

Interactive
Comment

Interactive comment on “Seasonal observations of OH and HO₂ in the remote tropical marine boundary layer” by S. Vaughan et al.

Anonymous Referee #2

Received and published: 21 September 2011

This paper presents a seasonal study of OH and HO₂ in the Tropics. Observations of these species at these latitudes are still rare so this work brings a positive contribution to our knowledge of the composition of the atmosphere. I think the work presented in this paper is fit for ACP and recommend publication after the authors have addressed a few minor points.

GENERAL COMMENTS

The authors mention in Sec 2 the Fuchs et al (2011) study which suggested the possibility of interferences in the detection of HO₂ by LIF. However this is barely mentioned in the rest of the paper. It is especially important to address this question when comparing the measurements made at Cape Verde with other, older, measurements. Even if alkenes are too low at Cape Verde, that is not necessarily true for other datasets. In

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any case this should be mentioned and discussed in Sec 4.1 and 4.2. Could this be a reason for HO₂ concentrations not following the P(OH) trend (page 21450)?

Section 4.4 (Variance Analysis) requires some work. First of all some background should be provided. Simply referring to the Rohrer & Berresheim paper is not enough. It should be explained how the variances were calculated, what are the timebins and how they were defined, how was Eq 18 derived, what are the timescales etc. Besides, without explaining how to read Fig 12, the explanation on pages 21456-21457 is highly confusing. The analysis is very interesting, but very badly explained.

I have some reservations regarding this statistical analysis: do 33 days provide a large enough dataset to make the results statistically valid? Could this maybe be improved by including the Whalley et al (2010) dataset in this analysis? Besides, the point of the paper is to study the seasonality of HO_x levels. Therefore, should the different seasons analysed separately for the variance analysis? It is conceivable that the results might be different, especially since SOS1 is significantly different from SOS2 and SOS3. The authors should address these issues before final publication.

SPECIFIC AND TECHNICAL COMMENTS

The figures in the pdf file are quite 'heavy'. I had trouble loading the pdf and printing the figures. I suggest the authors use some sort of compression for the figures. It should be possible without sacrificing the quality. All figures should have a legend to make them more readable. Figs 4, 5, 6 should have more dates on the x-axis.

- page 21433, l. 13-14 and 21434, l. 8-9: these assumptions should be justified.
- is Eq 5 derived from Eq 3?
- page 21446: it is mentioned a significant change in RH during SOS3. Is this due to changes in meteorological conditions, maybe different origin of the air masses? please put RH also for the 1st-9th period.
- page 21448, l. 1: "values of R₂ were similar" please be quantitative - page 21448, l.

7: and HCHO?

- page 21449, l. 23-27: please use a different verb than "yield", it is confusing here. The fact that the HO₂/HO₂+RO₂ ratio inferred from alpha agrees with the PERCA and FAGE observations during SOS2 is very interesting. However the disagreement during SOS1 should be discussed and possible explanations given.

- page 21450, l. 1: this seems to contradict what stated on page 21448, l. 4. Are the authors referring only to OH here? - page 21450, l. 18: the contributions of CO and CH₄ to OH loss should be easy to calculate with the CVAO dataset

- page 21452, l. 13-15: Is this statement following from Eq 7? Was $j(\text{O1D})$ the same (or similar) during the two campaigns?

- page 21453, l. 26: state the slope(s) during this study

- page 21454, l. 21, 24: be consistent with units

- page 21455, l. 6-8: this sentence is not necessary unless it is relevant under the conditions encountered at Cape Verde. the authors state earlier that alkenes levels at CVAO were very low. Too low to generate OH by ozonolysis?

- table 2: why are SOS3 measurements not shown?

- tables 3, 4: why were OH and HO₂ averaged differently? if there is a reason for the 4 and 5 min averages it should be explained, otherwise they should both have the same average. why are the errors and R² shown only for the "unforced fits"? the "forced fits" were used for the analysis, so errors and R² are more relevant for the "forced fits". It would be better to show them for both, anyways.

- figure 2: should show measured NO and/or wind direction

- figure 11: can the Whalley et al (2010) data be shown in this figure?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 21429, 2011.

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