Dear Editor,

Manuscript number: egusphere-2026-1064

Title: Measurement report: Optical properties of carbonaceous aerosols modulated by source variations of spring haze

Many thanks to you and the referees for the valuable comments and suggestions. We have considered the points raised and revised our manuscript accordingly. Our detailed responses and relevant changes are presented below.

Comments from Reviewer #1

General comments

This study used the results of three black carbon instruments, AE33, PAX, and OCEC, combined with offline component analysis, to analyze the optical properties of black carbon and brown carbon. Some of the results are very interesting and important (i.e., Lines 252-265), and it is recommended to publish them after minor revisions.

Specific points

(1) Conclusion and Abstract: Both mention E_{abs}, but there is no introduction of E_{abs} in the main text, only MAC. It is suggested to add it, e.g., near Fig. 11.

Our responses: The introduction of E_{abs} was added to section 3.5 as suggested: "*the light absorption enhancement (E_{abs}) factors were estimated to be...*" **(see lines 561-562)**.

(2) Lines 45-47, 52-62: The effectiveness of China's air pollution policy is evident to all. However, it seems that the background content in this study is too lengthy. However, the 'various detection approaches' are difficult for readers. Suggest adding introductions comparing devices such as PAX, AE33, and OCEC.

Our responses: Thanks for the suggestion. In the revised manuscript, we shortened the descriptions of China's air pollution policy **(see lines 57-61)** and expanded the introduction to BC measurement approaches: "*...measured BC were frequently found to differ by several times among various detection approaches such as the thermal-optical (e.g., carbon analyzer), light absorption (e.g., Aethalometer) and laser-induced incandescence (e.g., single-particle soot photometer) ones. These techniques are mainly based on the high thermal-stability, strong light-absorbing, and refractory properties of black carbon, respectively, while none of them has been established as a*

reference method" **(see lines 46-51)**.

(3) Lines 116-117: First of all, I completely agree with your point of view: the default correction of AE33 seems too rough, but such important content, which is also the core of this article, should not be included in the attachment. Or at least it needs to be clarified in the text that PAX is a standard measurement used to compare with AE33.

Our responses: Thanks for the suggestion. We modified the descriptions in the Methods section and clarified that PAX was used as the reference method compared to AE33: "*While the in-situ approach PAX was used as the reference method for light absorption measurements, the AE33, which was operated with M8060 filter tapes, provided aerosol absorption coefficients at seven wavelengths...*" **(see lines 114-116)**. Given the importance of this point, we highlighted it again in the Conclusions section: "*PAX was used as the reference method to constrain the scattering-associated artifacts in the AE33-based absorption measurement*" **(see lines 577-578)**.

(4) Figure 1: Considering the stage comparison in the following text, it is recommended to clarify the time periods corresponding to different stages in this section. Maybe use different color.

Our responses: Thanks for the suggestion. Following this comment, we incorporated the filter sampling information, especially the segments corresponding to the four groups of samples (i.e., dust-impacted, fire&dust-impacted, fire-impacted and typical ones), into Figure 1. As can be seen from the updated figure below, for different group of samples (i.e., cases A–D), the distributions of their sampling segments were highly irregular during the spring campaign, suggesting dramatic variations of aerosol sources for the measurement period. We prefer to present these discussions, which were more strongly associated with section 3.2, after the definition of the four cases **(see lines 343-345 and Figure S5)**.

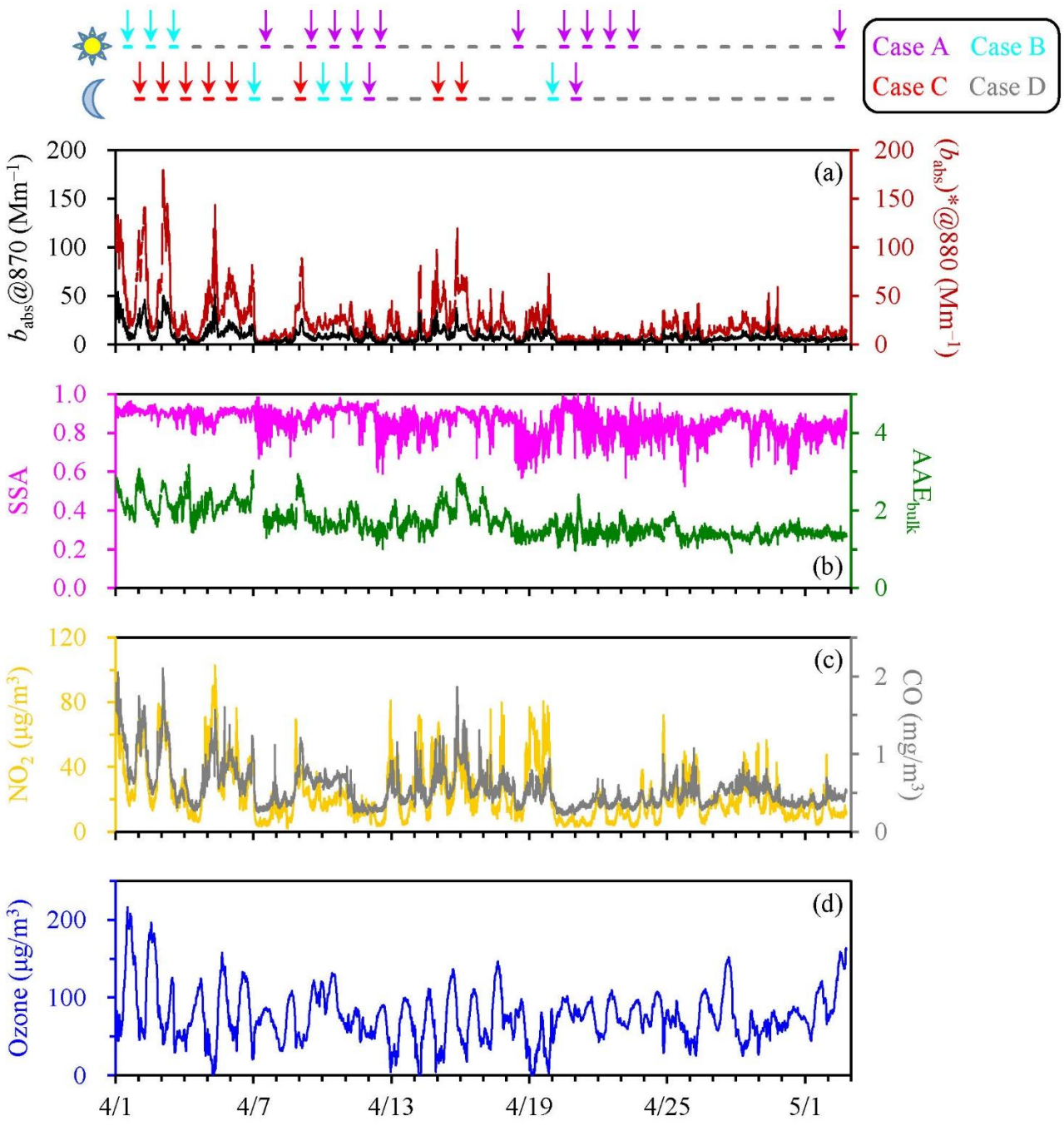

Figure R1. Distributions of the sampling segments for different groups of filter samples. Cases A to D correspond to the dust-impacted, fire&dust-impacted, fire-impacted and typical samples, respectively. The segments are color-coded by sample types, while for the distinct episodes (cases A to C), the samples in each group are also highlighted by the arrows. The sun and moon symbols indicate the daytime and nighttime samples, respectively. The on-line measurement results are also shown, with (a) for the PAX-based b_{abs} at 870 nm and the AE33-based $(b_{abs})^*$ at 880 nm, (b) for the PAX-based SSA and the AE33-based AAE_{bulk}, (c) for NO_2 and CO, and (d) for ozone. This figure was presented as Figure S5 in the revised manuscript.

(5) Table 1: Have you considered conducting machine learning simulations on C^* in your future work?

Our responses: Thanks for the suggestion. We will try to predict C^* using machine learning in future studies, e.g., by linking the C^* vs. SSA relationship and the aerosol composition vs. SSA relationship.

(6) Lines 195-196: Quartz film is a half day sample, so how to compare AE33 with offline needs to be introduced.

Our responses: This point was clarified as suggested: "*The time-resolved $(b_{abs})^*$@632 results were first averaged for the filter sampling segments and then compared to the off-line $(b_{abs})^{\#}$@632*" **(see lines 209-211)**.

(7) Lines 203-210: It is not clear for readers to understand why to discuss filter tapes?

Our responses: Here we found that the off-line carbon analyzer always reported higher light absorption coefficient values compared to the on-line AE33. The comparison was performed at the same wavelength (i.e., 632 nm; **see lines 206-209**) and as explained in our response to the previous comment (#6), the difference in measurement resolution had been accounted for **(see lines 209-211)**. Thus we think the most likely cause for the observed carbon analyzer vs. AE33 discrepancy was the difference in filter media used by the two instruments (quartz filter vs. M8060 filter tape). The discussions above were incorporated into the revised manuscript: "*As the differences in measurement wavelength and time resolution had been accounted for, a likely cause for the observed discrepancies was the difference in filter media used by the two instruments*" **(see lines 213-215)**. We think the related contents could be easier to follow after the revisions.

Comments from Reviewer #2

General comments

The manuscript by Cheng et al. investigated the optical properties of carbonaceous aerosols under changeable conditions of haze pollution in Northeast China. For this purpose, the authors combined on-line data (from a PAX, an AE33 and analyzers for several gaseous pollutants) and off-line results (from a high-volume $PM_{2.5}$ sampler) measured during April, 2023, when different types of haze episodes were encountered. They first explained the discrepancies between the light absorption results from different methods, and then discussed the influences of agricultural fire emissions and secondary aerosol formation on the optical properties of carbonaceous aerosols. In general, the results and discussions were scientifically sound. Given that the studied region is distinct (especially regarding the agricultural fires) but largely overlooked by previous studies, the manuscript is suitable for a measurement-report paper in ACP. My overall assessment is that it could be considered for publication after the following concerns were properly addressed.

Major points

(1) Lines 22-25. The two sentences were not well connected. It is completely unclear why the dust episodes need to be excluded.

Our responses: A new sentence was added to explain the exclusion of the dust episodes: "*BC concentrations were also overestimated for the dust episodes*" **(see lines 24-25)**.

(2) Lines 56-62. The new standard has been released. The statements need to be revised correspondingly.

Our responses: The statements were updated as suggested: "*More recently, the Ministry of Ecology and Environment (MEE) of China released stricter Ambient Air Quality Standards for $PM_{2.5}$ (MEE, 2026), e.g., starting from 2031, the Class 2 standards will be reduced to 25 from 35 μg/m^3 for annual average (with an interim limit of 30 μg/m^3 for 2026–2030) and to 50 from 75 μg/m^3 for 24-hour average (with an interim limit of 60 μg/m^3). The new standards provide additional impetus for air quality improvement...*" **(see lines 61-70)**.

(3) Table 1. Fractions in total data points should also be given for the SSA bins.

Our responses: The fractions were added as suggested **(see Table 1 in Page 12 of the**

revised manuscript):

Table 1. SSA-dependent *C** results determined during the spring campaign.

SSA range	0.50–0.60	0.60–0.65	0.65–0.70	0.70–0.75	0.75–0.80	0.80–0.85	0.85–0.90	0.90–0.95	0.95–1.00
Median *C**	1.24	1.65	2.06	2.24	2.26	2.46	2.76	3.06	3.27
Lower quartile of *C**	0.90	1.34	1.66	1.78	1.89	2.17	2.46	2.72	2.91
Upper quartile of *C**	1.35	2.07	2.40	2.53	2.62	2.77	3.07	3.40	3.92
Fraction in total data points (%)	0.03	0.12	1.37	4.40	11.89	24.73	30.30	26.53	0.64

Minor points

(1) Lines 12-13. Suggest replacing "Northeast China" by a more specific term, e.g., the site type (urban, suburban, rural, etc.) would be more useful.

Our responses: The change was made as suggested: "*a field campaign was conducted in a Chinese megacity during a spring season*" **(see lines 12-13)**.

(2) Lines 37-41. The sentence is difficult to follow. Revise it.

Our responses: The sentence was revised to: "*as indicated by the difficulties in proper predictions of several key fractions including primary OA emitted by open burning...*" **(see lines 38-40)**.

(3) Line 70. I guess the average temperature was for winter. Clarify it.

Our responses: This point was clarified as suggested: "*with a seasonal average of –15 ºC in Harbin's winter*" **(see line 79)**.

(4) Line 75. It is better to use "emission system".

Our responses: The change was made as suggested **(see line 84)**.

(5) Line 84. It should be "secondary".

Our responses: The mistake was corrected **(see line 93)**.

(6) Line 119. Suggest using "over" instead of "in".

Our responses: The change was made as suggested **(see line 129)**.

(7) Line 169. It should be "obtained".

Our responses: The mistake was corrected **(see line 179)**.

(8) Line 217. Suggest using "different types of filters".

Our responses: The change was made as suggested **(see lines 229-230)**.

(9) Line 219. Move "heavily loaded" to the next sentence, as the detailed carbon loadings had been given here.

Our responses: The change was made as suggested **(see lines 231 and 234)**.

(10) Lines 220-221. I think $PM_{2.5}$ concentrations were not highly relevant to the statement here, i.e., in addition to $PM_{2.5}$ concentration, filter loading also depends on the sampling volume or, more precisely, face velocity.

Our responses: Thanks for the suggestion. ATN levels, which are independent of face velocity, were presented instead of $PM_{2.5}$ concentrations in the revised version: "*...showed the highest carbon loadings (above 115 μgC/cm^2 for the sum of OC and EC) and ATN levels (exceeding ~2.5) throughout the campaign*" **(see lines 232-234)**.

(11) Lines 444-445. It is better to explain (b_{abs})@370 first, which is the basis for the calculation of $(b_{abs})_{BrC}$@370.

Our responses: The change was made as suggested **(see lines 459-461)**.

(12) Line 453. Change "used" to "considered as".

Our responses: The change was made as suggested **(see line 469)**.

(13) Line 462. It is unnecessary to define the ratio again.

Our responses: The duplicated definition was removed as suggested **(see line 478)**.

(14) Lines 545-546. "E_{abs}" should be introduced, as it was mentioned in the abstract. Accordingly, it is unnecessary to define it again in Line 596.

Our responses: E_{abs} was introduced as suggested: "*the light absorption enhancement (E_{abs}) factors were estimated to be...*" **(see lines 561-562)**. Correspondingly, the duplicated definition for E_{abs} in the Conclusions section was removed **(see line 613)**.

(15) Line 561. To my understanding, the C^* factor was used to correct for the overall effect of the multiple scattering by the filter media and the scattering by the collected

particles. Clarify it.

Our responses: Yes, C^* was used to account for the overall effect of the multiple scattering by the filter media and the scattering by the collected particles. The sentence was changed to: "*PAX was used as the reference method to constrain the scattering-associated artifacts in the AE33-based absorption measurement*" **(see lines 577-578)**.

(16) Line 600. Remove the comma before "and effectively".

Our responses: The change was made as suggested **(see line 617)**.

Again, we thank the referees very much for their valuable comments and suggestions.

Sincerely yours,
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