Reviewer #2

This paper investigates how nighttime oxidation of biogenic hydrocarbons contributes to organic aerosol loading. For their analysis the authors used vertically resolved aircraft data from low approaches to airfields in Houston, TX, reporting vertical profiles of NO3, N2O5, NO, NO2, O3, speciated VOCs, aerosol composition and aerosol size distributions, along with profiles of potential temperature. Box model simulations are used to estimate the magnitude of organic aerosol production. The authors conclude that organic aerosol is indeed efficiently formed within the nocturnal boundary layer, as a result of the oxidation of biogenic VOCs by nitrate radicals.

The paper provides new insights into chemical transformations during the nighttime, which take place in a stratified boundary layer. Vertically resolved measurements of chemical species are comparatively rare, hence this paper is a particularly valuable contribution. The topics of secondary organic aerosol formation and anthropogenic/ biogenic interaction fit well within the scope of ACP, and I recommend this paper for publication after the comments below are taken into account. I believe that these fall into the category "minor revisions".

General comment: While the analysis of the various flights is very thorough, it is easy for the reader to get lost in the details. It would be useful to add an additional paragraph that summarizes the take-home messages from the analyses of the three flights, highlighting differences and commonalities between them, and (for modelers) highlight which general features models should strive for replicating. It would also strengthen the paper to put the findings from this work into perspective with findings from previous studies.

Please see response to reviewer #1 above. This is an excellent suggestion. The new section 3.4 is intended to provide the requested summary.

The introduction already includes a literature review of prior laboratory, field and model work on the topic of nighttime secondary organic aerosol formation, including a description of how vertical profiling from an aircraft contributes to this literature.

Specific comments:

1. page 11870, line 18: The text and Table 1 mention that there were four nighttime flights, but the first one (Sep 29) is never discussed any further. Please state briefly the rationale for this choice.

The following has been added to the end of section 3:

"The September 29 flight was similar to October 8, but has not been illustrated specifically with a case study below, in part because it did not extend as far in time beyond sunset as the other flights."

2. None of the graphs show error bars. If these make the plots too cluttered, it would be helpful to add some information on uncertainty in the text. This is especially important when discussing the differences in k(NO3) and 1/tau(NO3) (e.g. Fig. 6, p. 11877, line 1). Given the uncertainties of the quantities that enter the calculation to derive these parameters, are these differences significant?

Instrumental uncertainties have been published in previous papers on the Texas 2006 night flights, but do not appear in this manuscript. We have added the following sentence to clarify the comparison between $\tau(NO_3)^{-1}$ and $k(NO_3)$.

"The uncertainties in $\tau(NO_3)^{-1}$ and $k(NO_3)$ are 20% each, based on the stated uncertainty for NO_3 (the dominant uncertainty for $\tau(NO_3)^{-1}$) and for the VOC measurements by the PTRMS (Brown *et al*. 2011; Parrish *et al*. 2008)."

3. Page 11872, line 20: When describing the potential temperature profile, the term "discontinuities" is used. I suggest rephrasing this to "the gradient of the profile changed" or something along those lines, since the profile is certainly not discontinuous. This term is used a few times in the paper.

We agree. Instances in which the term "discontinuity" was used to describe observed vertical structures in potential temperature have been replaced with the term "discontinuous altitude gradient," or similar wording.

4. Page 11874, line 25: The discussion of Fig. 4c is unclear. (". . . parameterizations for primary organic carbon emissions. . .") The figure doesn't show any emissions, please reword this.

We thank the reviewer for identifying this inconsistency. "Emissions" has been changed to "mass loadings" in all instances where the term "emission" had been used incorrectly.

5. The sequence of graphs is different for the three cases, for example case 1 does not have a figure that corresponds to figure 6. This makes the analysis somewhat unsystematic. Is there a particular reason for this choice?

The reason for this choice was stated in the discussion of case 1, for which the biogenic VOCs did not show measurable enhancement at low altitude, either due to sampling over a lower emitting area or due to more rapid oxidation by NO_3.

6. While section 3 discusses three flights, from section 4 on only two flights are presented. What is the rationale of this choice?

The justification for the choice had been given in introduction to section 4: "These profiles all occurred more than five hours after sunset and spanned the two different transport regimes (north to south and the reverse) described above."

We have added the following statement:

"The averages of profiles from October 8 and September 29, which all occurred earlier in the evening, show the same dependences of organic, sulfate and nitrate with height, but with smaller relative variation as a function of altitude."

7. Box model simulations: If a detailed description of the model of which reactions are included and what method is used to solve the equations etc. is available in another publication, please cite this here. If there is no such reference, I suggest including a table that lists the reactions that are included in this box model and a short description on numerical methods.

Please see responses to reviewer #1 above. Table 3 now includes the reaction scheme, and the following description has been added to the text.

"Table 3 lists the simplified chemical mechanism, rate coefficients and relevant references. The differential equations corresponding to this set of reactions were numerically integrated using an adaptive step size (Rugne-Kutta) method."

8. Page 11888, line 17: "other simulation parameters are similar". Please be more specific here, are they the same, or are they different? If they are different, please specify.

"similar to" replaced with "the same as"

9. Figure 17 and corresponding text: Why was the emission rate for this model run not consistent with the observed temperature? It seems like this would be a straightforward choice of parameter choice.

The intent of this simulation was to determine the maximum potential aerosol production from nighttime chemistry, and is now stated in the text.

10. Figure 17 and 18: How exactly is the blue shaded area obtained?

The figure caption has been modified: "The blue shaded range indicates high and low SOA yields (see text) for monoterpene oxidation."

Technical comments: 1. Page 11872, line 11: remove first "within"

removed

2. Labeling of subfigures is in capital letters but in the text they are referred to with lower-case letters. This should be made consistent.

Figure labels had been consistent in the originally submitted version of the manuscript. We will work with the editorial office to ensure consistency in the final manuscript.

3. Page 11872, line 21: at the very end of this line, add “being”.

The word “being” is unnecessary in our opinion.

4. Page 11873, line 21: reference to Table 1 is wrong.

Several figure and table references were scrambled in the ACPD version of the manuscript relative to the submitted version. We hope this error will be corrected in the ACP manuscript.

5. Page 11874, line 5: “enhanced nitrate”, suggest to change this to “enhanced nitrate concentration”

“mass loading” added

6. Page 11882, line 8: remove second “in”

removed

7. Table 2: notation for k(NO3) and k(O3) is inconsistent in caption and in table header.

Corrected

8. Figure 8b: typo in legend for red line.

Typographical error not clear to us?