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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Global dataset of biogenic VOC emissions calculated by the MEGAN model over the last 30 years" *by* K. Sindelarova et al.

Anonymous Referee #2

Received and published: 25 May 2014

General comments:

The authors present the results of model simulations performed to produce a dataset of biogenic emissions for the 30-year period of 1980-2009. Sensitivity studies are conducted to attempt to quantify the effect of altering model driving data or parameters on modelled isoprene emissions estimates. The authors compare their inventory with global and regional emissions estimates generated in previous studies, and evaluate their modelled data with flux measurements made during field campaigns in the Amazon and Borneo. Their results indicate the relative importance of certain geographical regions, modelling assumptions and driving data to estimates of biogenic emissions

derived from MEGANv2.1.

The data generated by this study are available to the community via the Emission of atmospheric Compounds and Compilation of Ancillary Data (ECCAD) website (http://www.pole-ether.fr/eccad). While such an inventory is welcome, and in many ways long overdue, it would be good to see how the authors envisage it could be used. Do they intend for it to be used in place of the GEIA emissions for use with models that do not generate biogenic emissions on-line, or for evaluation of model output? Furthermore, is it the intention of the authors to extend this study to produce emissions inventories for future scenarios?

While the methodology of the study is rigorous and relatively comprehensive, their analysis of it would benefit from further discussion of the sources of discrepancies between their results and those of previous measurement campaigns and model studies, and the uncertainties involved in the parameterisations within the model. Having conducted such in-depth comparison and evaluation, the authors are well placed to make recommendations for the best targets of future research (both experimental and modelling) for the biogenic emissions community to narrow uncertainties and constrain emissions estimates.

I recommend this article be accepted for publication in ACP subject to the authors satisfactorily addressing the comments and concerns outlined below.

Major concerns:

- In absence of available data the authors have used an average LAI derived from MODIS data for 2000-2009 for all other years (i.e. from 1980-1999 and 2010). There is no evidence that they have applied any weighting or scaling to these average data, and yet climate conditions varied markedly during the 1990s in particular which would be expected to affect GPP and hence leaf density. At the very least, the authors should conduct a further sensitivity study alongside S1 to indicate the potential error the use of average LAI may introduce to their inventory.

Minor concerns:

- Why stop at 2010? Is it the intention of MACC/CCMI to extend this dataset to include future projected emissions estimates for biogenic compounds?

- Why have the authors chosen to compare their estimates against those of previous datasets for the year 2007 when one of the previous studies only has estimates available for 2003? Surely it would make far more sense to evaluate data from the same year for all studies? As the climatology will be different for the different years, it would be expected that, particularly, on a regional basis, emissions would likely be very different.

- While the presentation and quantitative discussion of the results of this study are rigorous and detailed, they lack any real qualitative discussion and conclusions. In particular, I would like to see more attribution of the discrepancies between datasets (both modelled and measured). Why, for example, should there be large differences between MEGAN-MACC estimates and regional studies for Europe (which has also been demonstrated in previous work)? Why do different LAI datasets produce such different emissions estimates in Australia? Why do modelled Amazon fluxes differ in absolute value from measurements when they capture the seasonality so well? And on p10751, the authors write “other factors than meteorology are likely to play an important role in driving the emissions”; these other factors should be outlined here.

- In their conclusions, the authors assert that comparisons of their modelled emissions against measured fluxes show good agreement. This hardly seems the case for Borneo, where the authors appear to have (rather arbitrarily) scaled their emissions estimates by a factor of 1.7 to bring them in line with measured fluxes (see the further comment below regarding Fig.16); the authors themselves go on to highlight this discrepancy (rather inconsistently with their assertion of good agreement).

- Please consider including a comparison with Barkley et al.’s more recent Amazon emissions estimates, as per the Short Comment.

- On p107521, the authors discuss the comparison of modelled and measured night-time monoterpene emissions. Their results appear to suggest that South American monoterpene emissions are light and temperature controlled, and if that is the case, can they recommend an appropriate light-dependent factor to be applied to monoterpene emissions calculations for S America?

- Finally, the authors should make specific recommendations of where further research (modelling and experimental) would be of most benefit in constraining emissions estimates.

Technical corrections:

p10728, L7-11 – please make it clear that their impact on the atmosphere has been identified by MODELLING studies;

p10728, L11-13 – tropospheric ozone also has climate impacts via radiative forcing;

p10728, L23 – please give the version number of MEGAN used (i.e. v2.1);

p10729, L2 - please state the units of the flux, F;

p10729, L11 – replace “on isoprene and only temperature” with “on isoprene emission rates but only the temperature”;

p10729, L28 – replace “development” with “developmental”;

p10730, L7-9 – please include the equation that Sakulyanontvittaya et al introduced to better explain the inclusion of the light dependent factor;

p10730, L16 – replace “compounds” with “compound groups”;

p10732, L8 – I would suggest explaining why the CO2 factor is 1 for other compounds. Perhaps something along the lines of “In view of the lack of clear experimental evidence of an effect, gammaCO2 is set to 1 for all other species.”;

p10732, L10 – insert “a” between “for” and “canopy”;

p10732, L11-16 – this sentence does not make grammatical sense. Perhaps the authors could replace “Additionally” with “Further standard canopy conditions include” which would be slightly better;

p10733, L3 – please insert “the” between covers and modern;

p10734, L25 – replace “potential” with “potentials”;

p10735, 3&7 – I assume that by “8 daily” the authors mean 8 days rather than 8 times per day, in which case “8 daily” should be replaced with “8-day”;

p10735, L17 – replace “using a difference” with “based on changes”, and refer Guenther et al, 2012 here;

p10735, L20 – insert “the” between “to” and “spatial”;

p10736, L21 – insert “The” before “dominance” and “the” between “of” and “south”;

p10737, L7 – add “of mean annual monoterpene emissions.” at the end of the sentence;

p10737, L10 – insert “The” before “graphs”;

p10737, L11 – insert “,” after “species” and “the” between “and” and “higher”;

p10738, L4 – insert “the” between “and” and “species”;

p10739, L2-3 – insert () around “described in . . .. . . Eq. (3); insert “and” between “2.2.2” and “calculated”; remove the “the” before “Eq.”;

p10739, L9 – move “,” from after “Amazon” to after “)”;

p10740, L1 – Heald et al would be a better reference here as Heald used the same (Wilkinson) parameterization whereas Arneth used the Possell algorithms;

p10740, L10 – replace “equals to” with “is”;

p10741, L5 – move “gammaSM” to after “factor”;

p10741, L8 – replace “limit” with “limiting”;

p10741, L12 & p10742, L19-20 – Why do sensitivity studies S4 and S5 also have acronyms (MEGAN-MACC-SM and MEGAN-MACC-SW) when none of the other sensitivity studies do? p10743, L9 – remove “in global total” as this has already been made clear;

p10745, L11 – replace “at” with “in”;

p10747, L16 – insert “the” before “Northern Hemisphere’s”;

p10747, L28 – replace “update of” with “updating”;

p10748, L2-3 – replace “Difference” with “Differences” and “originates” with “originate”;

p10748, L27 – this should be a single sentence: “. . .regional totals, except for. . .”;

p10749, L22 – “fluxes” should read “flux”;

p10752, L4 – insert “the” before “dry season”;

p10752, L5 – insert “the” before “wet season”;

p10752, throughout – “during end of dry (wet) season” should read “during the end of the dry (wet) season”;

p10752, L16&17 – replace “in the end” with “at the end”;

p10752, L25 – insert “the” between “both” and “wet”;

p10752, L28 – insert “the” between “during” and “dry”;

p10753, L19 – “accual” should read “actual”;

p10753, L22 – replace “can be” with “is”;

p10753, L23 – “malaysian” should read “Malaysian”;

p10754, L1 – replace “can” with “could”;

p10754, L6 – remove “being emitted”;

p10754, L14 – replace “equals to” with “is”;

Table 3 – this table would be far easier to read and understand if the data of previous studies and this study were presented in 2 separate columns rather than being separated by “|”;

Fig. 5 – please consider giving some indication of the total emissions for each month on the graph as these fluctuate, either with a third bar or using a line graph;

Fig. 16 – please justify the apparently arbitrary scaling factor applied to modelled emissions of 1.7 – the emission potential used in the model is 7 while the emission potential estimated from measurements was 1.6, which would suggest a much higher scaling factor should be applied.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 10725, 2014.