

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.

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It's well known that the toxicity of PM2.5 is greatly related to its chemical composition and pollution sources. This work analyzed PM2.5 samples collected in Beijing during a one-year period, and the levels and oxidative potential (indicated by DTT) of a major water-soluble PM2.5 component, i.e. water-soluble humic-like substances (HULIS_{WS}), were reported. With the aid of various characteristic source tracers, PMF was applied to apportion the major sources of both HULIS_{WS} and its associated DTT activity. There are some new and interesting findings. The major sources of both HULIS_{WS} and DTT activity were coal combustion, biomass burning, traffic exhaust, waste incineration, and

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secondary formation. Waste incineration was probably identified as a contributor to HULISWS for the first time. Moreover, HULISWS from vehicle emissions was found as the most ROS-active, and HULISWS from secondary aerosol formation showed a lower intrinsic DTT ability than those of most primary sources except for coal combustion. This paper is well-written. The study is clear, informative, and novel in general, and the major data and their interpretation are scientifically sound. I suggest it to be considered by ACP for publication if the following concerns could be addressed.

Author Response: Thanks for the comments.

1. Line 22: Is it necessary to define the waste as “plastic waste”, as terephthalic acid is a marker of plastics?

Author Response: Since terephthalic acid is an important industrial material for making PET (polyethylene terephthalate) plastics, they have been found to be abundant in plastic burning smokes. In developing countries, plastic materials are dumped as domestic waste, which makes the waste plastic-enriched. In this case, terephthalic acid serves as a marker of plastic burning and plastic-enriched domestic waste burning. Therefore, we will keep the name of this factor as “waste incineration”.

2. Line 39-40: Add “an” before electron and “a” before continuous.

Author Response: This revision has been done.

3. Line 71: If the samples were taken every 6 days for a one-year period, there should be 60 samples. Why there were 66 samples as listed in line 123?

Author Response: In winter time, we collected several additional samples during severe polluted periods.

4. Line 100: How many individual hopanes have been identified? I guess the input species “hopane” in PMF must be the sum of all identified hopanes, right? What are the concentrations of hopanes? What are the water-soluble ions identified? The author may need to include a table in the supplementary information that provide levels of



hopanes, water-soluble ions, EC and OC in the batch of PM2.5 samples analyzed.

ACPD

Author Response: Five individual hopanes were identified, and these 5 hopanes were lumped together and put into PMF analysis. The concentrations of the measured hopanes, ions, EC and OC were listed in Table S1 in the revised supplementary material.

5. Line 108: DTPA was spiked to chelate transition metals. Could it also affect or even remove some HULISWS components?

Author Response: In the previous work by Lin and Yu (2011), they assessed the DTT consumption by 1) HULISWS sample without DTPA; 2) HULISWS samples with DTPA; and 3) standard mixture solution of metals with concentrations similar to those measured in HULISWS fraction. They found that the DTT consumption by residue metals in the absence of DTPA is similar to the difference between DTT consumption by equivalent amount of HULISWS in the absence and presence of DTPA. Their result suggested that the DTPA spiked to chelate transition metals could barely affect the HULISWS components. Although Dou et al. (2015) found that DTPA could suppress the DTT response of quinone by about 20%, considering this method is widely accepted in HULISWS redox activity measurement, we adopt the same method in order to be consistent and make comparison with other HULISWS studies (Dou et al., 2015; Lin and Yu, 2011). In the revised version, we mentioned the DTT activity of HULISWS measured was underestimated and could not be directly comparable with that does not use DTPA as metal chelators.

In the revised manuscript, line 126-128, we've added: "Since DTPA was added to suppress DTT consumption by metals ions throughout the incubation process and may affect the DTT response of quinones (Dou et al., 2015), the DTT activity of HULISWS measured here may be underestimated and is not directly comparable to those using DTT assay conducted without DTPA".

6. Lines 130 to 131: Were all the reference data observed during a one-year period

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and comparable to present study?

Author Response: Data reported for Guangzhou and Nansha sites were the average concentration of samples collected during a one-year period (2009 January – 2009 December). For data measured in Lanzhou, they were the average concentration of samples collected during two representative periods (June-July in summer and December in winter during 2012-2013).

Reference: Dou, J., Lin, P., Kuang, B., Yu, J.Z., 2015. Reactive oxygen species production mediated by humic-like substances in atmospheric aerosols: Enhancement effects by pyridine, imidazole, and their derivatives. *Environ. Sci. Technol.* 49, 6457–6465. Lin, P., Yu, J.Z., 2011. Generation of reactive oxygen species mediated by Humic-like substances in atmospheric aerosols. *Environ. Sci. Technol.* 45, 10362–10368.

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