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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterization of organic nitrogen in aerosols at a forest site in the southern Appalachian Mountains" *by* Xi Chen et al.

Anonymous Referee #2

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General comments

This paper presents the analytical results of water-soluble organic nitrogen (WSON) for both bulk and related molecular compounds in PM 2.5 filter samples collected at a remote montane forest site in the U.S. The authors present the season variation of WSON and related organic molecular compounds to characterize aerosol WSON and investigate its possible sources. Combination of bulk WSON and molecular tracer compounds related to WSON and WSOC obtained in the forest environment provides new insights into our understanding on aerosol WSON particularly from terrestrial biogenic sources. While the data presented are valuable, there are some important issues that

need to be worked out and clarified before I recommend its publication in ACP.

Specific comments

(1) One of my concern is on the interpretation for the positive correlation between biogenic SOA tracers and ambient temperature (Lines 470-476, 562-565). The authors conclude that such a relationship indicates temperature dependence of "oxidation." It may be true to some extents, but how about the temperature dependence of VOC emissions? Most of terpenes generally show temperature dependence of emission, which can also explain the correlation shown in this manuscript.

(2) The authors use the term "aged biogenic SOA" (e.g., Lines. 560-565 and others) in the text. Please add more discussion about specific time scale on this aging (hours, days?). This should be discussed relative to the time scale of transport (e.g., vertical mixing within the forest canopy or between the canopy and the above atmosphere, horizontal transport, etc.).

(3) Nitro-aromatics: In section 3.3., the authors conclude that the contribution of nitro-aromatics to WSON was generally "small," whereas they state potential importance of nitro-aromatics to the atmospheric N deposition budget (L.507-509) in section 3.6. These statements do not seem to be consistent and confusing.

(4) Lines 383-384: If the event cannot be attributed to local burning, then what is the most likely origin (source location)? "Long range transport" is not enough to explain the source of the observed particles in this event.

(5) Section 2.3: The authors should describe the measurement uncertainties for each analysis. This is particularly important for the analysis of WSON, whose measurement uncertainty includes propagation of errors of WSTN, NO3-, NH4+,. . ..

(6) Figure 6: I think that the author should show time series of integrated factor contributions vs. the measured WSOC concentrations to show how well the PMF reproduced the measurements. Then the authors should show fractional contribution of each factor

to WSOC in the time series as they discuss it in the text.

(7) The authors use the term “N/C ratio” in the manuscript: Lines 42,43, 517, 524, 592, and 593. Should this term be “(WS)ON/OC ratio?” “N/C” includes inorganic N and elemental C.

(8) Lines 522-525: The identified-ON/WSON ratios also show a seasonal difference (Table 4). Can the authors add a few more statement on this difference in terms of unidentified compounds?

Minor comments

(9) Abstract: The authors should specify that the sampled aerosols are PM2.5.

(10) L.312: Please define “[O3]” here.

(11) L.394: Correct “extreme” to “extremely.”

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