

## ***Interactive comment on “What effect does VOC sampling time have on derived OH reactivity?” by H. Sonderfeld et al.***

### **Anonymous Referee #1**

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This manuscript reports how OH reactivity values calculated from trace gas measurements could be impacted by unmeasured VOC variability when VOC instruments are not capable of continuous measurements, e.g. Gas Chromatographic instruments. The authors used 1-min continuous measurements of VOCs (PTR-ToFMS) from two different field campaigns to calculate hourly means of OH reactivity. Additional calculations of hourly values were performed by averaging VOCs over different time intervals (5 to 30 min) within each hour to mimic what would have been calculated from GC measurements characterized by various sampling durations. Overall, this study demonstrates that deviations up to 25% could be observed for targeted VOCs. This significant source of errors has to be accounted for when calculated OH reactivity values are compared to direct measurements.

This manuscript is well structured, clear and concise, and will be of interest for the

C1

atmospheric community. I therefore recommend publication in ACP after the authors address the following minor comments:

Minor comments:

P2 L12: “. . .it sinks are manifold. . .” should read “. . . OH sinks are manifold. . .”

P5 L9: Was the Kore PTR-ToFMS equipped with an ion funnel? It seems so since the study of Barber et al. (2012) is cited. It should be clearly stated in the text and the authors should indicate, if relevant, how the ion funnel impacts the PTRMS response (sensitivity, humidity effects. . .).

P5 L13-14: “For background measurements a hydrocarbon trap was employed”. Please provide details about this hydrocarbon trap. How efficient was it to scrub hydrocarbons. How were background measurements performed for OVOCs? How often were the background measurements recorded?

P5 L17-18: “The stability of the instrument during the campaign was monitored with a bromobenzene internal standard”. Could the authors indicate how stable it was during the campaigns? Was there a need to correct for a drift in sensitivity? If so, how was it done?

P5 L25-26: Was an ozone scrubber used for the GC measurements?

P18 L10-11: “A sampling time of only five minutes can cause a deviation of more than 25%. Accordingly, this would then artificially contribute to missing OH reactivity.” This reviewer does not agree with the last sentence, which should be rephrased. The deviation will either lead to a positive or negative bias and will not always appear as missing OH reactivity. This deviation should be discussed as an additional source of errors to account for when measured and calculated OH reactivity values are compared.

P18 L21: “. . .(cf. Figure 4). . .” Wrong figure.

P23 L5-10: “As can be seen in Table 9 at 20min still 2.78% of the ClearLo data exceed

C2

their hourly mean. At 30min all data lie within 7 the range of the standard deviation. Therefore, a sampling time greater than 20 min would be 8 required to represent the hourly mean. The random data reach a comparable level of data 9 exceeding the hourly mean by 2.80% for averaging over 5min only. Here, sampling for only 10 10 min would be sufficient for representing an hour worth of data.” It should be clearly stated that sampling periods of 5-20 minutes would be fine for these specific dataset but that longer sampling periods may be necessary for other environments, especially for measurement sites close to different types of emission sources (e.g. industries).

Fig. 5: Please indicate what “bvf” and “bvfo” mean in the caption.

Fig. 9: As indicated in the main text P18 L19-22, Fig. 9 displays “consecutive 5 min averaging periods within the hour”, i.e. 12 independent periods of 5 min. Since the deviation observed depends only on missing VOC variability for the 5-min calculations, shouldn't an average of the 12 residual slopes be zero? It is obviously not zero for each panel of Fig.9.

Fig. 12: Please indicate in the caption what the error bars are.

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