Response to the Reviewer 1

We thank the reviewer for the valuable comments. Our responses to questions and suggestions are outlined below.

Reviewer:

- p. 9249, l. 6: "... equal of exceed ...", please substitute "equal or" with "largely". It is consistent with what said in the next sentences.
- p. 9249, l. 11-12: ".. an inhabitant ...", probably a typo.
- p. 9249, l. 18: a reference such as Atkinson et al., Atm. Env. 2003 looks better than Ashworth et al. in this context.
- p. 9249, l. 19: "... equal to that of methane", may be useful to put a number.
- p. 9250, par. 2: May be worth mentioning also other global BVOC models, such as those described in the already cited paper by Arneth et al. 2011.
- p. 9259, l. 17-18: suggest adding a reference after this statement.
- p. 9259, l. 24: qualify –> quantify
- Figs. 1-3-5: I suggest showing the differences with same red/blue color codes used in Fig. 9, it is much easier to read
- Fig. 1-4-5-7: I suggest to redefine the color scale of emissions in these figures, because there is a lot of white space. E.g. over Europe, which is one of the focus area of the paper, there seem to be no emissions

Reply:

We agree with the reviewer and have revised the manuscript accordingly.

Reviewer:

- p. 9250, l. 1: "...ensures ... models are forced by same land use", this statement, true in general, is not true in this work as illustrated later. Should be mentioned here.

Reply:

The sentence is true for DEHM that uses GEIA. The land cover defined by Olson et al (1995) is used for both GEIA isoprene emission model and the dry deposition calculations of DEHM. Furthermore, because this statement is not true for MEGAN, we have revised the text indicating that it is a general statement.

Reviewer:

- p. 9251, l. 18-19: "... but not evaluated with observations." I am aware of at least one other paper on ACP that compares CTM-MEGAN results against measurements over Europe (Cursi et al. 2010, http://www.atmos-chem-phys.net/10/11501/2010/acp- 10-11501-2010.html).

- p. 9261, par. 1: these results on ozone levels might be comapared with those reported in the cited paper of Curci et al. 2009. Are that and present studies consistent?

Reply:

We thank the Referee for this reference. We have added it to the text in introduction section and corrected the specific sentences. Furthermore, we have provided the statistical parameters (correlation and fractional bias) for isoprene evaluation in Table 1 so that the reader can easily compare our results with the cited paper and we also discussed the consistency of our results with those of this reference.

Reviewer:

- p. 9252, l. 10: "... two-way nesting ..." is this also for chemistry? If yes, it is quite an original point of the model that might be enphasized.

Reply:

Yes, it is also for chemistry, but in this study, we have just performed the model simulations for the mother domain. Nevertheless, in the previous version of the manuscript, we used two-way nesting and showed the ozone concentrations over Europe in the nested domain. In the revised version we have skipped to do that due to negligible differences for the model performances using the two isoprene models in the nested domain.

Reviewer:

- p. 9252, l. 24-25: the reference for the chemical mechanism suggest that it is quite outdated. E.g. Stone et al. 2011 (http://www.atmos-chem-phys.net/11/6749/2011/acp- 11-6749-2011.html) discuss (among many others) new parameterizations of isoprene degradation, which apparently are not used here. Please add a remark is this is correct, because the chemical mech certainly has an important impact on results.

Reply:

Although DEHM uses the chemical scheme that is based on the approach of Strand and Hov (1994), the scheme has been extended by updating a few chemistry rates of reactions, where isoprene is involved (as mentioned in the text). On the other hand, although modeling of chemical pathways are important for evaluation of isoprene emission models with respect to ozone concentrations; here our focus is primarily to compare the ability of two BVOC models. In other words, the chemical mechanism is kept fix while comparing the two BVOC models. However, we agree with the reviewer, that further work on the chemical mechanism could improve the simulations of ozone concentrations.

Reviewer:

- p. 9254, l. 1-13: from the long list of MEGAN features is not clear what are used here. For example, is the CO2 effect included? Please clarify.

Reply:

CO2 effect is not included. We have extended the text to clarify more what are used. Moreover, effect of soil moisture, which had been ignored, has been implemented in the new version and we have modified the results accordingly.

Reviewer:

- p. 9255, l. 15-20: Guenther et al. 2006 report a better estimate of global isoprene flux of about 600 Tg/y. The authors seem to attribute the difference with their higher estimate (732 Tg/y) to the temporal resolution of the meteorological data. Is this correct? If so, what is the frequency used here and in Guenther 2006? Please clarify.

Reply:

First, our estimation is decreased to 592 Tg/y after we included the soil moisture effect and corrected a bug in reading the LAI that existed in previous runs. Therefore, our new simulated value is not far from the flux reported by Guenther et al. (2006). They derived hourly temperature and PAR from original 6-hourly values of the NCEP-DOE reanalysis weather datasets, and our hourly data similarly derived from 6-hourly data of the NCEP_FNL. Therefore, temporal resolution of the meteorological data could not be the reason for the slight difference.

The remaining difference could origin from other uncertainty sources. For instance, in this study we implemented the PCEEA approach as an alternative method to *the standard canopy environment emission activity factor* which was used for estimation of 600 Tg/y by Guenther et al., 2006. PCEEA algorithm is a simplified single-layer canopy-scale representation of the multi-layer model. As Guenther et al. (2006) mentioned, the PCEEA algorithm estimates annual isoprene emissions that are within 5% of the value estimated using the standard MEGAN canopy environment model. However, differences can exceed 25% for estimates at specific times and locations.

Reviewer:

- p. 9256, l. 24-25: not clear what EF are used in the sensitivity run: the same of GEIA? Please clarify.

Reply:

This issue is clarified in the manuscript as follows:

‘’In GEIA, All ecosystem types were assigned with one of the five values of emission factors given in Guenther et al. (1995) and summed to estimate the total emission for a location, while MEGAN uses an approach that accounts for geographic variations in the emission factors attributed to each PFT.’’(P8, 21-24).

‘’we carried out MEGAN simulations with an alternative emission factor scheme. This assigns a single isoprene EF for each PFT based on the approach used in GEIA ‘’(P8, 26-28).

Reviewer:

- p. 9257, l. 9-12: "... the effect of soil moisture is not included ... then MEGAN has higher emissions ...". The authors seem to imply that GEIA includes soil moisture effect: is this correct? If not, please reformulate, because it cannot be a difference between the two inventories.

Reply:

We agree with the reviewer and have reformulated the text. However, in the revised version we have considered soil moisture effect on MEGAN.

Reviewer:

- p. 9258, l. 18: "... are in acceptable agreement ...". I strongly suggest avoiding the use of such vague statements. Just describe quantitatively your results, as done in the following.
- p. 9258, l. 29/p. 9259, l. 1: "... a very small difference ... uncertainty within a factor of 4 ...". The two statements do not sound very consequential, a factor of 4 is not a very good agreement. Please reformulate

Reply:

We agree with the reviewer and have reformulated the text.

Reviewer:

- p. 9262, l. 26-27: "... since emissions rates depend linearly...", I do not agree this could be a reason for the sensitivity of emissions rates to emissions factors. ER have an exponential dependency w.r.t. temperature, so in principle that must be the driving factor. Please reformulate.

Reply

We agree with reviewer and have revised the text