

Interactive comment on “Seasonal variations of stable carbon isotopic ratios and biogenic tracer compounds of water-soluble organic aerosols in a deciduous forest” by Y. Miyazaki et al.

Anonymous Referee #2

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This paper presents unique and interesting data on the sources of water-soluble organic carbon associated with total suspended particulate matter (TSP) measured at a forested site. It provides new insights on the contributions of both primary and secondary sources, and the possibility of an important role of emissions from the forest understory and forest floor, relative to the canopy. The results, in my view, are somewhat obscured, however, by the fact that only TSP was measured and reported, and so it is not surprising that a mix of primary (likely coarse) and secondary (likely fine) particle sources contribute. Indeed, the Abstract does not mention that the results pertain to TSP, which I feel is a significant oversight. After consideration to some details discussed below I support publication of this manuscript.

In the abstract and the throughout the text it needs to be made clear what aerosol size range these results pertain to; specifically the WSOC reported is TSP. (An actual estimate of the upper size limit of the filter sampling system would be very useful). This is important since it tends to explain why the authors see significant contributions from both secondary and primary sources, whereas one may expect significant less contributions if the focus was say, fine WSOC. As a further example, the statement (page 30781 lines 26...) Quoted from the paper: In summary, the present results indicate that at this forest site, the SOA formations associated mainly with α -/ β -pinene oxidation and the primary emissions from biological sources contribute almost equally to the peak of WSOC in the growing season of early summer as well as in autumn (end quote). It needs to be specifically stated that TSP WSOC is being discussed.

Also, when reading the abstract my impression of the canopy floor is one barren of living vegetation and composed mainly of decaying plant matter. In this forest, it appears there is significant mass of living photosynthetic plants at or near the surface (page 30776 lines 8-10). The last line of the Conclusions provides this information in a nice format and I suggest should be included in the abstract.

A few more lines describing the NEE method (Section 2.3) would be useful. For example, details on the CO₂ measurement to determine eddy covariance fluxes, how was the change in CO₂ storage in air mass from ground to eddy flux measurement determined. Although more details are provided in the reference, the reader needs to know more on what is essentially a mass balance analysis, as it is an important component of this paper.

The Method section also needs a clear description of the principle behind the delta13C(WSOC) measurements; what does it tell you and why or how does this measurement provide this information.

Could not find any discussion of the MSA measurement?

No discussion on issues, if any, with measurement blanks, possible sampling artifact

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issues with integrated filters; are any expected, is this measurement include semi-volatile components, is any semi-volatile WSOC expected.

In figure 2 a very distinct peak in WSOC is seen in May-June 2010, and with additional data, is attributed to the growing season. However, measurements were made at the beginning of the study (June 1, 2009), but which appear not to show this large peak. Can the authors be sure that the may 2010 are a consistent feature of this forests seasonal trends? The same is true of MSA, it appears unique to 2010 since a similar large peak was not observed in 2009. Thus the discussion of section 3.3 seems to only apply to the rather unique year of 2010. Some discussion on this should be included.

In the factor analysis, as summarized in Figure 4, and throughout the paper when seasonal trends are discussed, are all the data used? That is there are some seasons in which sampling occurred twice and some seasons where sampling occurred only once.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 30773, 2011.

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