Atmos. Chem. Phys. Discuss., 10, C5422–C5425, 2010
www.atmos-chem-phys-discuss.net/10/C5422/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observations of OH and HO_2 radicals over West Africa" *by* R. Commane et al.

R. Commane et al.

rcommane@seas.harvard.edu

Received and published: 16 July 2010

Reviewer 2:

We thank the reviewer for their constructive and helpful comments and address them below. Each comment is shown in bold, with our reply directly below it. In light of these comments, Sections 4.1 to 4.4 will be restructured to best highlight HO_2 uptake in cloud. Sections 4.1 and 4.2 will be combined to become a new section 4.3, while the current Sections 4.3 and 4.4 will become the new Sections 4.1 and 4.2.

Section 3.3: This section compares OH with P(OH) and HO_2 with a simple steady state between P(OH) and HO_2 self reaction. My main concern here is that the conclusions being

drawn from these comparisons are less than compelling. First, the authors state that "OH mixing ratios were highly variable but were generally found to increase with increasing P(OH)." This is a highly subjective statement. A look at Figure 13 suggests that OH is only a handful of points away from having no correlation at all with P(OH). At a minimum, the authors should give the uncertainty in the slope and intercept values in both figures 13 and 14.

The statement will be amended to:
"OH mixing ratios were highly variable with only $\sim$ 10% of this variability explained by the P(OH)."
The uncertainty on the slope and intercept will be added to the caption.

The authors also discuss distinct groups of data in figures 13 and 14, but the only consistent behavior in any of the groups is for HO_2 in the presence of isoprene. Biomass burning behavior seems divided into two distinct behaviors for HO_2 and no clear behavior for OH. The authors conclude this section by stating that the more comprehensive analysis by Stone implies that HO_2 is "controlled by relatively simple processes." Based on the correlation coefficients, the simple analyses presented here account for only 10% of the variability in OH and one-third of the variability in HO_2. It would be nice to state how much more of the variance can be explained when Stone et al. employ a detailed chemical model.

The detailed chemical model employed by Stone et al was found to reproduce 55% of the variability in the observed HO_2, compared to 33% for the study here. The model relies on the availability of ancillary data (e.g. NO) so less data points were available for comparison than used in this study. However, due to large uncertainties and lack of coverage in the observations of OH and other key species (e.g. NO, photolysis rates), they could not confidently compare the model to OH.

Sections 4.1 and 4.2: There is not much of substance in these sections, which tend to generalize behaviors that are not consistently demonstrated in the data. For isoprene, the reader is given a little background and then reminded that the real work on this data is

ACPD
10, C5422–C5425, 2010

to found in the work of Stone et al. For biomass burning, the authors focus on VOCs as a HOx sink, however, depending on the VOC mixture and NOx level, oxidation can potentially lead to enhancements in HO_2. This might also help explain why the biomass burning data do not exhibit a consistent behavior in figure 14. In the end, there is no attempt to quantitatively explain the HO_2 discrepancies related to biomass burning. Does Stone et al. provide a deeper analysis of biomass burning chemistry?

The two distinct biomass burning episodes identified in Figure 14 are from July 22nd and August 8th. The data from August 8th (observed HO_2 < calculated HO_2) is examined in more detail as there are more observations of both HO_2 and other data available. There is no NO_X data available for the five data points identified as biomass burning on July 22nd (observed HO_2 > calculated HO_2). During the August 8th episode, NO mixing ratios vary from below 50 pptv outside the plume to 150 pptv within the plume. Stone et al does not provide a deeper analysis of biomass burning chemistry.

Section 4.3: This section provides the best case for having a paper that stands on its own. These observations are the most compelling observational evidence for cloud uptake of HO_2 that I have seen, and they deserve to be highlighted. However, I agree with the authors' concluding statement that we still don't have the observations required to fully explore the impact of clouds on oxidants. It would be very constructive if the authors not only called for "a more comprehensive field study" but also offered a brief list of the measurement suite that they would recommend to address this issue.

As suggested, this section will be moved forward in the discussion and a brief paragraph will also be added suggesting the measurements most useful to addressing HO_2 uptake by clouds.

Section 4.4: In this section, the authors discuss potential nocturnal HO_2 sources associated with ozonolysis of alkenes, specifically isoprene. While the discussion is mainly speculative, there is no indication that nighttime data over the forest behaved differently than over the ocean or Sahel regions. While the nighttime observations are a smaller subset of the data, some indication of where nighttime observations were collected would be useful.

All the night-time data were collected in the forested region so no comparison with Sahel or

ACPD
10, C5422–C5425, 2010

ocean data is possible here. Isoprene reached up to 1 ppbv in areas north of the most heavily forested region, with the highest mixing ratios coincident with the highest HO_2 mixing ratios. This night-time maximum in isoprene at latitudes north of the day-time maximum is consistent with transport of the airmass north with the monsoon winds.

Minor comments:

Page 7268: When discussing previous OH observations, measurements during the PEM-Tropics-A experiment (Mauldin et al., 1999) should be acknowledged. Reference: Mauldin, R. L. III, D. J. Tanner, and F. L. Eisele, Measurements of OH during PEM-Tropics A, J. Geophys. Res., 104, 5817-5827, 1999.

Reference was in Section 4.3 but will also be added to Section 1.

Page 7281, line 25: I would refrain from saying "compare well" since there is no way to formally compare the observations. Instead, I would say that the observations "fall within the expected range defined by previous measurements."

Correction will be made.

Page 7283, lines 3-5: Please provide some indication of abundances for NO and NO_X as you did for CO and O_3.

Abundances will be added.

Figure 8: This figure needs a color bar and scale.

Colorbar will be added

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7265, 2010.