The paper by Vaughan et al. describes OH and HO_2 measurements at Cape Verde Atmospheric Observatory during 3 short field campaigns in 2009 using the Leeds aircraft-FAGE instrument in ground configuration. The data analysis does not include model simulations (to be presented in a future paper). However, the paper includes a thorough statistical interpretation of the results and a comparison to other previous HOx measurements in tropical latitudes. It provides a systematic characterization of and approach to parameterize sensitivities in the oxidizing power of the MBL in the marine tropics for which so far only few data have been available. Overall, the paper is well written and in as much as possible - based on the available campaign data - analyzes the dependencies of OH and HO_2 on UV (JO^1D) and air mass climatology for different time scales up to one year. I recommend publication in ACP provided that the paper is further improved based on the following comments:

1. The authors briefly mention the recent paper by Fuchs et al. (2011) on p. 21440, line 20, which reported on interference effects in the LIF measurement of HO_2 in the presence of organic peroxy radicals. They then state that in their study these effects must have been negligible due to very low mixing ratios of alkenes measured at CVAO. However, with respect to Table 2 they list isoprene ratios of 0.4 pptv and 0.04 pptv for SOS1 and SOS2, respectively. I assume that the unit here is erroneous and should rather be ppbv ? (It would rather be a feat for itself to detect 0.04 pptv isoprene !). If this is true, then surely such interferences can not be neglected, at least not for SOS1 with 400 ± 800 pptv isoprene. A thorough discussion of the HO_2 measurements based on the Fuchs et al. results needs to be included, with an estimate of potential uncertainties propagating through the current data analysis.

2. p. 21444, line 14: It is reported here that tests were made on March 6 to confirm that the exhaust of NO did not contaminate sampling at the site. However, no evidence for this is shown. In Figure 5 data for NO are missing for March 6 (and other days).

3. Figure 1: Is this figure really helpful? In terms of the marine halogen cycle interacting with the HOx cycle there have been much more complex schemes published, e.g., by Carpenter (2003). In particular, it seems there is still a lot of speculation about certain pathways, and it is not quite obvious which reactions have been well established and which have not. For example, it has been suggested that HOX (X = halogen) is exchanged between gas and aerosol phase. This may have significant consequences for the chemistry in this region for which the authors conclude there are strong heterogeneous reactions on Saharan dust particles leading to HOx removal. I suggest to either extend the figure in light of this discussion or omit it.

4. Table 4: The values b and c for the unforced fits are shown, but not for the pre-factor a. Why not? These should also be included, with their uncertainties.

5. A few minor comments:

a) Several figures have too small fonts for the x,y axes labels. These are very hard to read.

b) Two references cited in the text are missing in the Reference section: Forster et al., 2007; Holland et al., 2003.

c) p.21440, line 15: With 10 m asl and 50 m distance from the coast of the CVAO station it needs to be mentioned whether there is an appreciable tidal cycle over the three seasons which may have affected the measurements. Some discussion concerning regional effects should be included (or cited from companion papers).