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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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This manuscript reports on airborne observations of OH and HO2 using low-pressure laser-induced fluorescence during the AMMA campaign. A detailed description of the instrument is followed by a general overview of the observations which are interpreted through comparison with a set of simplified calculations. My primary concern is that the paper appears to serve largely as an advertisement for two more in-depth papers which promise to describe the instrument more completely and analyze the observations more comprehensively using a detailed chemical model. The most notable result is the variability in HO2 with cloud water based on data at high temporal resolution. To my knowledge, this constitutes the first clear observational evidence of strong uptake of HO2 in clouds. Unlike the other results in this paper, this finding does not rely on the overly simplistic interpretation based on P(OH) and HO2 self reaction, thus it represents a stand-alone result that should not require more in depth analysis in a follow-up

modelling paper. I would suggest that the authors focus on the uptake and leave the analysis of isoprene and biomass burning to Stone et al. However, I have provided specific comments below on all sections of the paper.

Specific comments:

Section 3.3: This section compares OH with P(OH) and HO2 with a simple steady state between P(OH) and HO2 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 authors also discuss distinct groups of data in figures 13 and 14, but the only consistent behavior in any of the groups is for HO2 in the presence of isoprene. Biomass burning behavior seems divided into two distinct behaviors for HO2 and no clear behavior for OH. The authors conclude this section by stating that the more comprehensive analysis by Stone implies that HO2 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 HO2. It would be nice to state how much more of the variance can be explained when Stone et al. employ a detailed chemical model.

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 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 HO2. 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 HO2 discrepancies related to biomass burning.

Does Stone et al. 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 HO2 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.

Section 4.4: In this section, the authors discuss potential nocturnal HO2 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.

Minor comments:

Page 7268: When discussing previous OH observations, measurements during the PEM-Tropics-A experiment (Mauldin et al., 1999) should be acknowledged.

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."

Page 7283, lines 3-5: Please provide some indication of abundances for NO and NOx as you did for CO and O3.

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

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.

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