Atmos. Meas. Tech. Discuss., 8, C5572–C5579, 2016
www.atmos-meas-tech-discuss.net/8/C5572/2016/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Profiling the PM_{2.5} mass concentration vertical distribution in the boundary layer" *by* Z. Tao et al.

Z. Tao et al.

zzwang@aiofm.ac.cn

Received and published: 2 March 2016

(1) comments from Referees Anonymous Referee #1ïijŽ

The article "Profiling the PM2.5 mass concentration vertical distribution in the boundary layer" submitted by Z. Tao et al. for its review is a new technique. Previous results show that there is tied relationship between the PM2.5 mass concentration and AOD from the view of statistics. The authors want to match the aerosol extinction coefficient and PM2.5 and stress out the profile of PM2.5 based on a new CCD lidar technique. General comments: In my opinion, the study has a good presentation and has been clearly written. The scientific objective is important, and it is a fair study of a key issue that affects many other scientific works (such as the evaluation of air pollution

and forecast).therefore I consider it to be suitable for its publication on Atmospheric Measuring Techniques journal. Specific comments: 1ïijL'In the page 12934, line 9, should be "side-scatter lidar with a PM2.5 . . ." and line 14, the "characteristic" should be characteristics. 2ïijL'In the page 12935, line 1, "The PM2.5 poses great health risks. . ." is better. 3ïijL'In the page 12936, line 2, ". . .suitable to detect. . ." should be ". . .suitable for detecting.." and line 9, the results are. . . 4ïijL'In the page 12937, line 13, there are. . . 5ïijL'In the page 12940, line 16, ". . .is gotten. . ." and line 22, ". . .are shown. . ." 6ïijL'In the page 12941, line 3, ". . .in previous literature. . ., the Hefei site is situated in the east of China. . ..", line 6, please use "And in Spring, dust aerosol. . .", line 21, in the word "maximums", "s" is unwanted, pls correct it through the article. 7ïijL'In the page 12942, line 3, ". . .a rich structure." and line 7, "from 58% to 70%..." 8ïijL'In the page 12943, line 9, "At the meanwhile . . ." should be "At the meantime . . . "

Anonymous Referee #2ïijŽ

General comments: The manuscript presents a methodology and case study for profiling PM2.5 mass vertical distribution in the planetary boundary layer (PBL) using a CCD lidar and PM2.5 sampler. This subject is quite interesting to the air quality modeling and lidar remote sensing communities because there is lack of direct measurement of PM2.5 vertical profile by EPA and other agencies. The ratio of aerosol extinction to PM2.5 mass (or K) and its height dependence are critical in this study. Authors estimate the ratio K at the ground level with the RH<70% and then convert the aerosol extinction profile to PM2.5 mass profile in the PBL with an assumption of constant K. Three cases show a stable value of the ratio K over the nights, however the potential uncertainty or variation of the ratio K with altitude is not discussed except the RH effect. In addition, the validation on the CCD-lidar retrieved extinction or PM2.5 profile is not discussed or shown. The method might work in the well-mixed PBL where the aerosols are relatively homogenous. Overall, this manuscript is within the scope of Atmos. Meas. Tech. and can be accepted for publication once the following specific comments are taken into account. Specific comments: 1. The potential uncertainty or bias of the

CCD-lidar retrieved aerosol extinction is not discussed or presented. The validation of the CCD lidar retrievals with a Raman lidar or other ground in-situ measurement might be useful to evaluate the errors or bias of the CCD-lidar retrievals. 2. The height dependence or variation of the ratio of aerosol extinction-to-PM2.5 mass (or K) is critical to the accuracy of PM2.5 mass concentration profile. An assumption of homogenous microphysics (size distribution) and chemical compound (refractive index) of aerosols in this study is quite arbitrary for the night-time PBL due to the weak turbulence mixing or even in the day-time (see the varied aerosol sizes from the aircraft measurement by Li, et al., 2015). Thus, authors need to point out this limitation or/and show further discussions or evidences for this assumption. 3. There are a few measurements of aerosol hygroscopic factor f(RH)of scattering coefficients in China (see the reference by Chen, et al., 2014; Zhang, et al., 2015). It seems that the f(RH) is in the range of 1.2-1.4 at RH=70% which is still big. 4. For the case results in this study, if possible, the relative humidity (RH) profile needs to be shown from the measurements from either radiosonde, Raman lidar or microwave radiometer. Some minors: 1. Line-49, pleas add “in diameter” in the definition of the PM2.5. 2. Line-59, “were” should be “was”. 3. Line-117, what is the parameter “D”? 4. Lines 180-183, please add the reference for this point. 5. For the three cases, why is the distance D between the laser beam and CCD camera set to be different? 6. Line 171, Eq.(7) is an approximate formula in which Qext is assumed to be a constant. This is fine for a single particle, but for all different sizes of aerosols in the atmosphere Qext is a function of aerosol radius and refractive index. References: Junxia Li, Yan Yin, Peiren Li, Zhanqing Li, Runjun Li, Maureen Cribb, Zipeng Dong, Fang Zhang, Jin Li, Gang Ren, Lijun Jin, Yiyu Li, Aircraft measurements of the vertical distribution and activation property of aerosol particles over the Loess Plateau in China, Atmospheric Research, Volume 155, 15 March 2015, Pages 73-86, ISSN 0169-8095, http://dx.doi.org/10.1016/j.atmosres.2014.12.004. Chen, J., Zhao, C. S., Ma, N., and Yan, P.: Aerosol hygroscopicity parameter derived from the light scattering enhancement factor measurements in the North China Plain, Atmos. Chem. Phys., 14, 8105-8118, doi:10.5194/acp-14-8105-2014, 2014. Zhang, L., Sun,

J. Y., Shen, X. J., Zhang, Y. M., Che, H., Ma, Q. L., Zhang, Y. W., Zhang, X. Y., and Ogren, J. A.: Observations of relative humidity effects on aerosol light scattering in the Yangtze River Delta of China, Atmos. Chem. Phys., 15, 8439- 8454, doi:10.5194/acp-15-8439-2015, 2015.

(2) author’s response »> First, I will say thanks to reviewers for their kindly suggestions. In this report, I will reply to each issue as following in detail:

Response to Referee #1: The article “Profiling the PM2.5 mass concentration vertical distribution in the boundary layer” submitted by Z. Tao et al. for its review is a new technique. Previous results show that there is tied relationship between the PM2.5 mass concentration and AOD from the view of statistics. The authors want to match the aerosol extinction coefficient and PM2.5 and stress out the profile of PM2.5 based on a new CCD lidar technique. General comments: In my opinion, the study has a good presentation and has been clearly written. The scientific objective is important, and it is a fair study of a key issue that affects many other scientific works (such as the evaluation of air pollution and forecast).therefore I consider it to be suitable for its publication on Atmospheric Measuring Techniques journal. »> You are quite right. Thank you for your positive address. The CCD lidar technique is new issue for scientific use. We will continue this study.

Specific comments: 1ïijL’In the page 12934, line 9, should be “side-scatter lidar with a PM2.5 . . .” and line 14, the “characteristic” should be characteristics. 2ïijL’In the page 12935, line 1, “The PM2.5 poses great health risks. . .” is better. 3ïijL’In the page 12936, line 2, “. . .suitable to detect. . .” should be “. . .suitable for detecting..” and line 9, the results are. . . 4ïijL’In the page 12937, line 13, there are. . . 5ïijL’In the page 12940, line 16, “. . .is gotten. . .” and line 22, “. . .are shown. . .” 6ïijL’In the page 12941, line 3, “. . .in previous literature. . ., the Hefei site is situated in the east of China. . ..”, line 6, please use “And in Spring, dust aerosol. . .”, line 21, in the word “maximums”, “s” is unwanted, pls correct it through the article. 7ïijL’In the page 12942, line 3, “. . .a rich structure.” and line 7, “from 58% to 70%...” 8ïijL’In the page 12943, line 9, “At the meanwhile . . .”

should be “At the meantime . . . ” »> All of them are helpful to improve my paper, and I will correct them one by one.

Response to Referee #2: General comments: The manuscript presents a methodology and case study for profiling PM2.5 mass vertical distribution in the planetary boundary layer (PBL) using a CCD lidar and PM2.5 sampler. This subject is quite interesting to the air quality modeling and lidar remote sensing communities because there is lack of direct measurement of PM2.5 vertical profile by EPA and other agencies. The ratio of aerosol extinction to PM2.5 mass (or K) and its height dependence are critical in this study. Authors estimate the ratio K at the ground level with the RH<70% and then convert the aerosol extinction profile to PM2.5 mass profile in the PBL with an assumption of constant K. Three cases show a stable value of the ratio K over the nights, however the potential uncertainty or variation of the ratio K with altitude is not discussed except the RH effect. In addition, the validation on the CCD-lidar retrieved extinction or PM2.5 profile is not discussed or shown. The method might work in the well-mixed PBL where the aerosols are relatively homogenous. Overall, this manuscript is within the scope of Atmos. Meas. Tech. and can be accepted for publication once the following specific comments are taken into account.

»> You are right. Thanks for your positive suggestion. The subject is useful to the air quality modeling and lidar remote sensing communities. And it is a new technique and we try to make it more suitable for PM2.5 profile to fill in the lack. Of course, further investigation will be done in the near future. The following I will give the response to each point in detail.

Specific comments: 1. The potential uncertainty or bias of the CCD-lidar retrieved aerosol extinction is not discussed or presented. The validation of the CCD lidar retrievals with a Raman lidar or other ground in-situ measurement might be useful to evaluate the errors or bias of the CCD-lidar retrievals. »> Yes, it is very important. For concise purpose, we didn’t mention the details of the uncertainty as it has be described in our published paper in Applied Physics B wich is added into reference. We did the

validation of CCD lidar with two backscatter lidars. We add some sentences including errors and validation in this paper. The validation experiments and error analysis are shown in the reference [Tao et al., 2015]. When comparative experiments were performed, the C-lidar and V-lidar worked at the same position simultaneously, as well as another horizontal-pointing backscattering lidar (H-lidar). The result shown in the Fig. 2 of the reference [Tao et al., 2015] indicates a good agreement and the total relative error of extinction coefficient is less than 18% accordingly by applying the error propagation method and taking the typical example.

2. The height dependence or variation of the ratio of aerosol extinction-to-PM2.5 mass (or K) is critical to the accuracy of PM2.5 mass concentration profile. An assumption of homogenous microphysics (size distribution) and chemical compound (refractive index) of aerosols in this study is quite arbitrary for the night-time PBL due to the weak turbulence mixing or even in the day-time (see the varied aerosol sizes from the aircraft measurement by Li, et al., 2015). Thus, authors need to point out this limitation or/and show further discussions or evidences for this assumption. »> Yes, K may be the height dependent, but it is difficult to know the detail. Like lidar ratio for backscatter lidar, K is difficult to determine accurately for our case. Your suggestion is quite good, we will do it together with another multi-wavelength raman lidar system in the near future. And in this paper, I will add some sentences to point out it.

3. There are a few measurements of aerosol hygroscopic factor f(RH)of scattering coefficients in China (see the reference by Chen, et al., 2014; Zhang, et al., 2015). It seems that the f(RH) is in the range of 1.2-1.4 at RH=70% which is still big. »> Yes, you are right. The hygroscopic factor f at RH=70% may be about 1.2-1.4, but our cases presented in this paper, the retrieval K are approximate constant values. In the future, we will study the relationship between tke K and RH.

4. For the case results in this study, if possible, the relative humidity (RH) profile needs to be shown from the measurements from either radiosonde, Raman lidar or microwave radiometer. »> Yes, it is better to provide RH profile in the same time. In our case

results time, we did not detect the relative humidity profile.

Some minors: 1. Line-49, pleas add “in diameter” in the definition of the PM2.5. 2. Line-59, “were” should be “was”. 3. Line-117, what is the parameter “D”? 4. Lines 180-183, please add the reference for this point. 5. For the three cases, why is the distance D between the laser beam and CCD camera set to be different? 6. Line 171, Eq.(7) is an approximate formula in which Qext is assumed to be a constant. This is fine for a single particle, but for all different sizes of aerosols in the atmosphere Qext is a function of aerosol radius and refractive index. »> I will correct them one by one in my paper. As to Q5, The CCD camera is movable. When finishing the experiment, it will be carried back to the room. Based on our equipment, D could vary from 5 to 50 meters. For each experiment, the CCD camera is set arbitrarily, but the D was measured by a laser each time. As to Q6, Use the average or effective replaced the in the text.

References: Junxia Li, Yan Yin, Peiren Li, Zhanqing Li, Runjun Li, Maureen Cribb, Zipeng Dong, Fang Zhang, Jin Li, Gang Ren, Lijun Jin, Yiyu Li, Aircraft measurements of the vertical distribution and activation property of aerosol particles over the Loess Plateau in China, Atmospheric Research, Volume 155, 15 March 2015, Pages 73-86, ISSN 0169-8095, http://dx.doi.org/10.1016/j.atmosres.2014.12.004. Chen, J., Zhao, C. S., Ma, N., and Yan, P.: Aerosol hygroscopicity parameter derived from the light scattering enhancement factor measurements in the North China Plain, Atmos. Chem. Phys., 14, 8105-8118, doi:10.5194/acp-14-8105-2014, 2014. Zhang, L., Sun, J. Y., Shen, X. J., Zhang, Y. M., Che, H., Ma, Q. L., Zhang, Y. W., Zhang, X. Y., and Ogren, J. A.: Observations of relative humidity effects on aerosol light scattering in the Yangtze River Delta of China, Atmos. Chem. Phys., 15, 8439- 8454, doi:10.5194/acp-15-8439-2015, 2015.

(3) author’s changes in manuscript »> Some changes are listed in the following: 1: All grammars and words throughout the paper are revised according to reviewers’ comments and English speaker’s advices. 2: In the ‘3 Methodology’ part, “D is the distance from CCD camera to laser beam” is added in the paper, and “The result

shown in the Fig. 2 of the reference [Tao et al., 2015] indicates a good agreement and the total relative error of extinction coefficient is less than 18% accordingly in the error propagation method and by the typical example” is point out, and also “ is independent of altitude, though this assumption will lead to limitation” is added. 3: In ‘Acknowledgements’ part, “This research is supported by the National Natural Science Foundation of China (Nos. 41175021, 41305022 and 41590870), the Ministry of Science and Technology of China (No. 2013CB955802). We will also express our gratitude to all the anonymous reviewers for their constructive and insightful comments. ” the fund changes a little. 4: The detail changes will be shown in the red line manuscript.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/8/C5572/2016/amtd-8-C5572-2016-supplement.pdf

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 12933, 2015.