Response to Reviewer comments.

We sincerely appreciate the reviewers' comments; they were very constructive and aimed at increasing the impact of this work while ensuring no useful information was overlooked. Every comment has been addressed separately.

Based on the reviewers' feedback, we also added new sections and performed additional analyses, including:

- Assessing the impact of **Car-Free Day** as both a cause of reduced emissions on the day itself and during the enforcement period across all four seasons. This analysis focused on PMF sources, PM chemical compositions, absolute PM_1 concentrations (total PM_1 + BC), and $PM_{2.5}$ measured at the U.S. Embassy in Kigali. We examined seasonal trends, diurnal variability, and annual means to assess the possible shift in the PM sources, composition
- Conducting a similar analysis for the **Community Work Initiative (Umuganda)**, which occurs monthly in the same city.

However, given the large number of parameters analyzed (Org, SO_4, NO_3, NH_4, Chl, BBOA, HOA, OOA, BC, BC_bb, BC_ff) and the types of analyses performed (diurnal, seasonal, and annual comparisons), we only reported the most fluctuating parameters in Section V of the Supplementary Information and in the main manuscript.

In addition, we expanded the text with more paragraphs and sections to ensure that no important information was omitted.

Finally, we would like to clarify that the reference lines (e.g., L100) refer to the version of the document with track changes, not the cleaned version.

Responses are given in blue color for differentiation.

Reviewer 1:

Main Concerns/Questions:

1. It was not mentioned until the end of the paper that Kigali has several initiatives to reduce the amount of pollution in the city. One of these is "car-free-days" and data for this paper has not been parsed for car-free Sundays/regular Sundays nor for working day/non-working day trends. There is a missed opportunity here to compare the current data set more directly with the 2017 and 2018 PM2.5 mass and BC values reported in Subramanian et al. (2020) and a newer paper by Kalisa et al. (2025, 10.1080/23748834.2025.2468017) that had additional data covering the time period through COVID-19 partial lockdown days (through June 2020). The data reported in the current paper can provide more recent information on how the car-free-days project is working with respect to presumably more emissions than in 2017-2019 with extra information on how the PM composition changes.

Many questions arise relating to these comparisons. For example, has the BC/PM2.5 mass changed appreciably? Are the seasonal differences now greater or not as large as they were in the (pre-COVID-19) past. Are there differences in the relative contributions of BC from fossil fuels compared to BB from biomass burning on the cleaner days (either car-free or non-working day)? What other compositional differences (or similarities) are there for cleaner days? I suggest making an additional figure for the main manuscript, showing speciated changes (HOA, BBOA, OOA, BC_ff, BC_bb, NO3, SO4, and NH4) in different panels as a function of time-of-day for car-free Sundays/regular Sundays. Each plot should have averages with standard deviations, like Figure 6a in Subramanian et al. (2020). These might need to be separated by (combined long and short) wet or dry seasons. I urge the authors to expand on some of these and potentially other comparisons.

Thank you for the comments, we discussed the car free day and community work (Umuganda) initiative and have responded using the comment number 22. The findings were added in the main document from L 528 to L 543

2. There are clearly AI-generated images and some associated text in paper and SI that do not make sense. Per the Copernicus publication manuscript preparation webpage, "Should you have used AI tools to generate (parts of) your manuscript, please describe the usage either in the Methods section or the Acknowledgements." Furthermore, ALL AI-generated content should be checked for clarity and errors before submitting manuscripts with it.

Thank you for the observation, the manuscript does not use any AI generate plot, the map from the google earth tool was updated(Figure S1 in SI document) and we included the latitude and longitude and added the distance between the PM composition measuring site and the Meteo ~~statation~~station as well as the .PM2.5 measuring station.

Minor Concerns/Questions:

1. Because more PM research is being published on Eastern Africa, consider changing title to say "Kigali, Rwanda" instead of "Eastern Africa"

Thank you for the comment, the title was changed and as proposed.

2. Correct "slop" to "slope" everywhere.

Thank you, the typos error was corrected, and the word slope was changed to the Slop at L 174 and L 378 in the manuscript.

3. Abstract: Include standard deviations of the averages mentioned here and throughout.

Thank you very much, the SD was added in the abstract L 20.

4. L 65-71: Add here that while there is some PM composition data from the Rwanda Climate Observatory (Kigaro et al., 2022), a remote area outside of Kigali, and measurement of BC

(Subramanian et al., 2020; Kalisa et al., 2018, 2025), and PAHs, and NPAHs (Kalisa et al., 2018), no detailed PM composition data have been reported for the city itself.

Thank you. The section has been added along with all the references. The revised text is as follows:

"Data on PM composition and sources are scarce. ~~While~~ Some composition information comes from the Rwanda Climate Observatory, a remote site outside Kigali (Kirago et al., 2022). Within Kigali, studies have reported BC, PAHs, and NPAHs (Kalisa et al., 2018, 2025; Subramanian et al., 2020; Kalisa and Adams, 2022), but no detailed PM composition data have been reported for the city itself. From L70 TO L74

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5. L 99-101: What defines "short" and "long" seasons? The number of months for each are the same.

"The terms 'short' and 'long' refer to the intensity and consistency of rainfall and sunshine, rather than their duration." The reference were added L 119 -L121

6. L 101-103: The met values described here are somewhat inconsistent with the values shown in Figure S2a. This figure appears to be AI-generated and needs to be corrected. I'm a bit skeptical of the small bands for the standard deviations of the measurements shown. In particular, the relative humidity standard deviation of the mean and rainfall amounts should indicate that the relative humidity extends up to 100% at times during the wet seasons. I suggest breaking Figure S2a into averages for each season, and refining the seasons by examining the weekly precipitation averages. Furthermore, the solar radiation averages in Figure S2a are unrealistically too low for this location and the legend for solar radiation has the incorrect units. Since its importance is mentioned in interpreting time-of-day patterns later, is there any met data on boundary layer height that could be added, even if it's from a different location like the airport?

The plot was updated to the weekly average with the error bar representing the standard deviation from the mean, the precipitation and wind speed are multiplied by 10 for better representations. We also emphasis that the plot was not AI generated. The unit of the solar radiation was also corrected and mentioned in the caption of Figure S2 in the SI. We understand the importance of PBLH in the explanation of the changes in concentration; unfortunately, we did not find any PBLH data set in this area.

7. L 104-109: How do motorcycles and construction activities contribute to vehicle (fossil fuel) emission sources?

We thank the reviewer for the comment. In this context, we were referring to off-road machinery and trucks used at construction sites, as well as tailpipe emissions from twowheeled motorcycles. To improve clarity, the manuscript has been revised, and the following text has been added at L98:

"...leading to an increase in traffic and frequent traffic congestion countrywide, with tailpipe emissions contributing about 83% of the total PM mass in Kigali (REMA, 2018b)."

8. L 132: Correct the wavelength listed for the Magee AE33 from one value (880 nm) to the value range for this instrument. Later in that paragraph, two wavelengths (950 and 470 nm) are used to estimate the fraction of biomass burning black carbon. Are these two endpoints of the wavelength range for this instrument? Please clarify.

We thank the reviewer for highlighting this point. A clarification has been added, and the sentence at L149–L150 has been updated as follows:

"Ambient black carbon (BC) mass concentration was measured using a seven-wavelength (370nm, 470nm, 520nm, 590nm, 660nm, 880nm, and 950 nm) aethalometer model AE33(Magee Scientific, 2017)~~(Scientific, 2017)~~"

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Field Code Changed

L 158: Figure S3b shows SMPS data. This instrument was not mentioned earlier. Why is the ACSM+BC mass so much higher than the SMPS total?

We thank the reviewer for this observation. The SMPS (comprising the CPC Model 377 and Classifier 880) was used to perform relative ionization efficiency calibration for the ACSM. One possible reason for the lower particle concentrations measured by the SMPS is its limited detection range (10 nm to 440 nm), which may exclude larger particles. In response to this comment, the original figure was removed, and the caption was updated to:

"Figure S3: Comparison of PM_1 (NR_PM_1 + BC) with $PM_{2.5}$ measured at the US Embassy located 6 km from the sampling site."

9. L 174: add "the speciated" in front of PM1

Thank you, this was updated at L 194

10. L 178-182: Clarify where these measurements were made (urban road or urban background?).

Thank you, this was updated in section L 200

11. L 183-189: Are the absolute values of the various species measured in the remote area different from the absolute values measured in this study?

We thank the reviewer for the comment. Yes, the absolute concentrations are higher at this urban site compared to those measured at the Rwanda Climate Observatory (RCO). The comparisons. were added at the L 426

12. L 196: add "see Figure S4". It is interesting that the biggest change in the relative composition is for the long-rainy season where there is relatively more black carbon and less organic carbon. Might want to add an investigation of the BC sources as a function of season to the source apportionment section.

The pointer was added at L 198. However the investigation of the source of the BC in the rainy season was no performed!

13. L 208 (and Figure 2): consider replacing Figure 2 (annually average data) with the four seasonally average plots shown in Figure S4.

Thank you for the suggestion. Figures were swapped.

14. L 229-231: If Figure 2 is replaced with Figure S4, these numbers might need to be adjusted.
15. Thank you for the suggestion. The numbers did not change however we directed the reader to the plot9 plot 4 with the numbers in the test. As the plots were interchange the discussion were extended to reflect the seasonal variation rather than the overall mean variations. L255-l265.

16. L 243: Isn't there sulfur in the diesel and gasoline fuel (L 107)?

Thank you for your comment. This is what the measured data are saying!

17. L 247: Why is this eruption mentioned when it occurred 2 years before these measurements? It's unlikely that the high amounts of SO2 emitted from the main eruption remained in the atmosphere over this period. Is there evidence of continuing SO2 emissions from the area?

Thank you for pointing this out. However, we showed this to demonstrate to the reader that the source measured here is the background, and there is no potential source of SO_4 in the region. The volcanic eruption, is showed through it happened two years ago, however that is and active volcano that is potentially emitting the SO4 in atmosphere, which could be assumed as a potential source contributing to SO_4 variations though for the current period is not erupting however as an active volcano might degassing at certain rate that may contributing to increase in sulfate in the region but not at our site.

18. L 252 and following (Section 3.3): I suggest reorganizing this section significantly. An average mass spectrum covering an entire year is not particularly useful to discuss in the main part of the paper (nor SI) and could be omitted. However, mentioning the significance of the tracer ions is useful and should probably be moved up to the data analysis section 2.3 (right before or after PMF there). The nitrate attribution calculation (Eqn. 1) could also be moved up to the data analysis section and the discussion of it could be moved up to section 3.2. Since PMF analysis was performed on this dataset, the tracer ions do not provide additional information, and I suggest consolidating the relevant parts of the tracer ion discussion with the PMF results/discussion. The current Figure 3 could be moved into the SI.

We thank the reviewer for the valuable comment and suggestions. We fully acknowledge the importance of streamlining the discussion. However, since this is the first mass spectrometry dataset reported for this region, we consider the discussion of tracer ions and the annual average mass spectrum to be valuable in providing context and guiding readers toward understanding the PMF analysis. Therefore, we believe that retaining the current structure of this section, including the tracer ion and mass spectrum discussions, will be more informative for readers unfamiliar with such data in this region.

19. L 280: Include the values of R(NH4NO3) from this dataset and the calculated R(OrgNO3) based on the ratio-of-ratios. One other thing to mention in the data analysis section about "NO3" with unit mass resolution (UMR) aerosol mass spectrometers there is potentially organic (non-nitrate) contribution to m/z 30 from the CH2O+ ion. Contributions of CH2O+ at m/z 30 could inflate both the UMR "NO3" mass concentrations and the estimation of fOrgNO3 from either approach discussed in this paper. It could potentially be especially problematic when the organic signals from other ions in the rest of spectra are relatively large. Unfortunately, the contribution of CH2O+ ion at m/z 30 cannot be corrected for the UMR data here, but it could be discussed as a potential positive bias for both "nitrate" and fOrgNO3.

Thank you for the suggestion, the values were added into the main manuscript at the L 330. And this section is updated in the main manuscript:" Noting that the Q-ACSM is a unit mass resolution (UMR) instrument, there is a potential for interference at m/z 30 from organic fragments not necessarily related to nitrate, such as CH_2O^+. These contributions cannot be resolved in the case of a UMR instrument, potentially resulting in a bias in the estimation of the total fraction of nitrate (f_{OrgNO_3})." To highlight this potential bias.

20. L 380: The correlation plots are shown in both Figure S10 and Figure 4a (insets). Suggest removing them from Figure 4 because they are too small and are already shown in Figure S10.

Thank you for the suggestion, we believe the scatter plot are not very small for the reader, we prefer to keep them in the report to help the reader having inference on the spectra significance. Thanks to the review to suggest adding molecular ions on the dominant peaks, the the ions were added for more clarification and simplicity.

L 385-387 (and last line of the abstract): Is this statement valid? There seems to be a significant change in the contributions for each of the factors over the different seasons, with relatively more OOA and less HOA during the dry seasons than during the wet seasons. This result coupled with the relative composition changes shown in Figure S4 seem to indicate that there are source/sink differences between the seasons. Maybe wet deposition removes more OOA than HOA and more OA than BC? Or maybe there is more relative production/emission of OOA (and OA) than HOA (or BC) during the dry season?

Thank you for the comment, using the ratio of the dry/rainy season composition fractions , we have 1.15 for OOA(driven by chemical production, as you mentioned and as we concluded), 0.97 for HOA and 0.82 for BBOA (almost constant, this makes sense for un urban background site where we do not expect the change in activity at this site(HOA sources especially tailpipe emission from the diesel, gasoline vehicles and generators) either in the wet or dry season, We noted that the wet season in Kigali is not like the winter in USA where the activities stopes). Q hope that the 18-20% difference between all OOA, SBBOA and HOA, can be defined by removal process in the wet season that this is where we inferred that PM1 is more driven by deposition and chemical production rather than change in primary emission.

We updated the main manuscript, and the following sentence were adjusted. To

"This suggests that seasonal changes in PM_1 mass in Kigali are primarily driven by deposition and chemical production rather than shifts in primary" at the end of abstract and at L 432

21. L 393 and following: Like the swap between Figure 2 and Figure S4, the time-of-day trends should be shown for each season rather than an annual average shown in Figure 5. It would be good to have the relative contributions for each season as a function of time-of-day presented as well, similar to Figure 3b, but again separated for each season rather than an annual average. Separating them out by season would provide additional information on the seasonal differences.

Thank you for the suggestion. The suggested plot was added and the absolute mass diurnal plot in each season were also added in the SI(Figure S13) for more details and the discussions were also updated. L483-490

22. L 393 and following: This is also the main section that I think could be expanded to a comparison of car-free Sundays/regular Sundays or working days/non-working days.

There might be something interesting in the sources of BC and HOA/BBOA/OOA (and perhaps the other aerosol components) in these comparisons. I suggest an additional figure for the main manuscript, showing speciated changes (HOA, BBOA, OOA, BC_ff, BC_bb, NO3, SO4, and NH4) in different panels as a function of time-of-day for car-free Sundays/regular Sundays. Each plot should have averages with standard deviations, like Figure 6a in Subramanian et al. (2020). These might need to be separated by (combined long and short) wet or dry seasons.

Thank you for the suggestion, the plot and discussion were made and added as section in the SI. The Car free day and community work findings were further discussed in the conclusion section to highlight the impact of different initiatives on the air pollution reduction.

L 426-430 (and Figures S9-S11): These sentences need clarification. Anywhere the text says “This versus That”, “This” is expected to be on the y-axis and “That” is expected to be on the x-axis. Note that the information shown in Figure S10 is the factor correlations with the reference spectra, already mentioned in L 380. Figure S11 is essentially showing the fraction of those ions in the factor spectrum. It is not surprising that they are so well-correlated because the factors are defined with a specific ratio for each of those ions. These ratios are the “fractions of ion signals in the organic spectra” aka f44, f57, and f60. From the factor spectra in Figure 4, they appear to be around 0.095 for f57 in HOA, 0.035 for f60 in BBOA, and 0.24 for f44 in OOA. It would be good to note what those values are when first discussing the factors (L 368-374) and maybe discuss their significance. Figure S9 is a bit troublesome because the r-squared values make the correlations seem to be better than they appear in the plots. Glancing at the plots without looking at the r-squared values makes it appear that BC_bb is linearly associated with all three factors, whereas BB_ff does not appear to be linearly associated with BBOA or OOA and only loosely associated with HOA. Perhaps they would look better if the marker sizes were smaller? The words in the text do not seem to effectively convey what these correlation plots seem to be showing.

Thank you for your observation and your opinions, the captions were adjusted as per the proposal and. The significance of the mass fraction of the tracer ions in each spectrum were discussed from L425 to L437 in the main document.

And the paragraph were adjusted to “The mass spectrum of HOA is characterized by the pronounced hydrocarbon ion series of C_nH_{2n+1} and CnH2n-1 consistent with primary emissions from traffic and fossil fuel combustion (He *et al.*, 2010). Important peaks in this mass spectrum include *m/z* 41, 43, 55, 57, 69, and 71With f57 exhibiting a normalized signal fraction of 0.095, further supporting the assignment of this factor to primary hydrocarbon sources. In the BBOA mass spectrum, the most prominent peak is at m/z 29 with an abundance mass fraction of 0.17, likely indicating small oxygenated fragments associated with the incomplete combustion of biomass, with other significant peaks at m/z 60 (fraction 0.035) is a widely recognized tracer for levoglucosan-like compounds, supporting the biomass burning origin Other contributing ions include m/z 55 and 43 (0.08), reflecting mixed hydrocarbon and oxygenated fragments. The OOA mass spectrum is characterized by the large abundance of m/z 44 with a mass fraction of 0.24, indicative of aged, highly oxidized organic material dominated by CO_2^+ fragments. This ion is a well-established marker for secondary organic aerosol (SOA) formation(Matthew, Middlebrook and Onasch, 2008; Zhang *et al.*, 2011).“

The glancing on the figure is more likely to the point at most right corner that are not colletinmg hence reduces the R_2 values.

L 431-440: It is unclear what current results are intended to be compared with these other studies, which indicate that there was a fraction of the total PM2.5 attributed to traffic. Where is that result for the current manuscript? 32% of OA is HOA (annually averaged, L 382) and 59% of BC is BC-ff (annually averaged, L 409). Depending on how the contributions from traffic were determined from the other studies, the first sentence of these paragraphs should mention how the fraction of PM2.5 was determed to be the traffic contribution (is it HOA+BC_ff divided by PM2.5 mass?) and provide comparable seasonal values for the current study. What about the SOA from traffic?

Thank for the clarification on the approach, the paragraph is refined to: “ L517 While there is limited data on air pollution sources in other East African countries, our estimate that traffic emissions (HOA + BC_ff) contribute approximately 34–38% of Organic PM1 is consistent with past filter-based source apportionment studies in the region.”

23. L 441-466: How many of these policies are currently in effect for Kigali? As mentioned in the main concerns, it would be useful to have comparisons with prior studies in Kigali and assess any progress with these policies. Is the reduction in PM2.5 better/worse from those policies now than presented in the Subramanian et al. (2020) paper? Emissions everywhere have presumably increased since then.

Thank you for highlighting this point. All the policies mentioned above are currently in effect in Rwanda/Kigali. Unfortunately, there is a limited number of studies on source apportionment in the region. The only available data prior to the implementation of the referenced policies in this city is the emission inventory conducted by REMA in 2018. Noting that the inventory uses the estimates based on the primary emission and activities , there is no source apportionment studies at the same site/city, we were unable to do any comparison.

24. L 496-499: This is different from what is said at the end of the abstract, which should probably be revised.

Thank you, the abstract was revised to reflect the finding on L496-499.

25. L 501-510: This is somewhat inconsistent with first paragraph in the conclusion (L 469-472), because the results of this study indicate that pollution in Kigali is local (primary from vehicles and cooking) and is not related to transboundary issues. A discussion should be added to the end describing how well the current pollution control policies are working for the city.

A section on this discussion were added and the conclusion were updated.

"

26. Figure 1 and others: The font for the legends are too small.

Addressed thanks

27. Figure 2: Suggested above to replace it with Figure S4a.

    done

28. Figure 3: Suggested above to move it to the SI.

    We hope keeping the figure in the main section is ok!

29. Figure 4: Suggested above to delete the correlation plots in part a. Label the important ions in part a. The shading in part b makes it look like there isn't any HOA during the rainy season. Consider another way to distinguish the different seasons. The legend obscures a large part of the plot.

Thank you for the comment. The plots have been updated, and the correlation subplots was kept hopping that they are helpful for reader. Additionally, the shading colors in Figure 4 have been improved to enhance visual clarity and interpretation.

30. Figure 5: Use consistent colors for Figures 4 & 5. Since data like this from the study are used to estimate source apportionment, it would be interesting to see if there are seasonal contrasts (or not) as a function of time-of-day. Suggest replacing the current figure with a new 4-panel plot of that. Suggest adding a corresponding figure to the SI examining the mass fractions of the (OA+BC) from HOA, BBOA, OOA, BC-ff, and BC-bb for each season as a function of time-of-day.

Thank you for the suggestion, the recommended plots were made and are added in the main document a normalized masses contribution of the different sources were added in the main document. Addition discussions were added to highlight the relative contribution of the individual sources

31. Section 9 (References): Check for multiple listings of the same paper (e.g. Andersson et al. 2020 and Subramanian et al. 2020). Note that the Subramanian et al. 2020 doi number is incorrect. The correct number is 10.17159/caj/2020/30/2.8023.

Fixed

32. Figure S1: AI-generated. Remove labels for businesses on map and in legend. What is the significance of the pin? It would be more appropriate to show locations of the major highways along with the other locations mentioned in the main manuscript, such as the location of the met data (Kivugiza meteorological station) and the PM2.5 data (US Embassy). How far out does the metropolitan area extend? Could have a second map to show that.

Thank you for the comments, the plot was updated and we showed the lat, long and all the sampling sites in the study figure S1.

33. Organic Nitrate Section in SI needs to be re-written more clearly. For example, Eqn. 1 is for the organic nitrate mass (preceding sentence says fraction) and Eqn. 2 is for the organic nitrate fraction of the total nitrate mass (preceding sentence says particulate organic nitrate). Also, please use the same abbreviations/definitions for the variables as used in the main paper, so that Eqn. 2 is identical to Eqn. 1 in the main paper. Because the variables were ambiguous, I was unable to determine what was being plotted in Figure S5.

The figure encompasses both mass and fractions, the caption was revised to avoid confusion.

34. Other sections of the SI also need to be clarified and corrected for errors in the AI-generated content.

This was done and no AI generated plots were used in the document

Reviewer 2:

The authors present aerosol chemical composition data in Kigali Rwanda using a Q-ACSM and a BC aethalometer over a 12 month period. This is an impressive dataset in a critically understudied location. The scientific results are robust, though the writing, formatting, and proofreading could use some improvement. I recommend publication subject to what are largely minor revisions.

My only major comment is that the paper aims to be representative of all of East Africa; however, then spends a lot of time focusing in specifically on one location in Kigali, and attributing results to local behaviors in Kigali. East Africa is a large, heterogenous place, with larger cities than Kigali. The authors should spend some effort justifying why they think these results are representative of broader East Africa. A few sentences to a paragraph should be sufficient.

Thank you for the comment, to be more realistic, the title was changed to "**Chemical characterization and source apportionment of fine particulate matter in Kigali, Rwanda using aerosol mass spectrometry.**

”

Minor comments:

Abstract: The abstract ends kind of abruptly after a detailed result about seasonality. Consider adding some kind of broader concluding statement. I also don’t quite follow how the pretty small seasonal variation in the SOA/POA split (47/53 wet and 59/41 dry) suggests that (wet) deposition is driving PM mass changes? The differences between dry and wet aren’t very large. Maybe this is clarified further in the paper, but the abstract should be internally consistent as well.

The abstract was updated, and the general conclusion was added

Intro: There appear to be some citation issues (“n.d.” for no date after a date is given).

Thank you for the observations, we updated all the citations

There is also some repetition in the intro. For instance, line 65 says “There is limited information on PM composition and sources in East Africa.” But line 48-49 already makes this point.

Thank you for highlighting this repetition. We have revised the introduction to remove redundant statements and avoid repeating the same information.

Methods: the total PM2.5 mass data being 10 km away from the ACSM and BC sounds like a significant uncertainty. Especially in a heterogeneous city like many growing cities in Africa. How does this impact the comparisons? Also, clarify in the text if the Rwanda Meto station is at a third location and how far that is from everything else?

Thank you for this valuable comment. Indeed, there are limited PM measurement sites in Kigali, which poses a challenge for direct comparisons. We acknowledge that the ~6 km distance between the ACSM/BC sampling site and the US Embassy $PM_{2.5}$ monitor introduces uncertainty, particularly in a heterogeneous urban environment like Kigali. However, this comparison was included primarily for QA/QC purposes to assess the performance of the ACSM and to support calibration validating RIE calibration.

We have now verified and updated the exact distance between the sites in the manuscript. Additionally, we have clarified that the Rwanda Meteorology station is located at a third site, approximately 4 km from the ACSM station.

Does the aethalometer AE-33 have a 1 µm cut point PM cyclone as well? If the NR-PM1 is simply added to the BC from the AE-33 without a PM1 cyclone, the assumption would be that all BC is 1 µm or less. Not necessarily a bad assumption, but it should be cited/documented if so.

Thank you for your comment. We confirm that the BC was equipped with a 1 µm cyclone(L195) on the inlet. For clarity, we have updated the main document to explicitly state that both instruments utilize the same inlet configuration. (L215- L220)

Results: Line 217 and several other parts of section 3.2 mention a PBL height impact. Is this a result? If so, provide the data that supports it. If it is a general assumption add a citation.

This is a general assumption, and the references were added. Thank you very much.

Line 245: Regarding the discussion of SO2 or sulfate sources. What about vehicle fuel? Any diesel being used in Kigali?

Thank you for your comment. Vehicle fuel as a potential source of SO_2 emissions was discussed earlier in the discussion section, where we noted that vehicles in the region often use high-sulfur fuels (50 ppm sulfur for diesel and 100 ppm sulfur for gasoline, as is common in most Eastern African countries). However, due to the lack of data on the vehicle fleet composition and vehicle density in Kigali, we did not perform a quantitative analysis of this source.

Line 385. Related to the final line of the abstract, I think the reasoning for why the lack of seasonal variation implies that rain (wet deposition) is driving composition changes. As written it is a bit unclear.

Thank you for your comment. We have revised the abstract to clarify the reasoning behind the link between the lack of seasonal variation and the influence of wet deposition on composition changes. In addition, we have refined the discussion section to provide a clearer explanation of this relationship.