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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global emissions of non-methane hydrocarbons deduced from SCIAMACHY formaldehyde columns through 2003–2006" *by* T. Stavrakou et al.

Anonymous Referee #2

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Stavrakou et al. present a nice analysis of VOC emissions inferred from SCIAMACHY data. The paper is generally well-written. The study is appropriate to ACP and should be published once the following issues are addressed.

1- Very different emissions are obtained when you use a different chemical mechanism. The differences, in almost every case, are much larger than the prior-posterior differences that the paper is focused on interpreting (Fig 8). This implies to me that uncertainties in isoprene photo-oxidation provide a major hurdle to quantitative interpretation. I think a more prominent and detailed discussion of this issue is needed.

2- However, I applaud the authors for the sensitivity analyses shown in Fig 8, which are

more thoughtful than usually found.

3- I am also concerned about the fact that aerosols are not accounted for in the AMF. This seems to be a significant problem for interpreting biomass burning emissions! Please discuss how this effect will propagate to errors in your findings.

4- Going from HCHO columns from biomass burning VOC emissions requires important assumptions that are not mentioned here. Namely: 1) that the suite of VOCs emitted by fires is accurately reflected by the model EFs, and 2) that the model accurately captures the HCHO yield from that suite of VOCs, in the atypical photochemical environment of a fire plume. Please address the uncertainty in these assumptions and how they affect your results. For example, the Andreae and Merlet EFs are state-of-knowledge, but the reality will be more variable than a single set of numbers, depending on fire intensity, etc.

5- SCIAMACHY flies over at 10am and the authors apply a diurnal correction from the model. How good should we expect that correction factor to be?

6- what about the effects of your simplifying assumption for fire plume height – a single vertical distribution for each vegetation type?

In short, how do we know that the HCHO differences you're interpreting are actually due to emissions, and not to some combination of the above?

Lastly, a minor point – you find a small effect on HCHO over the Amazon from an increase in OH, and attribute it to the dominance of photolysis as HCHO sink. But there is also the fact that increasing OH increases both the source and sink of HCHO, right?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4609, 2009.