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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of two isoprene emission models for use in a long-range air pollution model" *by* A. Zare et al.

Anonymous Referee #1

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The manuscript present an original chemistry-transport model comparison between results obtained using two widely-used global biogenic isoprene emission algorithms: GEIA and MEGAN. MEGAN is the heir of GEIA and is set to be replaced by MEGAN in models. However, a direct comparison of the impact of switching from one inventory to the other on the global scale is still lacking, and this work may fill the gap. The results presented by Zare et al. here strongly encourages modellers using GEIA to migrate to MEGAN.

I suggest publication of the work, after the minor points listed below are addressed:

- 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, 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.

- 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. 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. 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.

- p. 9252, l. 24-25: the reference for the chemical mechanism suggest that it is quite outdated.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.

- 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.

- 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.

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

- 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.

- 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.

- p. 9259, l. 17-18: suggest adding a refence after this statement.

- p. 9259, l. 24: qualify –> quantify

- 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?

- 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.

ON FIGURES:

- 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

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of the paper, there seem to be no emissions.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 9247, 2012.