is not used when the correlation between LAI and T is low, which occurs only in a minority of cases. The coefficient B(x,m) is found to be almost always positive, reflecting the positive influence of warm conditions on vegetation growth. That being said, we thank the reviewer for the suggestion to evaluate our LAI dataset against other LAI datasets covering a longer time period than MODIS. Note first that neither GLASS nor the GIMMS dataset cover the entire target period of our study (1979-2014) and that therefore, extrapolation would have been required in any case. The comparison between the leaf area index from the GLASS dataset and from our study over Europe (see figure below) indicates a good agreement for both seasonal variation and the average LAI, especially before 2000. Interannual variability differs, however, as seen for example in the positive trend between 1981 and 2000 (1.5%/yr) in the GLASS dataset, as well as in the sudden increase in GLASS LAI after 2000, most likely due to the use of a different reflectance product. A recent intercomparison of 4 global LAI products has shown large inconsistencies regarding the interannual variability and trends, with for example the GLASS LAI trend being found to be 4.5 times higher than those of the GLOBMAP LAI dataset (Jiang et al. 2017).

Jiang, C., Ryu, Y. Fang, H. Myneni, R., Cleverie, M. Zhu, Z. Inconcistencies of interannual variability and trends in long-term satellite leaf area index products. Glob. Change Biol. 23 (10); 4133-4146 (2017).



2) In section 2.2, I am not sure to have understood the formula (7). If is a monthly correction factor for each site, where is the information about the month as in right part of the formula there are only annual averaged variables?

We have been sloppy with formula 7 (Eq. 6 in the revised manuscript), thanks for pointing this out. The text now reads :

'To account for observed solar radiation changes over Europe we performed a second simulation (H2) where the ERA-Interim downward solar radiation fields are adjusted based on homogenized composite time series of ground-based observations from 56 European sites (Sanchez et al. 2015). The sites are grouped in five large European regions (central, northern, eastern, southern and northwestern Europe, Fig.S2). We calculated the seasonally averaged solar radiation according to ERA-Interim at the locations of the observation sites over 1979-2014 and computed their averages $<SSR_{ECMWF}^{i,k}>$ over each large region i and each season k. The same procedure is applied for the ground-based observations, $<SSR_{obs}^{i,k}>$. We calculate correction factors

$$f_{i,k} = 1 + \Delta SSR_{obs}^{i,k}/<SSR_{obs}^{i,k}> - \Delta SSR_{ECMWF}^{i,k}/<SSR_{ECMWF}^{i,k}>$$

where $\Delta SSR_{obs}^{i,k}$ is the seasonal mean anomaly of solar radiation observed in region i, and $\Delta SSR_{ECMWF}^{i,k}$ is the corresponding anomaly of the ERA-Interim data. The correction factors $f_{i,k}$ are then applied to the solar radiation fields P of Eq.3.

3) I suggest to clearly indicate the spatial and temporal resolution for all simulations in Table 1 or in Section 2.2 where simulations are presented.

The first sentence of Section 2.2 now reads : 'The MEGAN-MOHYCAN model is run at hourly resolution on a 0.1^o x 0.1^o grid.'

4) As you said at the end of Section 2.3, the increase of 15% of LAI per 100ppm of CO_2 is a quite

crude parameterization. It is most likely that the various types of plants respond differently to CO 2 variation and the present parameterization do not take into account the dependency on Plant Functional Type (PFT). More generally the best way to model the future vegetation carbon balance (and so the LAI variation too) due to climate is to use dynamical vegetation models. The employ of this kind of models is beyond the scope of this paper, but I think that it's worth to discuss a little bit more this important point at the end of 2.3 section and insert a sentence in the perspective(see next point).

We inserted the following text at the end of Section 2.3 (now Section 2.4):

Dynamical vegetation models, e.g. ORCHIDEE (Krinner et al. 2005, Messina et al. 2016), would be required in order to provide a more mechanistic simulation of the LAI variations and of the distribution and structure of the natural vegetation but this lies beyond the scope of the present study. Note however, that dynamical vegetation models have identified weaknesses related to the use of a limited number of static plant functional types, and to the poor representation of species competition (Scheiter et al. 2013).

5) I would conclude with a short paragraph underlining the limits of this work, the possible evolution and future perspectives.

We inserted the following text at the end of the Conclusions :

The estimates provided in this study could be improved in future work by using e.g. meteorological output from more than one climate model, alternative long-term leaf area index datasets and especially, through the coupling with a dynamical vegetation model, in order to better evaluate model uncertainties related to climate and vegetation changes, and to better represent the complex and numerous biosphere-climate interactions. Moreover, the effects of soil moisture stress on isoprene emissions should also be considered, as climate scenarios frequently predict a higher occurrence of droughts in the future.

Technical corrections :

1) Page 2, line 20 : there many more global annual emission estimates, please look to Fig. 1 in Messina et al (2016) or Fig. 10 in Sinderalova et al. (2014).

Messina, P., Lathière, J., Sindelarova, K., Vuichard, N., Granier, C., Ghattas, J., Cozic, A., and Hauglustaine, D. A.: Global biogenic volatile organic compound emissions in the ORCHIDEE and MEGAN models and sensitivity to key parameters, Atmos. Chem. Phys., 16, 14169-14202, https://doi.org/10.5194/acp-16-14169-2016, 2016.

Sindelarova, K., Granier, C., Bouarar, I., Guenther, A., Tilmes, S., Stavrakou, T., Müller, J.-F., Kuhn, U., Stefani, P., and Knorr, W.: Global data set of biogenic VOC emissions calculated by the MEGAN model over the last 30 years, Atmos. Chem. Phys., 14, 9317–9341, doi:10.5194/acp-14-9317-2014, 2014.

We changed the sentence to 'Isoprene is the dominant biogenic hydrocarbon emitted into the atmosphere, with global annual emissions estimated between 250 Tg and 1000 Tg (Guenther et al. 2006, Müller et al. 2008, Lathière et al. 2010, Arneth et al. 2011, Guenther et al. 2012, Sindelarova et al. 2014, Bauwens et al. 2016, Messina et al. 2016)'.

2) Page 4, line 26: the equation (6) is not well formatted, I see one part of the formula on the right and another par t completely on the left side of the page.

Corrected.

3) Page 8, lines 26: how is the H3 simulation estimated at the field campaign locations? Is it simply a spatial interpolation?

We modified the text as : 'Figure 5 shows the monthly averaged mid-day fluxes estimated in the H3 simulation at the model grid corresponding to the location of 9 field campaigns.'

4) Page 10, lines 13-14: could you please detail more the sentence "An equally likely explanation is uncertainties associated with the activity factors representing the impact of past temperature and solar radiation in the MEGAN model (Eq. 2, 4)"? For example clarifying in which way the activity factors γ_T and γ_P set in MEGAN can explain the differences between modeled and observed seasonal pattern of isoprene.

We replaced the sentence by 'It should be reminded that the activity factors γ_T and γ_P have their own uncertainties which might also impact the modeled seasonal variation.'.

5) Page 12, lines 9-13 and 28-33 the two sentences "Precipitation plays only...particular over southern Europe" and "As the present study neglects... regions (Bauwens et al., 2016)" are both centered on soil moisture, I suggest to move the first sentence merging it with the second one.

We adopted the suggestion.

6) Page 12, lines 15-16: you say that the effect of CO2 fertilization increases by +15%for RCP4.5 and +32% for RCP8.5 compared to the simulation accounting only for climate effects, but is not rather equal to 19% (that is 52-33=19) forRCP4.5 and 58% (141-83=58) for RCP8.5?

In the case of RCP8.5, the flux is equal to the standard (S) multiplied by (1+1.41) when fertilization is considered, and it is equal to S multiplied by 1+0.83 without fertilization. The effect of fertilization is therefore the ratio (1+1.41)/(1+0.83)=1.32.

7) Page 2, line25: the plots related to "climate+CO_2 fert+inh (WH)" configuration are not present in Fig. 9, so after the sentence "...between 11% and 65% (using Wilkinson et al. (2009))", I would put "(not shown)" or I would add the concerning plots in the Fig. 9.

We adopted the suggestion and added 'not shown' in the parentheses after '(using Wilkinson et al. (2009), not shown)'

8) Page 13, line 3: in the sentence "The large dispersion of the different estimates of Fig. 9..." , do you mean Fig. 10?

Corrected.

9) Page 14, line 5:instead of "...to increase by up to 65%", I suggest to put the variability range.

The sentence now reads : '…the end-of-century isoprene emissions are calculated to increase by 0-11%, 9-35% and 17-65%, according to the RCP2.6, RCP4.5 and RCP8.5 scenarios respectively (Table 1).'

10) Page 18, line 24 : to respect the alphabetical order I would put "van der Schrier..." further.

Thanks for spotting this.