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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.

Anonymous Referee #1

Received and published: 12 April 2012

General Comments:

This manuscript presents a set of BVOC emissions estimates for China using the MEGAN model [Guenther et al., 2006] driven using high resolution meteorological and satellite data.

Overall, the paper is generally well-written with nice clean figures, and several useful tables. I guess for those studying and/or interested in China's BVOC emissions it may be a useful future paper to refer to.

That said, I'm not sure what new science (if any) is actually being presented. All the authors have really done is to run an established model (MEGAN) with a single prescribed

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set of high resolution MM5 and MODIS data. Other different high resolution meteorological or vegetation inputs have not been considered. It's hardly ground breaking work.

In addition, throughout the text there are statements that are slightly misleading, and in some instances plain wrong. I therefore echo the sentiments of the other reviewer in that for the manuscript to be published, careful rephrasing and corrections are needed (cf. the detailed comments of anonymous reviewer 2).

Specific comments:

Page 6553, Line 5: "Natural NMVOCs equal or exceed anthropogenic VOC emissions. . ." – which is it?

Page 6553, Line 15: Based on a wide range of studies, I think it is safe to say isoprene and monoterpenes are the most dominate BVOC emissions.

Page 6554, Line 2: What are the 'significant implications'? Please explain to the reader what these might be.

Page 6554, Line 20-21: The dependence of monterpene emissions on light has only been included in leading BVOC emissions models in the last few years. How can some of the early studies be deficient for only incorporating what was known at that time?

Page 6555, Line 5: Most BVOC emissions provide input to chemistry transport models (CTMs). A resolution of 0.5x0.5 degrees is not considered coarse for a CTM. The resolution one calculates the emissions at, is or course dependent on their application. Hence poor resolution input data is not a deficiency – it's just a constraint.

Section 2.2: I'm left unclear about the leaf area index (LAI) being used to derive MEGAN – where does it come from? The authors say they use the vegetation fraction (VGF) derived from MODIS NDVI but this is not the same as LAI. The vegetation fraction is the amount of each grid cell covered by vegetation; the LAI is the leaf size of that vegetation. Furthermore, the LAI in MEGAN should be weighted by the fraction of

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each grid cell covered by vegetation (as done in Guenther et al, 2006). Please provide more explanation on your approach.

Section 3.4: This section seems long-winded and over-elaborate. In my opinion, it could be shortened to a one/two paragraphs.

Section 3.5: A figure of the anthropogenic emissions would be useful.

Sections 3.6 and 4.1: Address the well-made comments made by reviewer 2.

Page 6569, Line 15: In the full MEGAN canopy model, on which the PCEEA algorithm is based, the humidity and wind speed are taken into account.

Section 4.2: The assignment of basal emissions capacities, whether it be in a canopy-scale model like MEGAN or a leaf scale algorithm like LPJ-GUESS (Arneth et al., 2007), are known to be a large uncertainty. You're not saying anything new.

Page 6571, Line 15-16: You've only used a different set of input data (which have uncertainties), not made 'obvious improvements'.

Page 6573, Line 9: Small typo.

Reference: Arneth, A., et al. (2007), Process-based estimates of terrestrial ecosystem isoprene emissions: incorporating the effects of a direct CO2-isoprene interaction, Atmos. Chem. Phys., 7(1), 31-53, doi:10.5194/acp-7-31-2007.

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