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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Isoprene emissions in Africa inferred from OMI observations of formaldehyde columns" *by* E. A. Marais et al.

Anonymous Referee #1

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The manuscript by Marais et al. reports an improved method to derive biogenic isoprene emissions from satellite formaldehyde column retrievals. The new approach to reducing the impact of biomass burning and anthropogenic activity represents an important advance. A second contribution is that the manuscript discusses emissions over Africa, which has not received as much attention as other regions. A third and equally important aspect is that the paper discusses the influence that the uncertainty in low NOx isoprene oxidation chemistry has on the derived isoprene emissions. The paper is well written and clear and I recommend publication after addressing the comments below:

1. The discussion of the low-NOx chemistry is important and the authors clearly have paid a lot of attention to this. The manuscript mentions that Stone et al. report an

average of 70% high NOx pathway during AMMA and that AMMA was between high and low NOx conditions. However, I think it would be useful to be more quantitative overall, especially as the authors have clearly considered this aspect carefully. The manuscript would be strengthened if the authors state what fraction of peroxy radicals reacts with NO versus HO2 for the different conditions discussed. Otherwise it is hard to judge the claim that smearing results from low NOx conditions. It could be useful to add a map of the fraction of peroxy radicals reacting with HO2, and use this to compare the AMMA conditions with Central Africa in view of the smearing proposed to result from the latter region. I think this would be particularly useful given the large current interest in this topic.

2. The authors state that uncertainties in low-NOx isoprene chemistry could affect the model results and the other reviewer and short comment also raise this point. I concur with these statements and also believe this suggests weakening the last sentence of the abstract with respect to MEGAN. In the conclusion this is already discussed in a more balanced way.

3. This work, as well as related manuscripts, exclude conditions above a certain degree of cloud cover, for obvious and valid reasons. This selects specific conditions for deriving isoprene emissions in that cloudy days are not amenable for analysis. I realize that the authors cannot derive isoprene emissions for these days, but are there estimates of how big uncertainties in the isoprene emission inventories are for cloudy days compared to cloud free days. In other words, what fraction of the annual isoprene emissions in Africa is captured by cloud-free days and how big is the uncertainty in this, if it is known. I think it would be useful to have a brief comment on the impact of cloudy days on the isoprene budget in the manuscript.

Technical comments:

a) P. 7478 Line 22-23. Although it is fairly obvious it should be made clear that the typical HCHO lifetimes mentioned are noon-time, if I understand correctly. It could

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be helpful to put this directly in context with the nighttime HCHO transport and state whether/where transported HCHO could contribute to the observed smearing. From my reading of the paper this is not the case for biogenic areas, but a more direct statement would be helpful.

b) Related to the above point: What is the lifetime of the low NOx isoprene oxidation products in the model. This information also would be useful to compare with the smearing observed in the satellite retrievals and what causes this.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7475, 2012.