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# ***Interactive comment on “Insights on organic aerosol aging and the influence of coal combustion at a regional receptor site of Central Eastern China” by W. W. Hu et al.***

**Anonymous Referee #2**

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This paper presents detailed results on organic aerosol ageing by performing an observation at a regional site in central eastern China using multiple sophisticated instruments such HR-ToF-AMS, GAC-IC and PTR-MS. The paper is organized well with some very interesting findings, although some important statements need to be clarified. I believe the paper could be accepted after addressing the following questions.

1) There are many interesting results in the paper, but as a regional site the difference in aerosol chemistry needs to be stated more clearly. 2) Page 10811, line 16. Abstract section and the context, authors claimed several times that they reported CCOA for the first time. This is not true. Sun et al have already reported CCOA for Beijing winter

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aerosols using a similar online technique. (Sun et al ACP 2013, 13, 4577-4592) 3) In the paper, authors classified aerosols into four groups, HOA, SV-OOA, LV-OOA and CCOA. The former three groups are defined based on chemical composition and/or volatility, how about the last one, CCOA, i.e., coal combustion organic aerosols. What is the difference between CCOA and other three groups? In fact, coal combustion emissions also consist of HOA, SV-OOA and LV-OOA. 4) Page 10811, line 22, combustion, not combustion. 6) Page 10812, line 16-23, here some statements need to be clarified. AMS usually takes HOA as a surrogate of anthropogenic primary organic aerosols and oxygenated organic aerosols as SOA, but the fact is that plants also emitted lots of HOA like n-alkanes and oxygenated OA like fatty acids. Moreover, biomass burning emissions also contain lots of SV-OOA and LV-OOA like carbonyls, polyols (e.g., glycerol) and carboxylic acids (malic acid), which are directly emitted rather than derived from secondary formation. 5) Supporting information title is not consistent with the paper. 6) Page 10815, line 5-10, Does it cause any particle loss when using such a 3-m long tube? How about evaporation loss under the  $RH < 30\%$  condition, because the marine sampling site is humid. 7) Page 10818, line 10-11, which should be “naphthalene and acetonitrile” not “acetonitrile and naphthalene”. 8) Page 10819, line 14-19, as shown in Fig S-7, high fraction of  $NO_3^-$  was observed compared to other two downwind sites in China. What is the meaning on such an abundant nitrate? The authors stated a positive correlation with higher RH, what is the correlation significance? And how about  $SO_2$ ? If a significant conversion of  $SO_2$  to  $SO_4^{2-}$  was also observed under such a higher RH condition? 9) Page 10820, line 18-19, the statement is not true, see the above. 10) Page 10820, line 15, it should be 0.35 (see Fig. 8), not 0.36. Page 10828, line 18, Yun et al (2013), not 2012. 11) Page 10820, line 25-27, the regression slope is lower at the Changdao site in comparison with those in Mexico City and US, why? What does it imply? 12) Page 10829, line 12-15, if this means the aerosol phase oxidation is more significant? 13) Page 10830, line 5-10, this statement is wrong, because the KOH for this study ( $5.2 \times 10^{-12} \text{ cm}^3 \text{ molec}^{-1} \text{ s}^{-1}$ ) is one order of magnitude lower than in previous studies ( $5-12.5 \times 10^{-11} \text{ cm}^3 \text{ molec}^{-1} \text{ s}^{-1}$ ). 14) Page 10830, line 17-

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20, this seems unreasonable, because many field observations and chamber studies found that SOA production is enhanced under a higher temperature due to enhanced photochemical oxidation.

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