Atmos. Chem. Phys. Discuss., 14, C2777–C2780, 2014
www.atmos-chem-phys-discuss.net/14/C2777/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Improved model of isoprene emissions in Africa using OMI satellite observations of formaldehyde: implications for oxidants and particulate matter" *by* E. A. Marais et al.

E. A. Marais et al.

emarais@fas.harvard.edu

Received and published: 27 May 2014

We thank the anonymous referee for suggested improvements to our manuscript. Responses to comments are below. Reviewer comments are in italics.

The authors investigate the distribution and magnitude of isoprene emissions over Africa derived from HCHO OMI column observations. These emissions are further scaled to provide corrections to the basal emission rates for forest and savanna ecosystems. The results are found to be consistent with field and aircraft campaign measure-

ments. The conclusions seem to be supported by the analysis. This study is interesting and well fitted to the scope of Atmos. Chem. Phys.. However, some points are unclear and need additional clarification. The publication is recommended provided that the following concerns are adequately addressed in a revised version.

General comments

• *Section 4 deals with the seasonality of OMI-derived emissions over 2005-2009. This is fine, but it is recommended to include a section on the interannual variability of the emissions over the five years, especially since the OMI-derived emissions are compared with REA measurements from different years. In addition, a comparison between the interannual variability of MEGAN, in response to the changes in meteorology, LAI, etc. and the top-down variability is necessary.*
We examined interannual variability and found it to be small (p. 6959, lines 24-26).

• *In Section 3 the authors provide comparisons with canopy flux measurements. However, too few elements for these campaigns are provided in the text. More details are needed for the specifics of each campaign, e.g. a table with the time of year, location, and the exact value of the measurement.*
Thank you for your suggestion. We now include details of the measurement campaigns in Tables 1a and 1b.

• *In Section 3 a general conclusion from the comparison is a strong overestimation of MEGAN compared to both OMI and (even more) field (tower and aircraft) measurements. However, those measurements were available since 2001 or earlier. Why were those measurements apparently not considered in MEGAN?*
These measurements were considered in deriving the MEGAN emission factors, but, as stated in the text, the flux tower sampled vegetation with a low fraction of isoprene emitters, while neighbouring trees have high isoprene emission factors. The distribution of these high emitters is unknown, leading to uncertainties in the MEGAN emission factors beyond the tower footprint (p. 6958, lines 13-20).

• *How can one rule out the possibility that the method applied for removing fire pixels, does not also remove pixels with high isoprene fluxes, and leads therefore to underestimated OMI-derived isoprene fluxes?*
The opposite is the case – filtering for biomass burning removes low isoprene emission scenes. In Marais et al. (2012), the comparison of OMI and MEGAN isoprene emissions is for coincident gridsquare-months. We now refer readers to Marais et al. (2012) for further details.

Specific comments

• *p.6955, l.2 : Please specify the value of C_{CE} used here.*
The normalization factor (C_{CE}) used in GEOS-Chem is 1.3. We now provide this value in the text.

• *p.6956, l.20 : "The sensitivity S of column HCHO to a perturbation Δ in isoprene emission.." : is the perturbation applied to 12-15 LT isoprene emission or to the daily averaged value?*
Addressed.

• *p.6956, l.21 : "Values of S are sensitive to NO_x concentrations and this was accounted for using concurrent observations of OMI tropospheric NO_2 columns": it is not clear how this is realized. Is a threshold used for specifying low-NO_x condition?*
We now elaborate on the method used to identify NO_x-dependent yields of HCHO from isoprene emissions (S).

• *p.6957, l.10 : "...the use of OMI NO_2 to obtain S under low-NO_x conditions" : please specify the criterion used for low-NO_x conditions.*
The low-NO_x threshold of 500 pptv boundary-layer NO_x is now included in the text.

• *p.6959, l.4-10 : Has the soil moisture activity factor been taken into account in MEGAN? If not, it could explain part of the difference in the comparisons between MEGAN and flux measurements shown in Figure 2.*
The soil moisture activity factor is included in our MEGAN emission inventory. It leads

to a 15 % reduction in annual average isoprene emissions over Africa (Section 3.3 of Marais et al., 2012).

• *p. 6961, l.3 : "...with temperature and the LAI as the principal drivers" : the argument here is simplified because the seasonal variability is also driven by solar radiation and soil moisture stress. Please elaborate.*
Addressed in Section 4.

• *p.6961, l.12 : "We can infer them from the OMI-derived..." : do you mean scale? Please clarify.*
We now explicitly state the variables we use in Eq. (1) to obtain new emission factors, so that it is clear that our OMI-derived emission factors are inferred.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 6951, 2014.