

Interactive comment on “Sources and oxidative potential of water-soluble humic-like substances (HULIS_{WS}) in fine particulate matter (PM_{2.5}) in Beijing” by Yiqiu Ma et al.

Anonymous Referee #3

Received and published: 24 January 2018

This paper is of general interest. It provides insights on sources of DTT activity associated with organic species in Beijing. There are, however, some major issues to address. First, the grammar is an issue; the paper needs editing. Secondly, unfortunately the authors apparently quantified DTT activities using a non-standard method, ie that of Li et al., 2009, which differs from the original DTT protocol described by Cho et al. (2005). The Cho method is widely utilized and is the basis for most DTT activities reported in the published literature. This means that the DTT activities reported here cannot necessarily be directly compared to generally published results, unless some type of conversion factor is given. This should be noted in the paper, discussed in detail (ie, difference in protocols explained), and optimally, a conversion factor given

Printer-friendly version

Discussion paper



based on further experimental work by the authors. Finally, despite a large contribution, there really is no explanation or in depth discussion of why the secondary PMF factor contributes most to DTT activity in summer.

Specific Comments:

Discuss possibility that DTPA not only reduces contributions of metals to DTT activity, but also additional species, such as quinones. Could this result in significant under-measurement of DTT activity in this work? What was the justification for wishing to remove metals contribution to DTT? Are they not part of HULIS, ie possibly as a part of a metal-OM complex?

Lines 116 to 120. This paragraph is not clear. Why should DTT activity be proportional to HULISws concentration with this method. This was a finding reported in the results, but it appears from this paragraph that it simply results from the method used. Clarification is needed.

Somewhere explicitly define the difference between total HULIS, HULIS and HULIS-associated DTT. I assume HULIS-associated DTT is just the DTT activity of the HULIS sample? Sometimes, just the term HULIS is used, which adds to the confusion. Is this total HULIS?. Consistency throughout the paper is needed. Suggest call it HULISws mass concentration and DTT activity of HULISws to distinguish the two.

The PMF factors need clarification. Vehicle emissions apparently include POA (primary) and SOA from vehicle emissions, maybe this should be noted in the figs? Is secondary aerosol mainly biogenic SOA, or are there anthropogenic species contributing to it? If both, can they be separated? Why is there little sulfate in coal combustion? Are there secondary species in biomass burning?

The discussion of possible reasons why DTT activities of the secondary factor are so high in summer needs more attention. Are the authors saying that biogenic SOA is the cause? There are papers that make this dubious claim (Kramer, A., W. Rattanavaraha,

[Printer-friendly version](#)[Discussion paper](#)

Z. Zhang, A. Gold, J. D. Surratt, and Y.-H. Lin (2016), Assessing the oxidative potential of isoprene-derived epoxides and secondary organic aerosol, *Atmos. Env.*, 130, 211-218.). Given that most sulfate is in this factor (and surprisingly in contrast to the coal combustion factor), it seems that the factor is really all about oxidation processes (as the name for the factor implies). One explanation is that this factor really has contributions from all source, such as coal, vehicles, and even biomass burning, given the loss of levoglucosan through oxidation (although this source is lower in summer). Verma et al, 2015a noted the strong dependence of aging on enhanced quinone DTT activity. This factor may just represent this process. Despite significant discussion of the other factors, this factor is not considered sufficiently give the large role it plays in summer DTT activities, a major finding of the paper.

Lines 271 and on. DTT per HULIS mass is reported. This is interesting, but also interesting would be DTT activities per OM. One could also expand the comparisons of these types of numbers from this study to the large list reported in a recent paper (Shiraiwa et al., 2017), Aerosol health effects from molecular to global scales, *Envir. Sci Technol.* , 51, 13545-13567). Make sure to note differences in DTT analytical methods when doing the comparison (eg, no metals in this work).

Regarding trash burning and DTT activities, also see: Vreeland, H., J. J. Schauer, A. G. Russell, J. D. Marshall, A. Fushimi, G. Jain, K. Sethuraman, S. N. Tripathi, and M. H. Bergin (2016), Chemical characterization and toxicity of particulate matter emissions from roadside trash combustion in urban India, *Atmos. Env.*, 147, 22-30.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, <https://doi.org/10.5194/acp-2017-740>, 2017.

[Printer-friendly version](#)[Discussion paper](#)