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Interactive comment on "Implementation of the biogenic emission model MEGAN(v2.1) into the ECHAM6-HAMMOZ chemistry climate model. Basic results and sensitivity tests" *by* Alexandra-Jane Henrot et al.

Anonymous Referee #1

Received and published: 27 October 2016

Major comment

This is a well-written and useful description of the implementation of MEGAN into a chemistry climate model, providing also interesting comparisons with a large number of previous MEGAN-based modelling studies. In addition, sensitivity simulations were performed to quantify the impact of different factors influencing the estimated emissions. The overall results appear very reasonable and are pretty well described and diagnosed.

My only major objection lies is the use of a hybrid MEGAN model algorithm which

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Discussion paper

mixes elements of the version 2.1 (Guenther et al., 2012) with the PCEEA developed for isoprene as part of MEGAN v2 (Guenther et al., 2006) and with the earlier algorithm of Guenther et al. (1993). The reader might want to know whether this setup provides results similar to those of the full version 2.1. Besides problems with the temperature dependence of light-independent emissions (see Specific comments, below), I am especially worried that the dependence on LAI is not correct. In Equation 3, I assume that γ_{LAI} multiplies both ligh-dependent and light-independent terms. The expression of γ_{LAI} is obtained from PCEEA, which is fine for light-dependent emissions, but certainly not for light-independent emissions, which are essentially proportional to LAI (cf. Equation 2 in Guenther et al., 2012). It is possible, although I'm not certain, that the authors were misled by a recently published model study (Messina et al., ACPD 15, 33967-34033, 2015). Messina et al. made the very surprising claim that, according to MEGAN, monoterpenes emissions show very little sensitivity (less than isoprene emissions) to changes in LAI. This result cannot be correct, and is contradicted by their reported results obtained with the ORCHIDEE model. Basically, the emission is proportional to the amount of leaf biomass, which is proportional to LAI. For light-dependent emissions, light attenuation dampens this relation. For light-independent emissions, this effect does not exist. Could the authors also estimate the sensitivity of their estimated isoprene and monoterpenes emissions to a given change in LAI (say, a factor of 1.5)?

To summarize, I recommend this article for publication in GMD, if the authors address this major objection, as well as the minor comments listed below.

Specific comments

The title is long, I think that its second part (Basic results and sensitivity test) could be dropped.

Page 3 line 30: please provide some more details on those global potential land cover maps. In what sense are those for potential land cover? Do they include realistic

representation of human influence (e.g. crops, managed forests, etc.)?

Page 4 line 25: the common MEGAN assumption that LDF is a species-specific constant is very unrealistic for monoterpenes (e.g. Rinne, Atmos. Chem. Phys. Discuss., 15, C11977–C11979, 2016). This should be (at least) mentioned somewhere in the manuscript.

Page 4 line 26: the temperature activity factor for light-dependent emissions is obtained from G06 (Guenther et al., 2006) but I find several differences between G06 and the expressions given in the supplement, e.g. the factor C_{eo} is equal to 1.75 for isoprene in G06. Please explain.

Page 4 line 28: this temperature dependence (Guenther et al. 1993) is considerably simpler than the temperature activity factor of Guenther et al. 2012. Please discuss the possible implications of this simplification.

Page 7 line 14: the globemission website does not provide the box locations. The latitudes/longitudes of the regions should be specified in the figure legend.

Figures 3, 4, 6, 15: please enlarge the fonts, or enhance resolution for better readability

Page 8, lines 30-32: Messina et al. fail to provide good reasons for this supposed lower sensitivity of BVOC emissions to LAI in MEGANv2.1. The ORCHIDEE sensitivity to LAI makes much more sense.

Page 13 lines 2-3: here the soil water activity factor is said to depend on relative soil water amount. But the supplement reports a dependence on volumetric water content, as in G06. Are those two quantities the same thing?

Page 14 lines 21-24: Interannual variability is not well quantified by the ratio of maximum and minimum values. Since the periods covered by the different studies are all different, I'm afraid that the comparison amounts to a comparison of apples with oranges. It would make more sense to compare the standard deviation of annual totals in the different datasets.

Page 15 lines 8-9: I suppose that the process working on here is the influence of precipitation on soil moisture and hence on γ_{SM}. If so, this should be stated explicitly.

Technical comments

Abstract, line 4: please specify that the total given (634 Tg C year-1) is for the reference simulation.

Abstract, line 10: replace "with the biogenic model" by "here"

Abstract line 14: "PFT-dependent emission factor distribution" is unclear, use rephrase.

Page 2 line 5: replace "as well" by "also"

Page 2 lines 24-25: each reference is given twice, delete the second occurrence

Page 3 line 4: remove comme after "reactions"

Page 3 line 15: insert degree sign (°) after 1.8

Page 4 line 9: "emission potential (...) into the canopy" is weird. The BVOCs are emitted BY the canopy.

Page 4 line 10: remove space before "i. e."

Page 4 equation (3): a parenthesis is missing in this equation.

Page 4 lines 24-25: "The activity factor for temperature is divided into (...) factor" is unclear and could be rephrased e.g. as "Different expressions for the activity factor for temperature are considered for light-dependent (γ_{TLD}) and light-independent (γ_{TLI}) emissions"

Page 4 line 26 "The light dependent factor" could be replaced by "For light-dependent emission, this activity factor..."

Page 4 line 27: same, for light-independent emissions

Page 5 line 26: replace "Sect." by "Table"

Page 5 line 32: insert a comma after "emission factors"

Page 5 line 33: "correspondence"

Page 5 same line: "classifications"

Page 6 line 9: replace "decrease" by "lies"

Page 6 line 15: delete "together"

Page 6 line 16: "correspondence"

Page 6 line 14: delete "the" after "All"

Page 6 same line: replace "south" by "southern"

Page 6 lines 20-21: suggest replacing "mainly impact" by "are the main driver for"

Page 8: the parentheses are mixed up for Misztal et al. 2015

Page 9 line 9: "Its"

Page 9 line 11: replace "emissions" by "CO_2 levels"

Page 10 line 8: replace "are" by "is"

Page 10 line 11 "PFT-specific emission factors calculated...": weird, please rephrase

Page 10 lines 22-23: parentheses mixed up for Guenther et al. 2012

Page 14 line 12: parentheses wrong for Zhang et al. 2014

Page 14 line 24: replace "fairly agrees" with "agrees fairly well"

Page 15 line 6: "impacts"

Page 15 line 17: replace "depending on" by "of"

Page 15 line 22: see remark above regarding interannual variability

Table 2: Please explain in the legend the meaning of the numbers shown into paren-

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