

Interactive comment on “Branch-level measurement of total OH reactivity for constraining unknown BVOC emission during the CABINEX (Community Atmosphere-Biosphere Interactions Experiments)-09 Field Campaign” by S. Kim et al.

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Received and published: 14 July 2011

We appreciate the referee's comments and constructive criticism on the paper. In general, we agree that we need for quantitative descriptions for comparing measured and calculated OH reactivity values as the referee pointed out. We revised the manuscript to more clearly describe limitation of results from this study and quantitative comparison between measured and calculated OH reactivity. We also added the use of a

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quantitative model to compare directly with the measured OH reactivity. Here, we respond to the five major points in the referee's concerns:

1) We agree with the comment that ideally, we should have two different PTR-MS systems for direct comparisons between measured and calculated OH reactivity. However, few research groups would be able to utilize two PTR-MS systems for this measurement considering the cost to obtain and operate the instrumentation. Therefore, the aim of this study was to demonstrate OH reactivity and VOCs observations with one PTR-MS systems and show how we can interpret the data with some limitations with auxiliary measurements such as temperature and PAR. We think it is worthwhile to report to the atmospheric chemistry community how researchers can expand measurement capability with limited resources (i.e. one PTR-MS system). In the revised manuscript, we clearly describe our aims and limitations as the referee suggested

2) Our assumption is that if significant unknown/unmeasured BVOC emissions are responsible for missing OH reactivity, then we should be able to see this discrepancy with our measurement set-up. Therefore for the first intuitive analysis, the temporal variations of measured and calculated OH reactivity were shown. For more quantitative comparison in this step, we added error bars in the all figures, showing the temporal variations of measured and calculated OH reactivity. In addition, we used a quantitative model to predict OH reactivity for the same measurement periods as the observed OH reactivity. Again, although we agree with the referee's point that this analysis has some limitations, we think this approach does constrain missing OH reactivity from unmeasured/unknown BVOCs. We clarified this point on the revised manuscript.

3) We have used two approaches to make the comparison more quantitative. One approach is use periods of similar temperature and PAR for comparison of observed and predicted OH reactivity analysis.. The second approach added for the revised manuscript is to use a quantitative model to predict OH reactivity for the same period as the OH reactivity measurement. It is a routine quantitative analysis method to plot BVOC emission and temperature to assess temperature dependence on BVOC emis-

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sion. We adapted the method to quantitatively compare measured and calculated OH reactivity from BVOC emission. In addition, the color coding of PAR over the given temperature range provides a quantitative clue to understand the effect of the important physical parameter (PAR) to differences in measured and calculated OH reactivity. We discussed the advantages of controlled environmental conditions in the revised manuscript. We clarified that there are only two data points in the highest temperature bin for the pine-tree measured OH reactivity dataset.

4) With all due respect, we disagree with the referee's point that we should not add oxidation model calculation results in this paper. Although there are a number of papers about isoprene oxidation chemistry and its implications, it is valuable to assess the implications from those new findings at this study site. The modeling results clearly provide quantitative information regarding how much of potential missing OH reactivity can be explained by first oxidation products of isoprene. This information also will provide quantitative perspectives for the importance of constraining isoprene oxidation products

5) We agree that water vapor and CO₂ measurements are necessary to understand the physiology of plants. However, this was not the objective of this study.

Minor Comments

Page 7786 lines 6-9 Since PTR-MS is not able to speciate monoterpenes which is the uncertainty in the measured OH reactivity?

In the method section, we described that we sampled GC-MS samplings to observe speciation of MT and SQT from the enclosures. We made it clear in the revised manuscript.

Page 7787 line 19: Probably you mean 2009 and not 2010.

That is correct. We corrected in the revised manuscript

Page 7788 line 19: ".....temperature drop in the..." please change with ".....temper-
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ature drops in the....."

We corrected it in the revised manuscript

Page 7806 figure 6 Remove "the blue" and "the red" because the color of the triangles and the circles change as function of PAR.

They are meant to indicate one minute average data points rather than the 5 oC moving average data points, color coded by PAR. We made this point clear in the revised manuscript.

Page 7790 line 1 Probably here ".....higher than measured....." is ".....higher than calculated.....".

We corrected it in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 7781, 2011.