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Atmospheric Chemistry and Physics Discussions

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.

M. Li et al.

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Reviewer 2:

General Comments:

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 The article boasts that past studies were based on outdated algorithms and too coarse meteorology.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. Guen- ther et al. 2006 and others, so the novelty here lies in the combination of MEGAN and high resolution;

Response: Both the meteorological model and biogenic VOCs emission model (MEGAN) are important in estimating VOCs emissions. The land use and land cover in China have been changed a lot during the recent 30 years. However, the meteorological outputs from MM5 were often derived by using the USGS land surface data, which could be outdated. Another weakness is that the MEGAN model could use the MODIS data, which does not match USGS data default in MM5. In this study, both the MM5 and MEGAN are improved by using the real time MODIS land surface data, including the land use land cover, vegetation fraction, leaf area index etc. The results are compared with the past estimations and the VOCs fluxes measurements in China.

There is obvious error of land-surface data in the BVOC estimation models dynamic and meteorological models applied in past studies, e.g., the use of USGS land-use data in Tie et al. (2006) led to underestimation (for detail see section 3.7.1).

Moreover, High-resolved input data, e.g., hourly meteorological outputs from MM5 and more accurate 8-day average LAI data with improved measurement algorithms, representative of the year 2006 (for detail, see Line 158-167).

(2) the paper doesn't assess the impact of the claimed improvements, so that we are left clueless as to the their real importance;

Response: Accepted. We tried to improve the estimated results through the update of algorithms and input data (meteorological data and LAI), but because of limitations of data we did not conduct enough scenario analyses to show our improvements and just provide several reported studies (de Foy et al., 2006; Garrigues et al., 2008; Warneke et al., 2010) to assist our opinions. According to your suggestions, we also conduct statistical analyses of meteorological input data (section 3.1) and several case studies to show the factors influencing BVOC estimations through inter-comparisons (section 3.7.1).

(3) the article does not provide any evaluation against e.g. ïňĄeld measurements or HCHO satellite retrievals;

Response: I'm sorry to say that because of the limitations of measurements techniques and other reasons, there were few intensive measurement results that can be used for comparisons and evaluations. So in section 3.7.2, we just conduct a comparison with limited field measurements results (Bai et al., 2004; Baker et al., 2005; Geron et al., 2006; Gao et al., 2011). For detail, see section 3.7.2.

(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 ïňĄelds from the MM5 model compare with meteorological analyses e.g. from NCEP, ECMWF; and

Response:

(1) The Four Dimensional Data Assimilation (FDDA) scheme coupled with NCEP/FNL data was included in MM5 meteorological simulation to refine model performance.

(2) In section 3.1, we also conduct performance statistical analyses and spatial-temporal analyses of 2 m temperature and solar shortwave radiation with hourly observed data of surface temperature of 378 sites downloaded from the National Climatic Data Center (NCDC) and daily total radiation data of 89 sites downloaded from the China Meteorological Data Sharing Service System. The evaluation results show that our simulated meteorological data were desirable compared with several past studies (Hanna et al., 2010; Wang et al., 2011; Zhang et al., 2011).

(5) the isoprene algorithm used in this article is a simpliïňĄed version (PCEEA) of the MEGAN model, which might differ from the "full" MEGAN model by 25% or more at speciïňĄc times and locations (Guenther et al., 2006). Also the soil moisture stress effect is ignored, with unknown consequences.

Response: Accepted.

I think that the text should be adapted in order to reïňĆect those various limitations. Sensitivity calculations (e.g. with alternative meteorological ïňĄelds) should be performed to evaluate some of the possible sources of uncertainty.

Response: Accepted. In section 3.1, we conduct detailed analyses to evaluate the model performance and uncertainties of simulated meteorological data. In section 3.6, in the discussion of inter-comparisons with past studies, we conducted several case studies to show the quantified influence of input data and outdated algorithms on BVOC emissions.

Comparisons with (1) at least some BVOC ïňĆux measurements, and (2) with other MEGAN-based inventories should be provided.

Response: Accepted.

In section 3.7.1, we gave more thorough analyses of comparisons with MEGAN-based results (Guenther et al., 1995; Klinger et al., 2002; Guenther et al., 2006; Tie et al., 2006) and estimated results by other approaches, e.g. GUESS-ES emissions based on process-based approach (Arneth et al., 2007; Schurgers et al., 2009). Additionally, we also conduct several case studies to explore the reasons responsible for the differences between the recent study by Tie et al. (2006) and ours. In section 3.7.2, we also conduct a comparison with limited field measurements results (Bai et al., 2004; Baker et al., 2005; Geron et al., 2006; Gao et al., 2011) as suggested by reviewer 2. For detail, see section 3.7.

Minor corrections:

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

Response: Accepted. For detail see Line 12-13.

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

Response: Accepted. It means “dynamic meteorological models, i.e. MM5 in this study”. We have rephrased it and for detail see Line 15.

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

Response: Accepted. In section 3.7.1 we conducted several case studies to show the possible sources of uncertainties of past studies. And again we also estimate the isoprene emission with MM5 based on default USGS land surface parameters and it showed a slight underestimation of 0.5 Tg C yr-1 (the result is not given in the paper) compared with this study.

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

Response: Accepted. It means the value estimated by our study fall in the range of past studies. We have rephrased it and gave more quantitative results. For detail see Line 24-28.

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

Response: Accepted.

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

Response:

(1) We mean that for many studies conducted in China on the national scale, they simplified the quantification of the influence of LAI and leaf age. For example, in the study by Tie et al., (2006), the influence of LAI is just reflect in the seasonal change of EFs in summer and winter but they did not give quantified influence of LAI.

(2) In MEGAN, by default it is based on monthly average LAI for the year 2003 and based on an early version of the MODIS satellite LAI data product. Therefore, in this study we used more accurate 8-day average LAI data representative of the year 2006 instead. The LAI data used in our study have three advantages: a. Since LAI distribution can change dramatically year by year, it can be representative of the specific year

2006; b. high spatial-temporal resolution; c. revised retrieval algorithms and measurements of MODIS. For detail, see Line 138-147.

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

Response: Accepted.

(1) The Four Dimensional Data Assimilation (FDDA) scheme coupled with NCEP/FNL data was included in MM5 meteorological simulation to refine model performance.

(2) In section 3.1, we also conduct performance statistical analyses and spatial-temporal analyses of 2 m temperature and solar shortwave radiation with hourly observed data of surface temperature of 378 sites downloaded from the National Climatic Data Center (NCDC) and daily total radiation data of 89 sites downloaded from the China Meteorological Data Sharing Service System. The evaluation results show that our simulated meteorological data were preferable compared with several past studies (Hanna et al., 2010; Wang et al., 2011; Zhang et al., 2011).

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

Response: MODIS land-use data (MCD12Q1) for the year 2006 and water mask data (MOD44W) for the year 2000, both with a resolution of 500 m, were used to calculate the coverage fraction of each vegetation in each grid cell. Fig. 1 below compared the coverage fraction of each vegetation type for MODIS and MEGAN. From the comparison, it can be seen that on the whole the distribution of coverage fractions for each vegetation type of MODIS data agreed well with MEGAN. Additionally, the plant distribution of MEGAN was highly consistent with the plant distribution map (Fig. 3c in the manuscript) provided by the Plant Research Institute of the Chinese Academy of Sciences. Specifically,

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(1) The coverage fractions of broadleaf forest in northeast China and southwest China were similar for MODIS and MEGAN data (Fig. 1a). But MODIS gave higher coverage fractions of broadleaf forests in the southeast of China.

(2) The coverage fractions of coniferous forests in south China identified by MEGAN was much higher than the values given by MODIS (Fig. 1b). It is because of the limitations of satellite sounding that MODIS failed to distinguish between mixed forest and coniferous forests, and MODIS identified large areas of south China as mixed forests (Fig. 1f).

(3) For grass, the distributions for the two datasets were similar but the MODIS data showed a high value center in Yunnan Province, which is because we also classify savannas and woody savannas into grass.

(4) For crop, the distributions were similar and concentrated in the north and center of China, but the fraction of MODIS was approximately 10% higher than MEGAN.

(5) For shrub, MODIS underestimate the distribution of shrub in south China.

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.

Response: We assumed that the two sets of land-use data were representative of the year of measurement. The USGS data derived from measurements of AVHRR satellite is representative of land-use distribution in China in the 1990's, while the MODIS data reflect land-use distribution in 2006 and the validation was verified by the plant distribution map provided by the Plant Research Institute of the Chinese Academy of Sciences (Fig. 2). In the study by Tie et al., (2006) the estimation of BVOC emissions was based on USGS land-use data and resulted in great underestimations of BVOC emissions.

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.

Response: The data of standard EFs used in our study differ from that used in Guenther et al., (2006). The data in our study were released in August 2007 and was the new version 2.1 of the MEGAN emission factors, but the data in Guenther et al., (2006) is version 2.0. The hotspot of EFs is related with the distribution of evergreen broadleaf forests in Southeastern Tibet. The differences are primarily attributed to introduction of more recent field measurements but there may also be some differences due to PFTs (Alex Guenther, private correspondence, 2006).

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.

Response: Accepted.

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

Response: Accepted. We have deleted it.

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).

Response: Accepted. For detail, see section 3.7.

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

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Response: Yes, but in the study by Tie et al. (2006) they did not give quantified influence of LAI. Tie et al., (2006) just reflect the influence of LAI in the seasonal change of EFs and for some vegetation types, the EFs in winter were half that in summer.

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.

Response: Accepted. we gave more thorough analyses of comparisons with MEGAN-based results (Guenther et al., 1995; Klinger et al., 2002; Guenther et al., 2006; Tie et al., 2006) and estimated results by other approaches, e.g. GUESS-ES emissions based on process-based approach (Arneth et al., 2007; Schurgers et al., 2009). Additionally, we also conduct several case studies to explore the reasons responsible for the differences between the recent study by Tie et al. (2006) and ours. For detail, see section 3.7.

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.

Response: Accepted.

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.

Response: Accepted.

p. 6569 last paragraph: The discussion on the possible inïňĆuence of CO_2 is irrelevant. CO_2 is important for future projections, but is not a source of uncertainty for present-

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day emissions, which is the topic of this article.

Response: Accepted.

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 ïňĆux 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).

Response: Accepted. We have deleted section 4 because we failed to give quantitative analyses of uncertainty.

I'm sorry to say that because of the limitations of measurements techniques and other reasons, there were few intensive measurement results that can be used for comparisons and evaluations. So in section 3.7.2, we just conduct a comparison with limited field measurements results (Bai et al., 2004; Baker et al., 2005; Geron et al., 2006; Gao et al., 2011). For detail, see section 3.7.2.

P. 6570 l. 7 "Errors from misidentiïňĄcation are likely quite low": Uncertainties related to the distribution of PFTs might be high, as illustrated by e.g. the large differences displayed in Fig. 2 for China.

Response: Accepted.

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.

Response: Accepted.

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Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C4922/2012/acpd-12-C4922-2012-supplement.pdf

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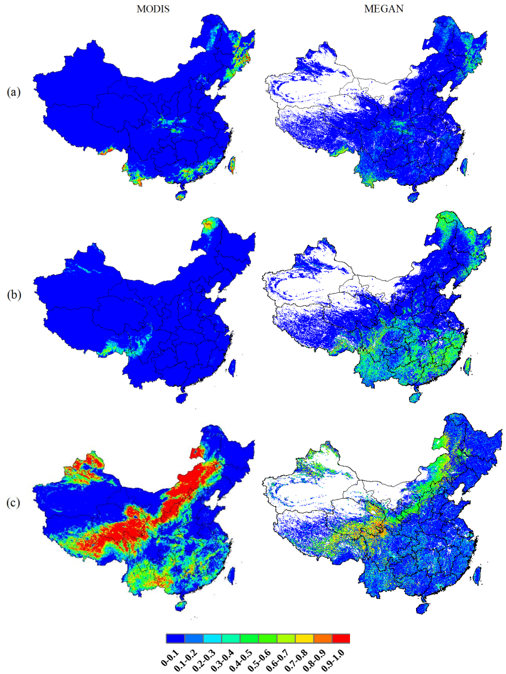

Fig. 1 The coverage fraction of each vegetation type for MODIS (left) based on 12 km ×12 km grid and MEGAN (right) based on 1 km × 1 km grid. (a) Broadleaf forest; (b) Coniferous forest; (c) Crop; (d) Grass; (e) Shrub; (f) Mixed forest.

Fig. 1.

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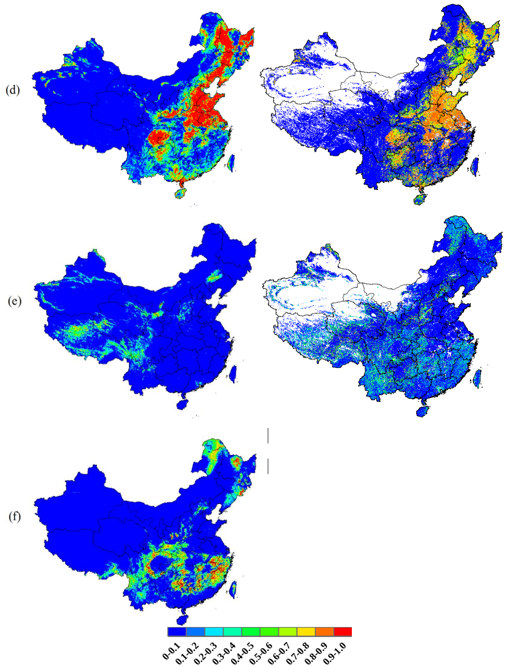

Fig. 1 The coverage fraction of each vegetation type for MODIS (left) based on 12 km ×12 km grid and MEGAN (right) based on 1 km × 1 km grid. (a) Broadleaf forest; (b) Coniferous forest; (c) Crop; (d) Grass; (e) Shrub; (f) Mixed forest.

Fig. 2.

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