

Interactive comment on “Aerosol chemistry and particle growth events at an urban downwind site in the North China Plain” by Yingjie Zhang et al.

Anonymous Referee #1

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The authors reported the chemical composition and particle growth event at a downwind ground site in the North China Plain. The results mainly focus on analyzing the aerosol chemical composition measured by aerosol chemical speciation monitor (ACSM) and particle number concentration and size distribution obtained based on scanning mobility particle sizer (SMPS). OA sources (cooking OA, oxygenated OA, and hydrocarbon-like OA) were also resolved by combining source apportionment techniques with ACSM dataset. The species contribution (SO₄ and OOA) are the main for particle growth has been analyzed, which is not new since most of the results have already been reported from multiple other field studies in NCP. The CE calculation and the way that the authors analyzed the correlation between chemical component in aerosols and growth rate have their own caveats, as addressed in the comments

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following. Based on all the major comments as following, I recommend a major revision for this paper: Major comments: 1 The detailed operation of AMS and SMPS is totally missed in the paper. No information on how the ACSM were calibrated. 2 The CE needs to be re-evaluated. I have already raised this question in the quick review, however, the authors did not revise it. A CE of 0.51 and 0.57 was estimated based on equation in Middlebrook et al. (2012), when the average mass concentration of main components in polluted and clean days in Table 1 were used. The higher CE than default value of 0.5 is mainly due to acidic effect on aerosol bounces in AMS. Thus, the mass concentration of AMS might be overestimated in this study. 3 Line 111: The statement that the calculate SMPS mass is only 75% of the total PM1 cannot not be used a supporting evidence for CE. Multiple other studies show a good comparison of mass concentration between AMS and integrated SMPS, as examples shown below: Y. Zhang, L. Tang, P. L. Croteau, O. Favez, Y. Sun, M. R. Canagaratna, Z. Wang, F. Couvidat, A. Albinet, H. Zhang, J. Sciare, A. S. H. Prévôt, J. T. Jayne, and D. R. Worsnop: Field characterization of the PM2.5 Aerosol Chemical Speciation Monitor: insights into the composition, sources, and processes of fine particles in eastern China, *Atmos. Chem. Phys.*, 17, 14501-14517, 10.5194/acp-17-14501-2017, 2017. X. F. Huang, L. Y. He, M. Hu, M. R. Canagaratna, Y. Sun, Q. Zhang, T. Zhu, L. Xue, L. W. Zeng, X. G. Liu, Y. H. Zhang, J. T. Jayne, N. L. Ng, and D. R. Worsnop: Highly time-resolved chemical characterization of atmospheric submicron particles during 2008 Beijing Olympic Games using an Aerodyne High-Resolution Aerosol Mass Spectrometer, *Atmos Chem Phys*, 10, 8933-8945, DOI 10.5194/acp-10-8933-2010, 2010. P. F. DeCarlo, E. J. Dunlea, J. R. Kimmel, A. C. Aiken, D. Sueper, J. Crouse, P. O. Wennberg, L. Emmons, Y. Shinozuka, A. Clarke, J. Zhou, J. Tomlinson, D. R. Collins, D. Knapp, A. J. Weinheimer, D. D. Montzka, T. Campos, and J. L. Jimenez: Fast airborne aerosol size and chemistry measurements above Mexico City and Central Mexico during the MILAGRO campaign, *Atmos Chem Phys*, 8, 4027-4048, 2008. The authors should look more carefully into their dataset to find out the possible reasons that could cause this difference. 4 Line 200-215: The cooking OA (COA) and hydrocarbon-like OA (HOA) have

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very similar diurnal variations, which show similar peaks in the noon and night time. It indicates that the PMF did not really separate the two factors. A detailed explanation on separating two factors are needed. 5 Line 291-292: please give statistic analysis result on correlation of Fig. 10 b-c to prove the conclusion stated in this study. I did not see the correlation between growth rate and condensation sink. Minor comments: Line 56-57: Give references. Why did the mass concentration of PM1 in Xingtai ($\sim 30 \mu\text{g}/\text{m}^3$) is so low compared with the measured annual mass concentration ($\sim 90 \mu\text{g}/\text{m}^3$) in the past. Line 108: Have the authors done the RIE calibration of NH_4 and SO_4 . If so, please specify. Line 129: Please give the range of f55 vs f57 in COA in Mohr et al. 2012. Line 271: Give detailed evidence for this sentence. Line 277: Please specify if the CO background is offset here. If not, the background of CO should be deducted in the calculation. For an example, J. A. de Gouw, D. Welsh-Bon, C. Warneke, W. C. Kuster, L. Alexander, A. K. Baker, A. J. Beyersdorf, D. R. Blake, M. Canagaratna, A. T. Celada, L. G. Huey, W. Junkermann, T. B. Onasch, A. Salcido, S. J. Sjostedt, A. P. Sullivan, D. J. Tanner, O. Vargas, R. J. Weber, D. R. Worsnop, X. Y. Yu, and R. Zaveri: Emission and chemistry of organic carbon in the gas and aerosol phase at a sub-urban site near Mexico City in March 2006 during the MILAGRO study, *Atmos Chem Phys*, 9, 3425-3442, 2009. Line 290: No information on how the growth rate was calculated. Line 291: What is the GR range in the literatures. How does it compare to the results from Beijing?

References: Ann M. Middlebrook, Roya Bahreini, Jose L. Jimenez, and Manjula R. Canagaratna: Evaluation of Composition-Dependent Collection Efficiencies for the Aerodyne Aerosol Mass Spectrometer using Field Data, *Aerosol Sci Tech*, 46, 258-271, 10.1080/02786826.2011.620041, 2012.

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