Reply to Referee#1, Dr. Alexandra-Jane Henrot

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

This paper presents a series of present-day and future high-resolution simulations of isoprene fluxes over Europe. Isoprene emissions from vegetation are calculated using the MEGAN-MOHYCAN model forced with meteorological fields derived from ECMWF ERA-Interim reanalysis for the recent period, and from future ALARO regional climate model simulations following several representative concentration pathways (RCPs) scenarios. The effects of changing climate (mainly temperature and solar radiation), CO_2 fertilization, and CO_2 inhibition on the distribution and variability of isoprene emissions are tested and discussed. Isoprene emission estimates for the recent period are evaluated against field campaign measurements at several European sites, showing the reliability of the model to reproduce the observations.
The manuscript is well written and clearly structured. The methodology and results are described very comprehensively. The overall results appear reasonable and are well discussed. I therefore warmly recommend the publication of the present manuscript in Biogeosciences after addressing minor comments listed below.

Specific comments

Section 2, lines 26 – 30: Could you please give the SEFs attributed to each of the seven PFTs used here (maybe in a table in the main text or in the supplement material) and explicit how they have been obtained. I think it is relevant to mention them here, as you discuss the SEF values in section 4.2. I suppose you worked with the SEFs of the 7 PFTs used in MEGANv2.0 (Guenther et al., 2006). Why didn't you use the more recent MEGANv2.1 (Guenther et al., 2012) 15 PFTs distribution and corresponding SEFs, which give you more details than the 7 PFTs of the previous version?

As indicated in the manuscript, the distribution of the basal emission factor is obtained from MEGANv2.1. This distribution is now displayed as Fig. S1 in the Supplement. The PFT distribution (here from Ke et al., 2012) is an input dataset required by the canopy environment model (MOHYCAN), as canopy structure and leaf properties are PFT-dependent. The manuscript has been clarified to avoid possible confusion:

"The MEGAN emission model (Eq. 1) includes the specification of a standard emission factor epsilon (mg m^{-2} h^{-1}), representing the emission under standard conditions as defined in Guenther et al. (2012). The distribution of the standard emission factor ε (Fig. S1) is obtained from MEGANv2.1. It is based on species distribution and species-specific emission factors (Guenther et al., 2012). The MOHYCAN canopy environment model requires also the specification of the plan functional type (PFT). The PFTs are defined ..."

Section 2.2: I suggest to split the subsection into two subsections: first "2.2 Input data and assimilation" regrouping the lines 6 to 34 of page 5 and 1 to 8 of page 6, and to describe the *meteorological forcings, and "2.3 Leaf area index" regrouping the lines 22 to 25 of page 4 and 1 to 5 of page 5, to describe the leaf area index.*

Thank you for the suggestion. We modified the manuscript accordingly.

Section 2.3, lines 15 – 19: Could you please give explicitly the CO_2 concentration for which γ_{CO2} is equal to 1 with the WI parameterization, as it is slightly higher than with the PH parameterization.

Done (402.6 ppm).

Section 3, line 10: How is it possible to distinguish between the contributions of Oak species in the isoprene emission you obtain, as you work at the PFT level? The broadleaf deciduous tree PFT regroups several Oak species, including the low and high Oak emitters, and a mean SEF is used for this PFT. Aren't the temperature and radiation effects mainly responsible for the higher isoprene emissions in the Mediterranean regions?

As explained above, our SEF distribution is based on species-specific emission factors and distributions. Besides temperature and solar radiation, the calculated isoprene emission is also clearly driven by the standard isoprene emission factors, as shown by the comparison between the MEGAN emission factor map (Supplementary Figure 1), and the isoprene flux distribution (Fig. 2). The elevated emission factor patterns in Croatia, Bosnia-Herzegovina, Spain, Algeria are also present in the emission maps. Oak, pine and beech is the dominant vegetation in these regions.

Section 4.1, lines 11 – 20: Could you please discuss why you have significantly lower isoprene emissions with MEGAN-MOHYCAN in comparison to previous MEGAN and satellite-based estimates? What are the factors that can explain the differences between your results and previous estimates?

The difference with respect to MEGAN-MACC over Europe (22%) is apparently mostly due to differences in above-canopy temperature and solar radiation between ECMWF ERA-Interim (used in our study) and MERRA2 (used in MEGAN-MACC) (Katerina Sindelarova, personal communication). The difference with respect to top-down estimates is small (~20%), considering the large uncertainties in both top-down and bottom-up estimates.

Section 4.2, Line 16, Page 9: Could you please make explicit the unit mentioned here (µg g-1DW h-1) ?

We added that DW denotes dry weight of leaf biomass.

Section 4.3, lines 15 – 34, page 10: Why did you choose to validate the model using the Stordalen site, as the MEGAN algorithms are not correctly adapted to the type of vegetation found in the site? Isn't it another site (forest site) with available isoprene flux measurements to validate the model, without strong bias induced by the vegetation type?

As shown in Fig. 5, comparisons were performed at 9 sites, most of which are dominated by forests (Fig. S5). We don't understand the objection, the comparison is meant to test MEGAN in different environments.

Section 4.3, lines 26 – 28, page 10: You argue that the value of SEF used in the model for C3 arctic grasses can be underestimated and lead to lower calculated emissions. However, the basal emission rates of the dominant vegetation types of the site (given in line 20 of Section 4.3) are lower than the SEF used in the model. How do you explain this?

The basal emission rate reported by Eckberg et al. (2009) is an emission rate per unit leaf area at a temperature of 20^{o}C, and therefore cannot be directly compared with the MEGAN SEF. To avoid confusion, we deleted the mention of the Eckberg et al. emission rate, which was not useful for the discussion.

Section 5.2, lines 11 – 22, page 13: Including the impact of land-use change in your simulation set (e.g. using the ALARM scenarios (Settele et al., 2005)), with additional simulations accounting only for land-use change would have been very interesting. I understand that implementing the land-use change scenarios to obtain vegetation maps is a hard work to do. As you discuss this effect here, could you also add some perspectives in the conclusions about land-use impact on isoprene emissions with MEGAN-MOHYCAN?

Settele, J., Hammen, V., Hulme, P. et al., ALARM: assessing LArge-scale environmental risks for biodiversity with tested methods. Gaia - Ecological Perspectives for Science and Society, 14, 69–72, 2005.

We added a sentence at the end of Section 5.2 : 'The application of land use change scenarios (e.g. those of the ALARM project, Settele et al. 2005) to future isoprene emission estimates with MEGAN-MOHYCAN will be carried out in future work'.

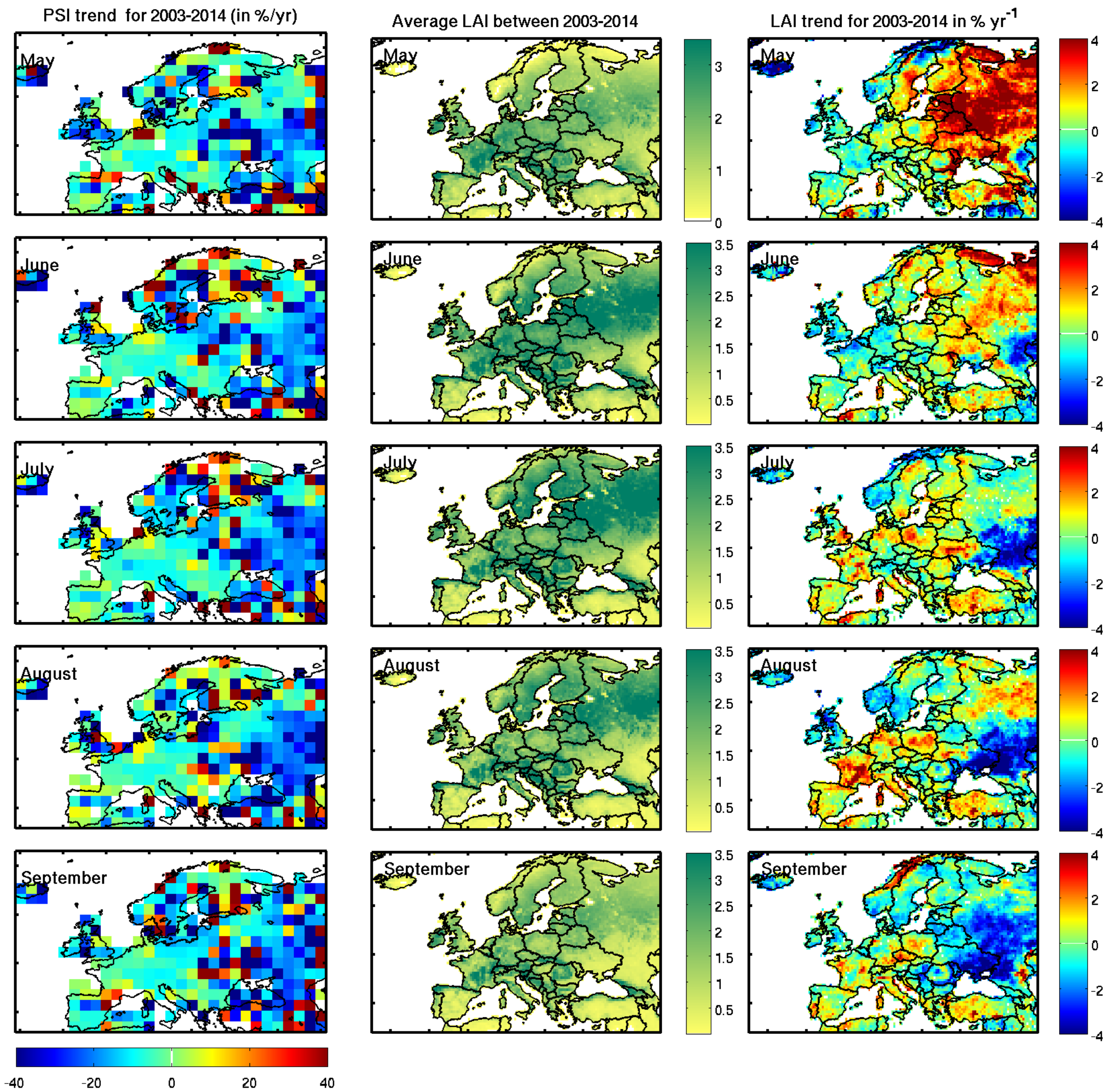
Conclusions, line5, page 14: Where does the 65% of increase come from. Could you explain how, from which simulation you obtain it?

We followed the suggestion of referee#2 and provided variability ranges. 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).'

Figure S2: How do you explain the strong negative trend in July LAI for southeastern Europe (Ukraine, Romania,...)?

The decreasing LAI trends in these regions are likely driven by the decreasing water availability, as reported in Zhu et al. (2017) based on long-term LAI data records. To test this hypothesis, we used monthly PSI data (Palmer Drought Severity Index) over 2003-2014 openly available at https://www.esrl.noaa.gov/psd/data/gridded/data.pdsi.html. This index represents the severity of dry and wet spells based on monthly temperature and precipitation data as well as the soil-water holding capacity. The calculated PSI trends for all months between May and September illustrated in the figure below (left panels) confirm the negative trend in PSI value in South Russia and Eastern Europe, corresponding to the increased drought severity over this period.

Zhu, Z., Piao, S., Lian, X., Myneni, R. B., Peng, S. and Yang, H. Attribution of seasonal leaf area index trends in the northern latitudes with 'optimally' integrated ecosystem models, Global Change Biology, 23, 4798-4813, doi:10.1111/gvb.13723, 2017.



Technical comments:

Abstract, line 2: Replace "aerosol" by "aerosols"

Corrected.

Abstract, line 3: Replace "...solar radiation: in addition..." by "...solar radiation. In addition.."

Corrected.

Abstract, line 5: Delete "also"

Removed.

Abstract, lines 11-12: "as a result of climate change" could be deleted

Removed.

Line 28, Page 3: Replace "and consider" by ". Seven PFTs are considered: ..."

Replaced.

Line 26, Page 4, EQ. (6): The equation is split into two lines, maybe an error in the latex code

Corrected.

Line 1, Page 7: Rephrase "ecosystem models, a widespread increase in LAI.." by "ecosystem models, Zhu et al. (2016) obtained a widespread increase in LAI ...", and delete the reference at the end of the sentence.

Done.

Line 7, Page 9: Replace "In other to" by "In order to"

Corrected.

Line 32, Page 9: Replace "trees species" by "tree species"

Corrected.