

Interactive comment on “Estimation of biogenic volatile organic compound (BVOC) emissions from the terrestrial ecosystem in China using real-time remote sensing data” by M. Li et al.

Anonymous Referee #2

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This study presents an high-resolution inventory of BVOC emissions in China based on the MEGAN algorithm. The article boasts that past studies were based on outdated algorithms and too coarse meteorology. Although there are nice aspects to this work (e.g. the high resolution), I want to point out that

- (1) Chinese isoprene emissions were already computed using MEGAN by e.g. Guenther et al. 2006 and others, so the novelty here lies in the combination of MEGAN and high resolution;
- (2) the paper doesn't assess the impact of the claimed improvements, so that we are left clueless as to their real importance;
- (3) the article does not provide any evaluation against e.g. field measurements or

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HCHO satellite retrievals;

- (4) the paper does not address the impact of possible uncertainties in input data, e.g. in meteorological variables, LAI, etc. for example we don't know how the meteorological fields from the MM5 model compare with meteorological analyses e.g. from NCEP, ECMWF; and
- (5) the isoprene algorithm used in this article is a simplified version (PCEEA) of the MEGAN model, which might differ from the "full" MEGAN model by 25% or more at specific times and locations (Guenther et al., 2006). Also the soil moisture stress effect is ignored, with unknown consequences.

I think that the text should be adapted in order to reflect those various limitations. Sensitivity calculations (e.g. with alternative meteorological fields) should be performed to evaluate some of the possible sources of uncertainty. Comparisons with (1) at least some BVOC flux measurements, and (2) with other MEGAN-based inventories should be provided.

Minor corrections

Abstract l. 5 "outdated algorithms": see above remark. Please rephrase.

Abstract l. 7 what is meant by "dynamic models"? Please clarify.

Abstract l. 8 "large inaccuracies in the estimated results": the article doesn't provide any quantitative assessment of those inaccuracies.

Abstract l. 9 "to further explore the role of BVOCs in the atmosphere": please drop this part of the sentence, this is not the scope of the paper.

Abstract l. 17 "a relevant value ...": what is meant by "relevant"?

Abstract l. 27-29 "In this study, we present... in atmospheric processes": drop this sentence which does not provide any new information.

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p. 6554 l. 17 "did not fully consider... LAI...": wrong regarding LAI. Most studies did consider the influence of LAI. Please rephrase.

p. 6556 l. 16-21 Use of MM5. High resolution is good, but still the MM5 fields could be biased. A comparison of MM5 output (for air temperature and PPF) with meteorological analyses (or with measurements) should be conducted.

p. 6557 l. 22 To what extent is the MODIS distribution of vegetation fractions consistent with the tree species distributions used in MEGAN?

p. 6558 l. 2-3: "The land-use distribution in China changed dramatically over the past decade": How do you know if the differences are due to temporal changes or to inconsistencies (or errors) between the two databases? It seems very unlikely that Southeast China was mostly covered by crops as suggested by the USGS map.

p. 6560, Fig. 3: The isoprene standard emission factor presents a hot spot in South-eastern Tibet in Fig. 3. These high values were absent in the standard emission factor map presented by Guenther et al. (2006). Please explain.

p. 6564 l. 12 "an observation attributed to the lack of light dependence of monoterpene emissions...": this is in contradiction with your statement on p. 6554 l. 20 (light dependence). Please clarify.

p. 6566 l. 10 The algorithms and data applied in this work do not represent the "latest findings on BVOC emissions". Please rephrase.

p. 6566 l. 15: "The results of this study were comparable with the results of previous studies, hence our results were assumed to be reasonable": this doesn't make sense, especially since you emphasized the differences with previous studies. In essence, your estimates lie in the range of previous studies, that's all. You should also compare your results with a previous MEGAN-based inventory, such as Guenther et al. 2006 (grided emissions can be downloaded from the internet at the GEIA Emission portal http://accent.aero.jussieu.fr/database_table_inventories.php).

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p. 6566 l. 26: Tie et al (2006) did account for LAI effects.

p. 6567 l. 21-26 Please drop that paragraph which doesn't bring anything new. The comparison with previous studies is very poor. The authors should try to explain at least the sign of the differences with past studies. Also comparison with MEGAN-based inventories are needed.

p. 6568 l. 11 "Guenther's algorithms represent the most advanced approach for reliable determinations of BVOC emission...": there are other models such as LPJ-GUESS (e.g. Arneth et al., 2007) which relies on a process-based isoprene production algorithm. I would rather say that MEGAN is probably the most advanced *empirical* approach for determining BVOC emissions.

p. 6569 1st sentence: the PCEEA does not *neglect* the extinction of radiation as a function of LAI etc. It is just a parameterization based on the full MEGAN model, which does take those effects into account. Please rephrase.

p. 6569 last paragraph: The discussion on the possible influence of CO₂ is irrelevant. CO₂ is important for future projections, but is not a source of uncertainty for present-day emissions, which is the topic of this article.

Section 4.1-4.3 Those sections present only very general statements which resemble a recycling of similar general statements made in previous articles. I would have liked to see a more to-the-point discussion with a focus on Chinese emissions. If the authors cannot provide a more quantitative analysis of their uncertainties, they should drop that section. There are many BVOC flux measurements available in the literature for China (e.g. Baker et al. 2005; Klinger et al., 2002, probably much more in recent years); the Chinese BVOC inventory should be confronted with at least some of those measurements, even though they are not of the same year (interannual variability is lower than expected error).

P. 6570 l. 7 "Errors from misidentification are likely quite low": Uncertainties related

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to the distribution of PFTs might be high, as illustrated by e.g. the large differences displayed in Fig. 2 for China.

p. 6570 l. 10-20 Note that the standard emission factors are not directly measured. Flux measurements in various conditions are used to derive standard emission rates, assuming that the emission model is valid, which is often not the case.

Technical corrections

Abstract l. 7 inconsistencies → inconsistencies

p. 6556 l. 17 "radiations" → radiation

p. 6557 l. 15 what is meant by "By far"? Please clarify.

References

Arneth, A. et al.: Process-based estimates of terrestrial ecosystem isoprene emissions: incorporating the effects of a direct CO₂-isoprene interaction, *Atmos. Chem. Phys.*, 7, 31–53, 2007.

Baker, B. et al.: Wet and dry season ecosystem level fluxes of isoprene and monoterpenes from a southeast Asian secondary forest and rubber tree plantation, *Atmos. Environ.*, 39, 381–390, 2005.

Guenther, A. et al.: Estimates of global terrestrial isoprene emissions using MEGAN (Model for Emissions of Gases and Aerosols from Nature), *Atmos. Chem. Phys.*, 6, 3181–3210, 2006.

Klinger, L. et al.: Assessment of volatile compound emissions from ecosystems of China. *J. Geophys. Res.*, 107 (D21), 4603–4624, 2002. Müller, J.-F. et al.: Global isoprene emissions estimated using MEGAN, ECMWF analyses and a detailed canopy environmental model, *Atmos. Chem. Phys.*, 8, 1329–1341, 2008.

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