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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.

Anonymous Referee #1

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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.
- 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.
- 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?
- 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?

Specific comments

- p.6955, l.2 : Please specify the value of C_{CE} used here.
- 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?

- p.6956, l.21 : “Values of S are sensitive to NOx 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-NOx condition?
- p.6957, l.10 : “...the use of OMI NO_2 to obtain S under low-NOx conditions" : please specify the criterion used for low-NOx conditions.
- 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.
- 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.
- p.6961, l.12 : “We can infer them from the OMI-derived..." : do you mean scale? Please clarify.

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