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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Secondary Organic Aerosol from biogenic VOCs over West Africa during AMMA" *by* G. Capes et al.

G. Capes et al.

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The authors would like to thank both referees for their constructive criticism and positive comments which contributed to a substantial improvement in the manuscript.

A specific response to each comment is given below (referee comments are given in italics, above each response)

Referee 2 comments:

-However there is one major error in the analysis, related to the choice of SOA yields used.
We are grateful to the reviewer for this correction. The estimates of SOA formation have been recalculated using the lower yield of 1% from (Kroll et al., 2006) and monoterpene yields extrapolated from (Griffin et al., 1999) as suggested. This uses the same ap-

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proach as the studies discussed in (Volkamer et al., 2006) and so a direct comparison between the studies has now been made. OM during AMMA appears to be under predicted by our revised yield calculations on a scale comparable to the under predictions in polluted mid latitudes. We have now revised the discussion section where we compare our findings with those in mid latitudes and highlight that whilst our measurements agree with global model estimates they are much higher than the estimates from our bulk yield predictions. Given that global model estimates of SOA are performed in a similar manner to our calculations this implies other sources of error exist in global models in this region - most likely in the emissions inventories. These findings now form the basis of the discussion section.

-I agree that the neglect of sesquiterpenes will contribute to an underestimation of SOA loadings; however this general issue - neglect of lower-volatility, high-yield SOA precursors - is not unique to biogenically-influenced areas.
This is now discussed at the beginning of section 4, and the omission of sesquiterpenes from our SOA prediction has been removed from the discussion for clarity.

- p.2534, line 26: the discussion of SOA underestimation should probably reference more papers to show it's a common model result. One of the papers that is cited [Jang et al., 2002] does not directly deal with model-measurement comparisons.
Several appropriate references have now been cited (Heald et al., 2005; Johnson et al., 2006; De Gouw et al., 2005)

- p. 2535, line 18: only 17 of 24 flights were included; it's never stated why 7 were omitted (I assume it's to remove the effects of biomass burning, etc., but this should be stated explicitly).
This is due to (lack of) availability of data from instruments and has now been explained in the introduction

- Equation 1: a short derivation might be helpful here. The key assumption is that the measured MVK and MACR gives the total amount of isoprene oxidized; but this only

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works for the first several (8) hours, before MVK and MACR begin reacting away also. If the air masses measured have spent a lot of time (many hours to days) over vegetated areas, this approach may underestimate integrated isoprene (and therefore monoterpene) emissions. On the other hand, if this air is relatively "young" (coming from the ocean), or the lifetimes of the particles (vs. vertical transport, for example) are short, then this method should be an accurate estimate of total isoprene (and monoterpenes) emitted and oxidized. A discussion of meteorology may help distinguish these.
The intermediate steps have now been included in the derivation of Eq.1. A discussion of the meteorology has also been included which suggests the air masses are likely to be of the order of a day old or less, due to the passing of mesoscale convective systems every few days, and smaller scale diurnal convection, mixing surface air aloft.

- p. 2543, line 9: I'm unaware of any chamber studies that show continuous growth after 24 hours (or even last that long!). In batch reactor studies of SOA formation from isoprene, aerosol growth appears to be over after 6-10 hours [Kroll et al., 2006; Dommen et al., 2006].
This line has now been corrected with appropriate references.

Referee 1 comments:

Choice of yields has been amended - see response to reviewer 2.

-A further concern regarding this manuscript is with the VOC measurements, and why only a subset of the measurements are used in the calculations.
A subset of the VOC data was used in the SOA calculations for consistency with the AMS measurements, which use a selection process to give a regional mass loading.

-One additional comment regarding the numbers used for the yield calculations: Shilling et al. 2008, showed low mass concentrations yields of a-Pinene from continous flow ozonolyis experiments to be 0.09 (organic loading of 0.15 ug/m3).
A range of SOA estimates have been calculated using the recent yield result from

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(Shilling et al., 2008) and is compared with the extrapolated yields from (Griffin et al., 1999) in the discussion.

-In particular the measurements of mono-terpenes were described in the text, but these measurements are apparently not used in the yield calculations...
The re-analysis now makes greater use of the monoterpenes data and so the sampling description has been left in the instrumentation section.

-At the very least the a-Pinene to Isoprene emission ratio could be checked for consistency between the plane data and the Saxton et al. 2007 data, since the authors state the lifetime is nearly identical, the ratio should remain constant. This comparison needs to be done for the revision, as the current analysis relies heavily on the emission ratios measured by Saxton et al.
This comparison between the ground and aircraft data has now been included, and though the ratios are significantly different, a discussion of the discrepancy is included, and a range of SOA estimates are provided using emission ratios from the aircraft data, Benin ground measurements, and from the literature.

-p 2536 line 19: Noise in the AMS measurments for 30 second averages is stated at 1.7 ug/m3 for organics, however in Figure 3, it looks like there is considerably more noise with some concentrations reaching nearly -10 ug/m3 for organic species. Has the noise calculation for this campaign been calculated, as the Crosier reference is for a separate campaign?
An estimate of the Q-AMS noise during AMMA has been calculated as 3.3μgm-3 and has been included in the text in place of the Crosier et al reference at the end of paragraph 2 in section 2.

-p 2539 line 4 and p 2543 line 20 and caption Table 2: The atmospheric lifetime of isoprene is given as less than one hour, then later as 2.1 hours. First which lifetime is correct, and second what were the conditions (OH, O3, etc) for which this was calculated.

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This has now been corrected (around 1.75 hours) and conditions detailed in the caption for table 2.

-p 2540 line 7: Do isoprene concentrations less than 100 ppt really indicate "highly aged regional air?" For a parcel of air with an initial concentration of 1000pptv, it will take approximately 2-4 hours (depending on lifetime, see previous comment) to reach 100 pptv assuming no further emission into that parcel.
The median isoprene concentration for the Ilow subset was 23ppt, and 480ppt for the Ihigh subset, marking a distinction between air that was dominated by recent BVOC emission and that which wasn't. The wording has been amended to better reflect this.

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