Atmos. Chem. Phys. Discuss., 15, C11186–C11188, 2016
www.atmos-chem-phys-discuss.net/15/C11186/2016/


Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Understanding isoprene photo-oxidation using observations and modelling over a subtropical forest in the Southeast US" *by* L. Su et al.

**Anonymous Referee #2**

Received and published: 6 January 2016

This paper shows some very interesting data collected during SAS field campaign over Southeast US in the summer of 2013. The authors first show the data collected from the WASP system on an aircraft, and then show surface data collected over SEARCH and AABC sites. Then the authors use a box model (MXLCH) with prescribed boundary layer conditions and chemical fluxes to simulate species associated with isoprene oxidation. They find that their model is in good agreement with surface and aircraft observations for isoprene, NOx, O3, MVK+MACR, ISOPN and other species. The model is then used to understand the contribution of different chemical and physical processes to isoprene oxidation. This paper is certainly of great interest and well within the scope of ACP, but it can also be significantly improved. I have several comments:

1. Figure 3 suggests strong gradient of isoprene at noontime, from 4 ppbv at 200 m to 1 ppbv at 1200 m. While the MXLCH model agrees well with averaged values from WASP system and NCAR C-130 aircraft (Figure 5), the comparison for ISOPN, NOx, and OH is in fact between surface observations on the tower and modeled bulk values within the whole CBL. I would expect some difference between surface observations and modeled bulk values for these species. Some caveats should be given here on comparing these species.

2. I am impressed by the good agreement between observed and modeled ISOPN. But I think the authors should provide more details on this comparison.

For observations, what is the observed ISOPN? Does it include all daytime C5 hydroxyl isoprene nitrates? Or just some isomers? Is there any nighttime isoprene nitrates being measured here?

For model, the authors show a short lifetime of ISOPN, but it seems to me that it is mainly due to a fast ozonolysis rate, which has been suggested to be much slower from recent lab data. How would that impact their results? There has been discussion on hydrolysis of ISOPN. Did the authors see any evidence of that? How would change their results?

3. It seems to me that comparison of HCHO should be included in Figure 5, if possible. Also I don't see observed HO2 in Figure 5. It would make sense to make sure that modeled HO2 is in the right range, before the discussion of NO:HO2 in the following sections.

4. In Equation 1, what is the role of advection here? Given the lifetime of ozone, I would expect advection plays a role in its budget. This should be discussed and quantified in the text.

Minor comments:

1. Page 31624 Line 11: "Six isomeric hydroxyl-substituted isoprene peroxyl radicals



ACPD
15, C11186–C11188, 2016





(HOC5H8OO; ISOPOO) are then produced." There are minor channels that not considered in current mechanisms. I wouldn't use six here.

2. "model outputs generally agree with observations of OH concentration during noontime (Shirley et al., 2006; Hofzumahaus et al., 2009)." Please read the cited papers and check your statement.

3. Page 31641, Line 4: "The ISOPN yield in the complex scheme is set at 6 %, which is within the range of the results from the chamber experiments (93%) carriedout at the SEARCH site (Xiong et al., 2015)." But it appears that the authors use 12% in Table S3. Please clarify this.

4. Figure 7, it should be "CH3C(O)OO" not "CH3(O)OO".

5. Page 31645, Line 4, "One possible explanation of the large discrepancy between model output and observation is the partitioning of ISOPOOH to aerosol phase due to its lower vapour pressure and potentially high condensed phase reactivity (Rivera-Rios et al., 2014)." It seems very unlikely that this discrepancy is due to the partitioning.

---

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 31621, 2015.