

RESPONSE TO REVIEWERS

Manuscript: *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.**

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

Responses to Reviewer #1:

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 measurements. 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 NO₂ 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 NO₂ 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.

Responses to Reviewer #2:

Marais et al. present an analysis of new isoprene emissions over Africa derived from OMI formaldehyde observations in comparison with the MEGAN inventory. They explore the factors (temperature, leaf area index) which control the seasonal and spatial variability of the African emissions. They show that emission factors tend to be overestimate in MEGAN inventory especially over equatorial forests. The results are validated using direct leaf measurements from field campaigns taken from literature and using isoprene measured during the AMMA aircraft campaign. The total emission of isoprene in Africa is then estimated and the impact on surface ozone and particular matter quantified. The paper is well written in a concise and clear manner. This work is suitable for ACP publication and I recommend it after the following comments are addressed.

General comment on the evaluation with canopy flux measurements

The OMI-derived and MEGAN isoprene emissions are compared to flux measurements reported in literature and corresponding to years outside the 2005-2009 period analyzed with OMI. Is the interannual variability of isoprene emissions sufficiently small compared to the errors to make this comparison valid? The authors should discuss this point.

Addressed.

On the other hand, most of the flux measurements have a small footprint (600m). Is the representativity of these measurements sufficient to be compared the emission derived from OMI (1x1 degree gridsquare average). The authors should address this point in more details in their discussion.

The small footprint of the flux towers (sites 1 and 4) is an issue in our comparison, as these towers were in a location with a low proportion of isoprene emitting species, while vegetation beyond the sampling footprint of these towers had a high proportion of isoprene emitters. We already acknowledge this and other shortcomings in Section 3.

Moreover, p 6958, lines 22-23, I do not understand the given argumentation for the observed discrepancies at site 2. I would expect that the fact that both flux measurements and OMI have similar footprint would improve the representativity of in situ measurement compared to satellite observation and then improved the comparison

The aircraft REA flux measurement at site 2 has a representative sampling footprint.

Unfortunately, REA flux measurements from aircraft are susceptible to a negative bias of

at least 25% due to vertical flux divergence between the altitude of the aircraft and the surface flux (p. 6958 lines 27-29, p. 6959 lines 1-3).

Specific comments

1) Page 6954, line 10: The reference Marais et al. 2012 should be added here

The Marais et al., 2012 reference has now been added.

2) Page 6957, lines 14-16: If I well understood, the errors detailed in the lines above are related to individual observations. The authors should precise and discuss the errors on the 1x1 degree gridsquare observations as well.

Assuming that the error for individual scenes reduces by $1/\sqrt{n}$, where n is the number of observations, the error in OMI-derived isoprene emissions of 1.4×10^{12} atoms C cm⁻² s⁻¹ (annual average for a gridsquare over the equatorial forests) would reduce to 2.9×10^{10} atoms C cm⁻² s⁻² (2%) at high NO_x and $2.9-6.6 \times 10^{10}$ atoms C cm⁻² s⁻¹ (2-5%) at low levels of NO_x. We would prefer to show in the text the error on individual OMI scenes. Our error estimate for monthly average gridsquares provided here is optimistic, as it assumes all error contributions are random.

3) Page 6964, lines 5-8: In order to evaluate the effect of isoprene emissions on surface concentrations of ozone and particulate matter, the authors compared GEOS-Chem simulations with and without the isoprene emission. I wonder if considering no isoprene emissions does not introduce a non-linearity in the chemistry and makes the simulations with and without isoprene emissions not really comparable by the end. Is the impact of isoprene emissions linear from 0 to the 77 Tg C a-1?

In Africa the effect of isoprene emissions is positive over regions with high levels of NO_x (northern and southern savannas), so that the impact of isoprene emissions on ozone is linear.

4) Figure 1: I would suggest the authors to add a table with all the references cited in the caption reported with the measurements conditions summarized.

Thank you for your suggestion. We now include Tables 1a and 1b.

References:

Marais, E. A., Jacob, D. J., Kurosu, T. P., Chance, K., Murphy, J. G., Reeves, C., Mills, G., Casadio, S., Millet, D. B., Barkley, M. P., Paulot, F., and Mao, J.: Isoprene emissions in Africa inferred from OMI observations of formaldehyde columns, Atmos. Chem. Phys., 12, 6219-6235, doi:10.5194/acp-12-6219-2012, 2012.