

## ***Interactive comment on “Chemical and isotopic composition of secondary organic aerosol generated by $\alpha$ -pinene ozonolysis” by C. Meusinger et al.***

**S. Irei**

satoshi.irei@gmail.com

Received and published: 24 February 2016

I recently found this interesting publication in ACPD. In this article, the authors are citing my publications (thanks for using them). I downloaded it and looked how my publications were cited. I then realized that the authors may have misused one of my publications, "Irei et al. (2015)".

I have published a paper in 2015 in Atmospheric Environment, reporting isotopic composition of low-volatile fraction in airborne PM and fraction of m/z 44 signal in organic mass spectra (f44). The main focus was to report the limitation of f44 oxidation indicator measured by AMS. I have also published another in 2015 in Journal of Physical Chemistry A, which is the one the authors are citing. However, this paper reported a

C1

different topic from ambient aerosol, a study of chemical reaction mechanism using the information of site-specific kinetic isotope effects, which were based on the results of laboratory studies for VOC oxidation.

After checking the discussion citing "Irei et al. (2015)", I found that none of these does not seem to fit the referring sentence in the text (page 2, line 26-28), but likely the one published in 2014 in Environmental Science and Technology. Even if so, I had impression that the authors may have misinterpreted this publication; The authors seem to mean that the publication reported chemical reactions inside PM resulting in the  $^{13}\text{C}$  depletion in low-volatile fraction, however, I have not ever concluded so.

Rather, I meant in the paper that atmospheric oxidation of volatile organics more likely resulted in the light isotopic composition (less  $^{13}\text{C}$ ) of low-volatile fraction. That is, the low-volatile fraction we observed was really SOA formation converted from volatile organics, but not oxidation products converted from chemicals that already stayed inside PM. These two are different processes. Unfortunately, I have not found a clue for the latter process in ambient measurements yet.

I would like to ask the authors to make corrections above in the future publication of this manuscript, if my points were correct. I apologize if I am misunderstanding the statement.

---

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-98, 2016.