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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Process based inventory of isoprenoid emissions from European forests: model comparisons, current knowledge and uncertainties" *by* T. Keenan et al.

Anonymous Referee #1

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General comments

This manuscript explores a crucial question in biosphere-atmosphere interactions: are current leaf-level models of biogenic volatile organic compounds (BVOC) emissions able to be extrapolated into the future? By incorporating two processed based models and an empirical model with the same emission factors and scaling methodology, the authors find a very interesting result. The three leaf-level models concur for present conditions, but they differ by almost a factor of two when making predictions for the end of the current century. This supports the thesis proposed in a recent paper (Arneth et al., 2008, ACP 8(16):4605-20) that the current agreement of BVOC emission models

is an "illusion of convergence."

Given the importance of the topic, I suggest the manuscript and underlying analyses be substantially revised before publication in ACP because of the following three major issues. (1) I recognize that synthesis is an important endeavor for advancing complex topics in earth system science, but the manuscript devotes too much effort to restating the work of previous investigators. (2) In the abstract, discussion and conclusions, the authors highlight the importance of their improved species-specific emission capacity database. But this improved database does not have a significant impact on their estimate of total annual European isoprene emissions (page 6171, lines 25-27). (3) The discussion of differences between the model results is very qualitative. Because the authors have implemented the three models in a common framework, they are uniquely positioned to explore the varying results at a very detailed level. In summary, by reducing the emphasis on describing previously published work and their emissions capacity database, the authors could focus on a detailed analysis of why the models produce different results for the end of this century.

Specific comments

Too much text is devoted to restating how the three leaf-level models work. For the Guenther model (section 2.1.1), the first ten lines on page 6153 are appropriate. From line 11 to line 4 on the following page, the manuscript provides no new scientific insight. Along with lines 9-17 on the same page, this material should be condensed or eliminated. Lines 4-8 are much better–they are unique to the current study. For the Niinemets model (section 2.1.2), from line 19 on page 6154 to line 4 on page 6155 is fine. From line 5 to line 6 on the next page should again be greatly condensed or eliminated. A similar analysis should be performed for the Martin model. Also, the paragraph starting at line 22 on page 6157 is an excellent example of the analysis that is appropriate for publication.

The discussion of the incorporation of phenology starting on line 8, page 6158 is a bit vague. The authors should clearly state the algorithm that empirically implements the time lag for the onset of isoprene emissions. Although the algorithm is common to the three modeling approaches, the authors should address its extrapolation to future climate conditions. Also, the discussion on soil water availability effects starting on line 23 on the same page should address the observation that drought stress can also stimulate canopy isoprene emissions (Pegoraro et al., 2007, Functional Plant Biology, 34: 774-84).

The discussion of the GOTILWA+ model should also be abbreviated. In particular, the paragraph starting at line 13, page 6162 is not very relevant to BVOC modeling. The paragraph starting on line 12, page 6163 (section 2.3.3) is another example of what would be appropriate to include.

In the results section 3.1 and the corresponding discussion starting at line 6, page 6170 is an example of where the authors need to dig into their model results to gain a deeper understanding of the differences between the three leaf-level models. A figure that gives a detailed presentation of which elements of the process based models drove the midday depression would add impact to the general observation that the empirical model missed this facet of the diurnal cycle. A great example of a beginning of this type of analysis is given on lines 19-20 on page 6168 (but the data should be provided in a figure).

In the next section (3.2), it is not clear from Figure 2 that the dataset "was accurately simulated by each model." In particular, there appears to be a large discrepancy during the first half of 2000 for each of the models. Perhaps this is covered by the statement about larger variability later in the paragraph? Also, the statement about missing data in 2002 is also not clear. Perhaps what's presented as very low emissions is missing data? If so, the data is not correctly displayed.

As a more general note, the statistical treatment of the comparisons in the results is weak. No statistical measures are used in section 3.1. In section 3.2, only a simple

linear regression is employed. But only r2 and p values are displayed. At the very minimum, the slope of the regression lines in section 3.2 should be discussed. And in section 3.4, the manuscript states that "no significant difference was observed between emission model predictions for this period." There is no discussion of a statistical test–was this merely a qualitative judgment?

The paragraph starting at line 15, page 6170 goes into the importance of the revised species-specific emissions database. But then the authors state on lines 25-27 that their emission inventory is similar to previous results because isoprene emissions are "dominated by a few highly emitting and well documented species." I believe this statement is correct, and the manuscript should reflect this fact as noted above under general comments.

On lines 14-16, a key difference in sensitivity between the Guenther and Martin models is stated, but there is no follow up to delve into the reasons for this observation. Again, this is the type of question that must be addressed for this paper to be published.

Technical comments

In the abstract on lines 7-10, the authors state they will "explore the interactive effects of climate, vegetation distribution, and productivity, on leaf and ecosystem isoprenoid emissions" but this topic is not addressed in the manuscript.

Page 6150, line 22: The sentence is not grammatically correct.

Page 6161, line 13: EU 15+2 is defined later in the paper, but should be defined here.

Page 6164, line 17: "Eddy" should not be capitalized.

Section 2.4: The source of the meteorological data should be described.

References: The references are not all in alphabetical order.

Figures 3 and 4 are a bit confusing. The figure caption for 3 states these are "Estimated average annual isoprene emissions (log scale) from European forest **canopies**" while for 4 it states “Estimated average annual monoterpene emissions (mgC m-2 a-1) from European forest **species**.” I imagine one of these is incorrect. Also, the manuscript refers to “canopy emission factors” (line 16, page 6166) for these figures, which is not the same as averaged annual emissions. This must be clarified.

The error bars in Figure 7 are confusing. Do they represent spatial variability? If so, they appear to show significant differences between the model simulations, which would have nothing to do with spatial variability. Or perhaps the climate predictions were ensembles? Then the error bars are more appropriate.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 6147, 2009.