

Thank you for your comments on the manuscript. All your comments help us improve the manuscript significantly.

We corrected all the typos you pointed out in the revised manuscript and below we answer your other substantive comments.

This paper describes measurements of formaldehyde over a forest canopy at the Blodgett Forest field site in California over a 4-week period in 2007. Evidence is presented that on warm, sunny days the formaldehyde levels are higher than can be explained, suggesting strongly that the formaldehyde is being produced in the canopy, possibly from very reactive biogenic hydrocarbons. Regardless of the factors controlling the HCHO on “Low” days, the evidence seems to point to an additional source on the “High” days.

1. Unfortunately, only four such days were present during the month-long study period, which raises the question of just how representative they are. The only data from individual days are cramped into Figure 4, and otherwise averages are shown. It would be good if the authors could expand maybe days 259-266 so that the individual days were discernable. That way it would be possible to see if the results were being unduly impacted by an outlier.

The time-series of observed species was expanded for days 259-266. However, not only does the box plot of HCHO diurnal variations (Fig. 5) show distinct differences for daytime (roughly for 10:00~16:00) data between the High and Low phases, but further the diurnal variations of other BVOCs and HO<sub>x</sub> show similar pattern differences supporting the HCHO observations. Most notably, the HO<sub>2</sub> was significantly lower in the HCHO Low phase. In addition, the mean HCHO production rates from the known sources were also significantly suppressed during the Low phase, although they cannot fully explain the elevated daytime HCHO observed in the High phase. All these observations and estimates appear to support that the results were not impacted by individual outliers.

2. A large part of the discussion focuses on formaldehyde produced from acetyl radicals. However, there is no mention of acetaldehyde concentrations. I think it is critical to discuss the possible contribution of acetaldehyde in this context.

Acetaldehyde was included in the calculation of steady state PA radical concentrations. Also, in response to another reviewer's comments we have added additional precursors to PA production not included in the original manuscript draft following Lafranchi et al.'s (ACP, 2009) analysis (See the response 2-4 to D. Taraborrelli's comments "RC C2383"). We have added brief descriptions about their contributions to steady state PA estimates in the revised text.

3. The results as they stand are fairly convincing. The paper is quite well written, with errors in places. The referencing is quite bizarre at times, and some references to original work should be included. As described above, the figures are not always as informative as they could be. Overall, the paper can be published after minor revisions.

3-1. P9849, L14: What is the integration interval? Is that value per  $\text{cm}^{-1}$  or just across the line width?

The interval is across the line width, which is equivalent to  $\sim 0.1 \text{ cm}^{-1}$ .

3-2. P9851, L10: Is Tuazon and Atkinson the correct/best reference for  $\text{NO} + \text{O}_3$ ?

We corrected the rate constants according to IUPAC recommendations (Atkinson et al., ACP, 2004)

3-3. P 9851, L 15: What does this mean? If  $\text{NO}_x$  data is available for days 245-279, then it includes the HCHO measurement period (days 259-277).

We have corrected this description. The diurnally averaged  $\text{NO}_x$  data during the period of the HCHO measurements (day 259~279) were used in the analyses, because only intermittently obtained  $\text{NO}_2$  data are available. Thank you for pointing it out.

3-4. P 9860, L5: I would not say that HCHO reaches an equilibrium (which is clearly not the correct description). Looking at the data, I would not even say it reaches steady state, it simply passes through a minimum. So the mention of HCHO lifetime is not really relevant.

We have corrected the description. We intended to describe that the calculated HCHO decreases towards a steady-state level due to the excess HCHO loss, and levels off in about 3 hours from the initial point. We did not intend to say that HCHO exactly reaches steady state.

3-5. P9863, 2 paragraphs starting L19: I am not sure how germane this discussion of OH recycling is. Since the present measurements offer no new evidence for OH recycling it doesn't seem like this discussion adds anything to our understanding, and the authors do not make it clear if this is a region where such reactions would be important.

Response: According to Mao (AGU Fall meeting, 2008), modeled OH concentrations were significantly lower than observations, whereas model simulations of HO<sub>2</sub> roughly match the observations. This gap between model simulations and observations in OH is likely to show that Blodgett Forest is indeed a region where additional OH recycling processes would be important. Similar results were also found recently by Hofzumahaus et al. (Science, 2009) in a low-NO<sub>x</sub>, high-VOC environment. We have made a brief description about the coauthors' results in the revised manuscript, as a prelude to a future HO<sub>x</sub> manuscript (Mao et al., in preparation.)

3-6. Appendixes. In general, I would suggest adding a reference to an Atkinson review (IUPAC, for example) and using that and/or the review article by Tyndall et al., which is already in the reference list, for rate constants and mechanisms.

Response: We corrected the reference to Atkinson et al. (ACP, 2006) and also changed all of the pertinent rate constants according to their work.

3-7. P 9867, L8-10: The reaction numbers do not agree with the reactions below.

Response: We have corrected them.

3-8. P 9868, Eq. (C2): I am not sure if the factor of 0.95 is correct. It is probably taken into account in measured HCHO yields, and hence is included in the 0.63. In any case, neither lab nor field measurements are accurate enough to warrant a factor of 0.95 being included.

Response: Yes, the factor of 0.95 was inserted by mistake and it is corrected in the revised version. A detailed discussion concerning this issue is provided in the response 2-3 to D. Taraborrelli's comments "RC C2383".

3-9. P 9869, L6: The MeGLY lifetime of 16 hours is calculated for an OH concentration of 1E6. For the conditions given here, OH is 4~5 times larger, and the MeGLY lifetime correspondingly shorter.

Response: We included the OH-initiated MeGLY oxidation in the calculation. More details about the estimate of steady state PA concentration are described in the response for 2-4 of "RC C2383".

3-10. P 9869, L15: Beine and Krognes not an appropriate reference, use Atkinson.

Response: Referred to Tyndall et al. (2001) and corrected the rate constants according to their recommendations.

3-11. P 9870, L7: As far as I can tell, Takezaki et al. measured decomposition rates of dimethyl peroxide. So, the chemistry concerns methoxy, but not methyl peroxy, which is implied here. As suggested earlier, use a recent review for the CH<sub>3</sub>O<sub>2</sub> reaction mechanisms.

Response: Referred to Lightfoot et al., Tyndall et al., and Atkinson et al. (2006) instead of Takezaki et al.

3-12. Table 2. Not good references for OH+isoprene or MBO.

Response: Referred to Atkinson et al. (2006).

3-13. Figure 10. Would it be possible to reverse the order of the species in the legend?

Right now, the figure reads bottom to top, but the legend reads top to bottom.

Response: We corrected the order of the species in the legend.