Dear Prof. Dr. Andreas Petzold:

Hereby we resubmit manuscript “**Aerosol optical properties within the atmospheric boundary layer predicted from ground-based observations compared to Raman lidar retrievals during RITA-2021”** after major revisions. Based on the valuable suggestions of the reviewers, we performed extra data analysis of the lidar data, namely a retrieval of relative humidity profiles and an overlap correction. Particularly the overlap correction significantly improved the comparison of the retrieved lidar ratio with the predicted lidar ratio. In addition, we have made extensive revisions of the manuscript (especially abstract, figures, and supporting materials) and we added more detailed explanations to the method section.

In contrast to reviewer 1, we do believe it is worthwhile to combine in-situ and remote sensing measurements and such studies can be valuable for both communities. The main result, namely that estimates of lidar ratios based on routine ground-based measurements and simple assumptions are quite representative of the lidar ratios higher up, can find wider applications. Especially, since our test cases in the evening hours constitute a difficult scenario with usually poor mixing and complicated vertical layering, we would expect even better predictions during daytime.

Two other major concerns of the reviewer have been addressed and we show that our (i) study is not biased towards clean cases (Figure R1 in the file responding to Referee #2) and (ii) that our results hold even in cases, when the coarse more contribution is significant (coarse-mode mass fractions up to 81% are considered in this study).

The review process has been very valuable for improving the content and the presentation of this study and we sincerely thank you for handling this manuscript.

On behalf of all the co-authors,

Dr. Ulrike Dusek

Centre for Isotope Research (CIO),

Energy and Sustainability Research Institute Groningen (ESRIG),

University of Groningen, Groningen,

9747AG, the Netherlands

Email: u.dusek@rug.nl

Tel: +31 503634689

Response to referee#1 for manuscript "Aerosol optical properties within the atmospheric boundary layer predicted from ground-based observations compared to Raman lidar retrievals during RITA-202" by Xinya Liu, Diego Alves Gouveia, Bas Henzing, Arnoud Apituley, Arjan Hensen, Danielle van Dinther, Rujin Huang, and Ulrike Dusek. (manuscript ID: EGUSPHERE-2023-2262)

We would like to thank the referee for the valuable comments on our paper, we believe that the manuscript has been improved significantly due to their suggestions. To facilitate the review process, we have copied the comments in black text and renumbered them for easy cross-referencing. Our responses are in standard blue text. We have responded to all the comments made by the referee and have revised the manuscript accordingly.

Referee #1 comments:

1 General remarks:

The manuscript discusses an interesting approach and brings together in situ observations of microphysical aerosol properties and chemical composition at ground (at Cabauw in The Netherlands) and optical modeling and comparison of the modeling results with lidar profiles of measured aerosol optical properties. There are many examples of such so-called closure experiments in the literature (since about 25 years), however, such exercises are still needed and thus the manuscript is a good addition to the literature in this field of optical closure studies.

In contrast to the other reviewer, I do not think that the paper should be rejected. It shows the present state of the art when combining ACTRIS observations from super sites… equipped with (a) aerosol monitoring tools and (b) remote sensing instruments. I also do not agree that this manuscript is a measurement report. Closure studies as presented here are more than just observations.

My main point of criticism is the following one: A lot of essential information is given in the rather extended supplement. That means one has to be very 'active' as reader and switch from main text to supplementary material and back and so one. A fluent reading is not possible. In the detail section, I will provide a few suggestions how this can be overcome, at least a bit.

Thank you for acknowledging the significance of our study and providing valuable suggestions for improving our manuscript.

In response to your comments, we have streamlined the main text by integrating critical information from the supplementary material directly into the main manuscript, wherever feasible. In addition, in order to minimizes the amount of supplementary material while still providing an access to readers who interested in this information, we have transferred the FigureS21-42 in the original manuscript to a public repository (https://doi.org/10.5281/zenodo.11174465).

Furthermore, we have carefully considered your detailed suggestions and have made corresponding adjustments to further improve and clarify our study.

We hope that these revisions address your concerns and look forward to your further guidance and feedback.

2 Details:

2.1 The Abstract needs to be adjusted after finalization of the revision.

We have carefully revised the abstract to better reflect and summarize the contents of our manuscript.

2.2 p2, l53: Cooney et al. and Melfi references are not appropriate here, in the context of aerosol extinction retrieval… Cooney and Melfi are pioneers in the field of Raman lidar developments because they introduced the temperature and water vapor Raman lidar technique.

We have revised our manuscript to correct this oversight (line 53) and ensure that our references accurately reflect the context of cited works. Thank you for bringing this to our attention.

2.3 p3, l81: be more specific already here, mention time periods.

We have adjusted accordingly and the time period is stated at the beginning of the methodology description on line 81.

2.4 p3, l84: I would prefer to include Figure S1 in the main text, and even Figure S2.

We have combined Figure S1 and S2 into a single figure, which is now included in the main text as Figure 1.

2.5 p5, l144: Avoid confusion (with lidar backscatter at 180°) already in the beginning, mention the angle range directly after … backscatter coefficient (7° to 170°).

We now mentioned the angle range (7° to 170°) directly after introducing the backscatter coefficient, to avoid any potential confusion with lidar backscatter at 180° on line 146. Thank you for highlighting this point.

2.6 p6, l174: When having a near range telescope you should be able to show extinction and lidar ratio values even down to 500 m height (after overlap corrections). And for heights above about 1000 m, you should be able to use the far range observations (after overlap correction) and then we would have much better, less noisy lidar ratios between 1000 and 2500 m height. So, why are these data not included? ….. should be stated…. I would recommend to use your own Raman analysis method instead of using the automated SSA software, and in this way, to optimize the lidar products in these optical closure studies.

We thank the referee for these observations and suggestions. Encouraged by this comment, we investigated the application of an overlap correction to reduce the minimum valid altitude for the extinction and lidar ratios. For that, the overlap function has been determined using the method proposed by Wandinger and Ansman (2002) from measurements with relatively low aerosol loads (ext. coef < 100 Mm-1, clean/no residual layer), and the results were used to reprocess the data. The average overlap correction found agrees with our expectations based on the telecover tests, being < 3% for ranges above ~700m. We found that the overlap correction improved the retrievals, allowing us to reduce the minimum valid altitude for all cases down to 810 m, and also demonstrating a better match with the model calculations. Examples are provided below (Figure R1-R3), showing the comparison of profiles before and after overlap correction with model results. For heights below this altitude, the overlap vertical gradient increases rapidly and the uncertainty of the a-priori lidar ratio becomes more important, even for clean days. To avoid these issues, we keep the minimum valid altitude as 810 m and we have incorporated the reprocessed data to the revised manuscript.

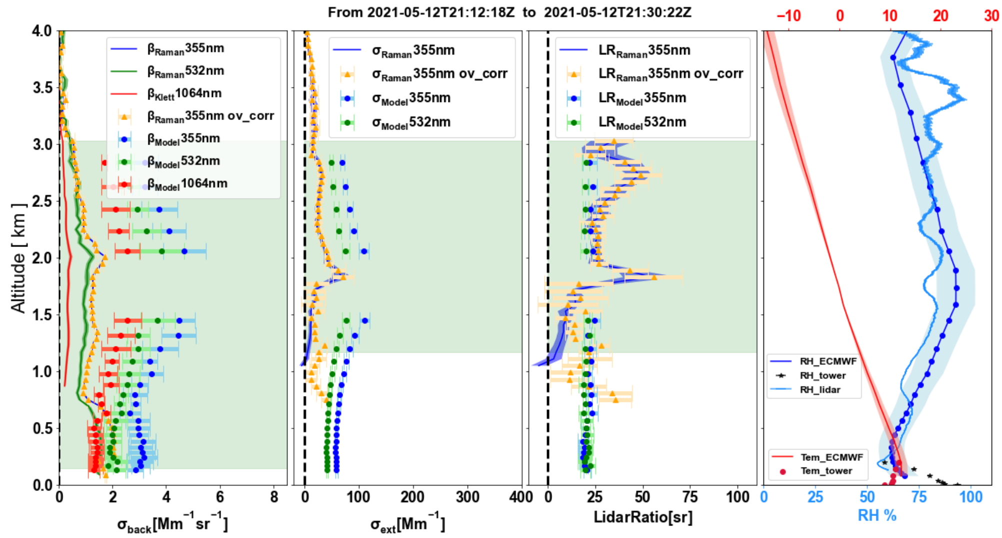

Figure R1: Comparison of profiles before (blue line) and after (orange triangle) overlap correction with model results (blue dot) from 21:12 to 21:30 at UTC time on 2021-05-12.

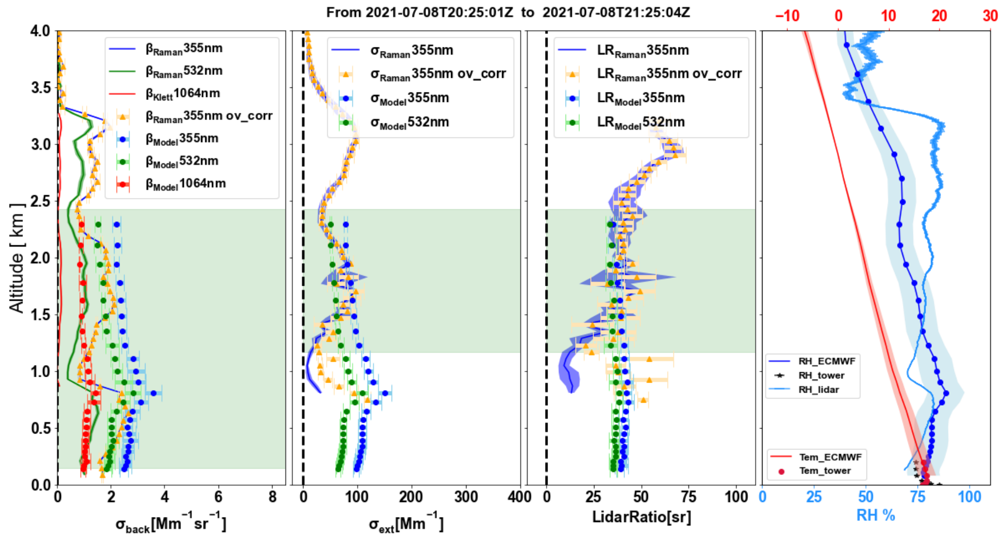

Figure R2: Similar to Figure R1 but showing profiles from 20:25 to 21:25 at UTC time on 2021-07-08.

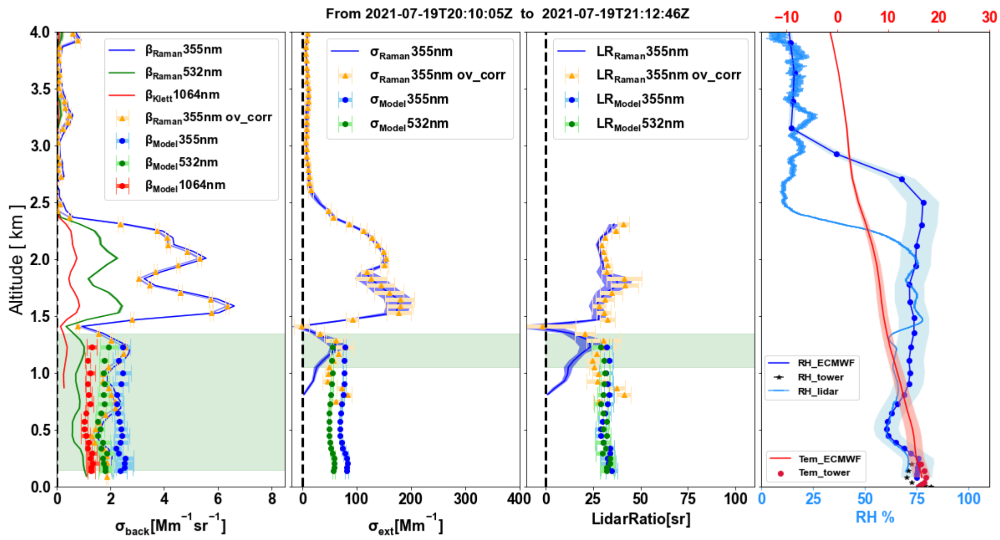

Figure R3: Similar to Figure R1 but showing profiles 21:12 to 22:00 at UTC time on 2021-07-19.

Regarding the far field range telescope (FFR) not being used for the retrievals: The Cabauw station has been prioritizing lidar processing using the Single Calculus Chain (SCC) in a networked effort for centralized, harmonized and quality assured data processing in the framework of ACTRIS/EARLINET. Unfortunately, the SCC cannot yet combine the near and far field telescopes in its retrievals. That is a more practical reason why only the near field range telescope (NFR) was used in the retrievals. Although it is true that the combination of the near and far field telescopes would yield optical products with better signal-to-noise ratios, the retrieved extinction profiles below ~1500 m wouldn't be greatly different at 355 nm and the SNR improvement on the 532 nm extinction would still be insufficient to compensate for the low pulse energy we had for the visible wavelength during the campaign. This reduces the added value of including the FFR signal for this work. For those reasons, we will remain using the Single Calculus Chain for the backscatter and extinction retrievals in the revised paper.

The paragraph describing the lidar processing (section 2.3.1) was changed accordingly in line 178-184 of the revised manuscript

2.7 Another question: What about the Raman lidar observations of the water vapor mixing ratio. In combination with ECMWF temperatures (usually very accurate) one could present them in the panels with ECMWF T and RH profiles. Even during daylight conditions, I could imagine that signals are good enough to show water vapor data up to 1000 m height.

The ECMWF RH profiles are rather uncertain (as usual for modelled water vapor profiles), so one needs more information about the 'real world' RH conditions. I would appreciate, if one shows radiosonde profiles in the respective panels in Figures 5,6,7,8, and if possible the Raman lidar RH profiles. The humidity has such a large and critical impact on the modelled optical properties, one needs to show better RH values, even if ECMWF RH values are considered in all the modelling, the reader should know about the quality (uncertainty) of these ECMWF RH profiles.

Thanks for your suggestions. We recognize the importance of accurately representing real-world RH conditions and the critical impact of humidity on modelled optical properties, thus, we have included radiosonde profiles and Raman lidar water vapor mixing ratio derived RH profiles in Figure 6-8 in the revised manuscripts. This addition is aimed at providing readers with a clearer understanding of the quality and uncertainty associated with ECMWF RH profiles.

2.8 Figure 1 is certainly confusing for non-lidar scientists, especially regarding all the vertical white lines up to 5 km height. Is that just noise or is that strong backscatter from clouds…? Furthermore, what do you mean: an overview is given in Figure 1…., when nothing is explained? What is then the message to the reader? The Raman lidar observations need to be better indicated by thicker lines and brighter color, maybe yellow or orange.

We apologize for any confusion caused by the initial demonstration of Figure 1. In response to your feedback, we have revised the description of Figure 1 (line 195-198) to clarify its content and purpose. Additionally, we have improved the visualization of Raman lidar observations within the figure by employing thicker lines and brighter colors.

2.9 p9, l232: I would include Table S3 in the main manuscript.

Response: Thank you for your advice. We acknowledge the significance of the data in Table S3. Accordingly, we have incorporated Table S3 into the main text as Table 1 in the revised manuscript.

2.10 p9, l243: One should better highlight and explain, how the vertical profile is obtained…. Maybe, one should have a subsection (on vertical aerosol profile) , and show a sketch…, showing T and RH profiles, a well-mixed PBL, and maybe even T and RH profiles for a well-mixed layer, i.e. pot temp = const, RH increasing according to mix ratio = const. In addition, the optical properties as modelled at the surface (indicated by a big symbol) should be shown and finally the aerosol extinction profile, that is in agreement with the RH height profile structures.

Such a sketch would support the reader to understand the closure results…. in Figs. 5-8.

Thank you for your suggestion. In response, we have improved the depiction of the calculation flowchart and made a sketch for the vertical profile calculations, now presented as Figure 3 in the revised manuscript. We added a much more detailed explanation of how the vertical profile is obtained, with all the relevant equations along with the reasoning behind it in section 2.4 of the manuscript.

3 Results and discussion:

3.1 I would prefer to start with Figure S1 and S2 in the main text! Four case studies are then discussed. To provide all necessary details (to the field site, trajectories etc…) in the main text, one probably has to reduce the number of case studies. In the case of Figure 5, I would prefer to see in addition Fig. S9 (showing the full advantage of a lidar, clearly indicating many different aerosol layers, rather than any well-mixed layer), Fig. S10, providing information about the chemical composition, and Fig. S11, showing the origin of the pollution. However, we need at least different trajectories at 250 m (representative for surface aerosol conditions), 900 m, and also one for the 1200-2500m aerosol layer.

In this way (Fig. 5, S9, S10, S11), we would have a complete story and could much better discuss the results of the closure study, and why there is disagreement, especially for heights above 1200m.

I also believe that a full set of observed information (including a much better description of the humidity conditions and air mass transport at different heights) will allow a critical and much deeper debate on the applicability of the closure approach presented here and the especially concerning the limits of the approach.

And as mentioned, I would include a nighttime radiosonde RH profile (19 May, 23:30 UTC, Figure S6 shows it), and if Raman lidar mixing ratio data are available even Raman lidar based RH profiles.

Fig. 5: I do not see (a), (b), (c), (d), where did you put/place these letters? If there are only 355 nm extinction and lidar ratio profiles, then one should not show 532nm in the boxes (with line and symbol explanations), and these white boxes should not hide values. This holds for all other figures and panels as well.

Thank you for your comprehensive feedback, which has guided us to make thorough adjustments to present our story more coherently.

Firstly, we have integrated Figures S1 and S2 into the main text within the methods section 2.1 to provide readers with a clearer understanding of the experimental setup from the outset.

Following your suggestion, we have combined the content of Figures 5, S9, S10, and S11 into a single figure (Figure 6 in the revised manuscript). This allows readers to grasp the entirety of a case study without the need to navigate between the main text and supplementary material.

As per your recommendations, we have incorporated the lidar derived RH profile, radiosonde RH profile, ECMWF profiles into a single plot as shown in Figure 6(d) in the revised manuscript. We also added backward trajectories at 3 different heights (Figure 6(f)) for providing a more comprehensive information.

Finally, we have improved the visualization of the original image, adding necessary numbers and preventing valid information from being hidden. Subsequent pictures have also been adjusted accordingly.

3.2 The same for Figure 6, we need in addition, Fig S12, S13, and 14 (with three trajectories) in the main text. And on 9 Sep, it was probably dark over Cabauw at 21 UTC…. so please show Raman lidar RH profiles plus radiosonde RH profiles (9 Sep 23:30 UTC).Now, we can discuss this closure study in very large detail, including the uncertainty in the model results caused by the ECMWF RH profile.

Similar to the previous response 3.1, we have made the corresponding changes in the revised manuscript.

3.3 I would skip the Fig. 7 closure study. There is already the 19 May case, and the lidar ratio shows marine conditions. Figure 8 is nice, could be presented with the figures S18-S20 here in the main manuscript, and again more trajectories for more heights (250 m, 800m, 1600 m) should be shown. Furthermore, Raman lidar and radiosonde water vapor profiles, if available. Alternatively, one could try to combine Figs. 7 and 8 in ONE figure and show only the optical properties, and briefly discuss the results of these closure study.

In the revised manuscript, we have removed original Figure 7 and provided a detailed discussion on only one clean case (Figure 8). The corresponding modifications are in the revised version.

3.4 Figure 9 shows just ONE 532 nm lidar ratio. I would remove this 532 nm value, so that only measured 355 nm lidar ratios are considered in Fig 9a and 9b.

In response to your suggestion regarding Figure 9, we have removed the retrieved lidar ratio at 532 nm to avoid any potential confusion.

3.5 The supplementary material is too much, no reader (except the reviewers) will study all details so one should reduce the amount of figures and plots to an absolute minimum.

Thanks for the suggestions. We have taken steps to streamline the content, reducing the number of figures and plots to an essential minimum. However, considering the potential value of these materials to interested readers, we have relocated the additional content to a publicly accessible repository (https://doi.org/10.5281/zenodo.11174465).

Ref.: Wandinger, U. and Ansmann, A.: Experimental determination of the lidar overlap profile with Raman lidar, Appl. Opt., 41, 511, https://doi.org/10.1364/ao.41.000511, 2002.

Response to referee#2 for manuscript “Aerosol optical properties within the atmospheric boundary layer predicted from ground-based observations compared to Raman lidar retrievals during RITA-202” by Xinya Liu, Diego Alves Gouveia, Bas Henzing, Arnoud Apituley, Arjan Hensen, Danielle van Dinther, Rujin Huang, and Ulrike Dusek. (manuscript ID: EGUSPHERE-2023-2262)

Referee #2 comments:

The authors present a comparison of backscatter and extinction coefficients as inferred from ground-based in-situ measurements and lidar observations. The manuscripts describe the steps taken to convert in-situ measurements to optical properties and shows findings for a five-month measurement campaign in 2021. Given these specifics, the manuscript would potentially qualify as a Measurement Report. However, I don't find the work to be within the scope of ACP and there are several issues that lead me to recommend rejection:

1. The authors fail to motivate why the scientific community should be interested in this work. Will the method be applied in future analyses of CAELI observations? Can it be adapted to other sites? How is it superior to the traditional approach of just assuming a lidar ratio?

   In response to the referee’s feedback regarding the motivation behind our work, we have taken steps to more clearly state the significance and relevance of our study in the revised manuscript, particularly in the abstract. Here, we wish to emphasize several key points why such a study is interesting and worthwhile:

   a. **Novelty:** The methodology developed in this study provides a valuable approach for obtaining vertical profiles of the lidar ratio through in situ measurements and readily accessible ECMWF meteorological data. This is particularly valuable for atmospheric research sites lacking advanced lidar techniques. We show that for realistic aerosol size distributions the lidar ratio is rather insensitive to relative humidity and can be used to evaluate typical lidar ratio assumptions more quantitatively.

   b. **Integration**: Our research contributes significantly to bridging the gap between in situ measurements and remote sensing lidar observations. The methodology provides a good connection from the ground to the lowest lidar profiles, effectively helping solve the overlap issues that are often a challenge in lidar systems.

   c. **Broad Applicability**: One of the key advantages of our approach is its only reliance on routine in situ measurements to derive aerosol vertical optical properties. This principle is applicable to other sites. As part of our future research efforts, we aim to collect and analyse data from additional sites to conduct broader and more comprehensive studies.

   d. **Validation**: While our primary aim was not to apply this method directly to CAELI observations, it nevertheless offers a significant reference values for validating observations made by CAELI or similar instruments.

   Moreover, while the literature suggests that traditional methods can provide reasonably reliable lidar ratio values, the effectiveness of these traditional approaches is inherently limited to sites equipped with Raman lidar measurement capabilities. This limitation is a significant constraint, as not all atmospheric research sites possess such advanced instrumentation. Our methodology offers a valuable alternative for deriving lidar ratio through conventional in situ measurements, significantly broadening the applicability of aerosol research to sites without advanced lidar technology.

2. The presentation is rather unfocussed with an additional 42 figures in the supplement. The authors also mention instruments like the microwave radiometer and the ceilometer that are not really used later in the work. I suggest to identify key messages and trim the presentation accordingly.

We have undertaken a thorough revision of our manuscript to address this issue. We have now put some key information from the appendix to the main text, ensuring that the most critical data and findings are readily accessible. Additional detailed profiles and supplementary data (previously Figure S21-42) have been relocated to a publicly accessible repository (https://doi.org/10.5281/zenodo.11174465), providing an opportunity for interested readers to explore this information further.

Furthermore, we have clarified the role of the ceilometer in our study, specifically its utility in identifying boundary layer heights, which is mentioned on line 193-195 in the revised manuscript.

While the microwave radiometer data were not a primary focus of our investigation, the meteorological data, particularly relative humidity measurements, play a crucial role in our research. These data were incorporated for comparison purposes, underscoring that ultimately, utilizing data from ECMWF is a superior choice, which also broadens the applicability of our model. However, in the revised version of the manuscript, we have chosen to retain only 3 profiles (Figure 6-8) to avoid overwhelming the readers with excessive detail.

3. The presented measurements seem to be quite specific focussing almost exclusively on clean conditions. It would have been nice if there had been an effort to put the aerosol conditions during RITA into a long-term perspective, e.g., using long-term sun-photometer measurements.

In our original manuscript, we discussed two relatively polluted cases and two clean cases to present a balanced view of aerosol conditions. In the revised version, we continue to showcase results under both polluted (originating from the continent, 1st case and 2nd case) and clean conditions (originating from the ocean, 3rd case). While the pollution levels in these cases might seem mild when compared to the more heavily polluted regions globally, they are representative of the pollution encountered in the Netherlands. This is demonstrated in Figure R1. The boxplots below compare $PM_{2.5}$ concentrations from May to October 2021 to the conditions encountered during the two RITA campaign periods (Figure R1(b)) and the lidar case studies (Figure R1(c)). The cases discussed in the manuscript are indicated by dashed grey lines.

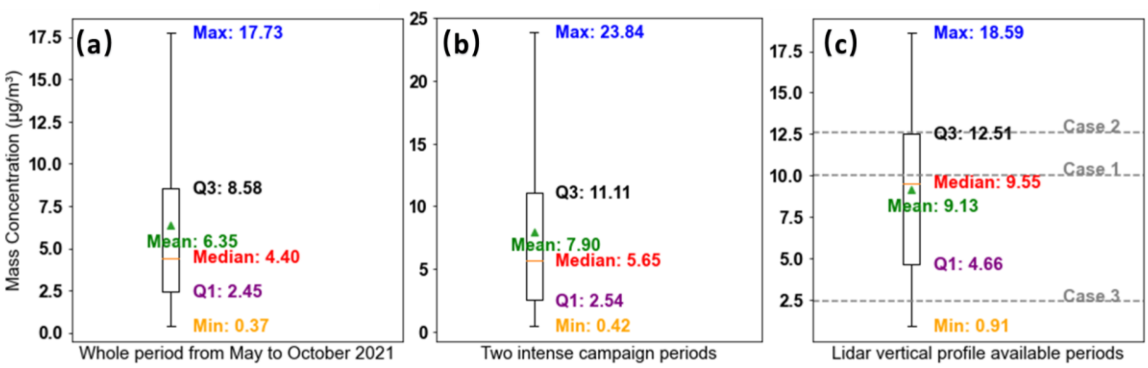

Figure R1: Boxplot of $PM_{2.5}$ mass concentration (a) during the entire period from May to October 2021 with outliers excluded; (b) Two intensive campaign periods (11th May - 24th May and 16th September - 12th October) with outliers excluded; (c) lidar vertical profile available periods with all data included. The three gray dashed lines represent the mass concentrations for the three cases discussed in the revised manuscript.

Among all the lidar measurement periods, there was only one case where considerably higher pollution values were recorded at the surface than in case 1 and 2, namely 18.6 µg m^{-3} as shown in Figure R1(c). This corresponds to the profiles showing below in Figure R2. However, the layer above 1 km was very clean, and only a very small region with a valid lidar ratio retrieval was available, therefore we did not focus on this case. However, within the small region, predicted and retrieved lidar ratio agreed well. This case further underscores the benefits of using in situ measurements to validate the reliability and accuracy of lidar observations.

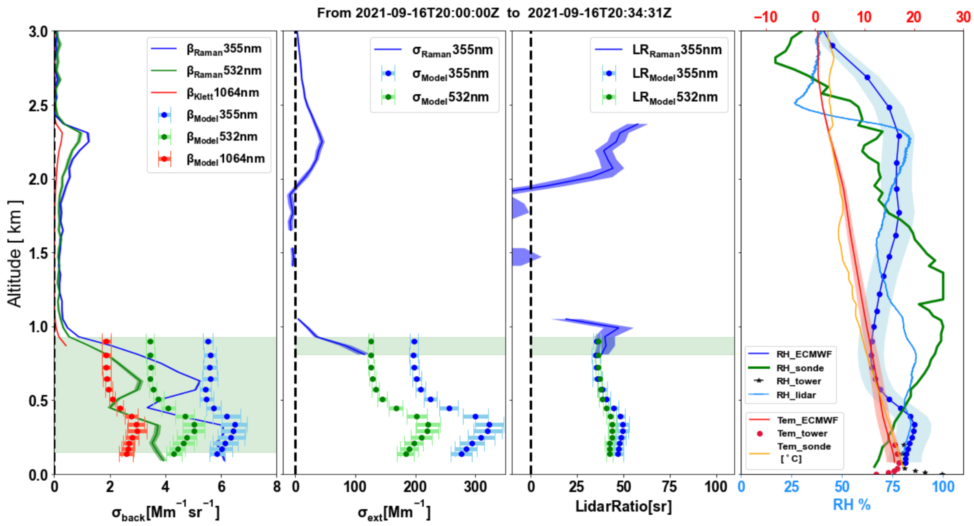

Figure R2: Profiles from 20:00 to 20:34 at UTC time on 2021-09-16.

Regarding the incorporation of long-term measurements to provide a broader perspective on aerosol conditions during the RITA campaign, we acknowledge the value of such an analysis. However, the availability of sun-photometer measurements during the RITA-2021 campaign period was very limited, which constrained our ability to conduct a more extended analysis. Despite this limitation, integrating long-term data into our research is indeed part of our future plans.

4. The authors admit that coarse-mode aerosols have a large impact on the scattering calculations. While this is somewhat minimised by the clean conditions considered in their work, it is likely to be of huge importance during other conditions. In that context, it would have been nice to get some long-term perspective on the occurrence of coarse-mode aerosols. The authors should also mention that Mie theory is inadequate to infer optical properties of dust particles.

In our study the mass fraction of coarse mode ($PM_{2.5}$-$_{10}$) vs PM_{10} aerosols was about 49% on average, ranging from 14% to 81% during the two RITA campaign periods (11th May - 24th May and 16th September - 12th October in 2021). This is in line with previous longer-term measurements of $PM_{2.5}$ represented 50 % of the PM_{10} mass fraction at Cabauw from September 2007 to October 2008 (Mensah et al., 2012). The coarse mode accounted for about 13% of the total light scattering on average (ranging from 0.05% to 94.5%) in our study. Under these conditions, our method can still provide reasonably accurate lidar ratio predictions even in the absence of specific chemical composition data for coarse-mode particles, as long as the size distribution of the coarse mode is known. Although in individual cases, there are sometimes significant differences in predicted and retrieved lidar ratios in the original manuscript (as the dates are indicated in the Figure R3(a) below), we found the coarse mode mass fraction was about 10% to 20% during these specific cases, so the difference were not due to the coarse mode. In fact, the comparison has been improved when we applied the overlap correction function to our retrieved data (Figure R3(b)) in the revised manuscript. This suggests that in our study, the mismatches between the model and the lidar measurement were seldom due to coarse mode but rather to the lack of overlap correction.

This is a significant result and we aim to apply this method at other sites and conduct long-term studies in the future to provide a more comprehensive understanding of aerosol conditions, for which this applies. We expect at more inland sites this result might still be valid, even if the coarse mode makes up a bigger mass fraction, because the chemical composition is typically much less variable than the extreme cases (pure sea salt vs pure mineral dust) we consider for the Cabauw site that is sometimes considerably influenced by marine air masses.

We acknowledge the limitations of Mie theory in accurately predicting the optical properties of non-spherical dust particles. This limitation is an important consideration in future research, especially in regions where dust plays a significant role in atmospheric aerosol composition.

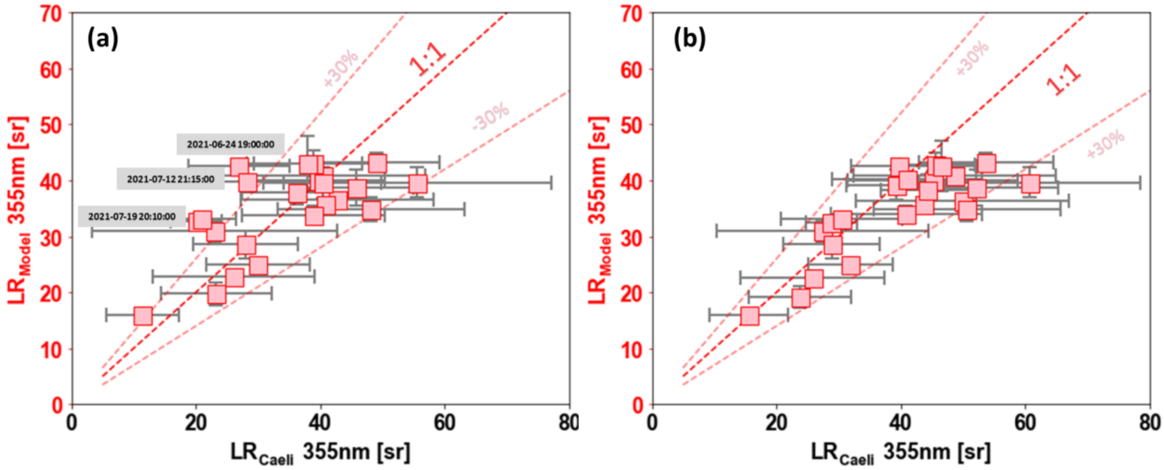

Figure R3: Scatter plot of the lidar ratios from Raman lidar measurements (x axis) (a) original data (b) new data with overlap correction applied, and from calculations (y axis) at 355 nm.

5. The optical profiles inferred from the ground-based in-situ measurements all look like scaled versions of the RH profile. This is not surprising as only RH might give some insight on vertical variation and the authors assume aerosol conditions to be constant with height. It doesn't seem fair that any discrepancies between measured and modelled optical properties are then attributed to errors in the RH profile. The authors should rather find a way to identify a maximum height up to which the vertical extension of ground-based in-situ measurements can give meaningful results. High-resolution sounding profiles could be a source for such an assessment and I wouldn't assume any connection between the ground and above the first inversion - particularly later in the year.

In our methodology and results, relative humidity (RH) indeed plays a significant role in the outcomes of the vertical profiles. However, as stated in our original manuscript, discrepancies between the model and lidar measurements are not solely attributed to variations in RH. Other factors, such as different aerosol layering (line 439-441) and aerosol shape effects (line 426-429 and line 463-466), can also significantly contribute to overestimation or underestimation of the backscatter and extinction profiles. Indeed, these extensive aerosol properties are not directly predicable from ground-based measurements above the mixed layer. Nonetheless, an intensive property, such as the lidar ratio is still comparable between retrievals at around 1 km and the ground, if that height is within the boundary layer or residual layer.

The maximum height up to which the vertical extension of ground-based in-situ measurements (in particular the lidar ratio) can yield meaningful results was determined based on ceilometer data, as described in the original text (line 194-196). In the revised version of our manuscript, we have emphasized this point (line 193-195) to clarify the basis our methodology.

6. The authors overstate their findings. They state "a representative lidar ratio can be estimated based on ground-based in-situ measurements". However, the presented results give the impression that they are by no way superior to an analysis by an average lidar operator. The suitability furthermore hinges on the assumption of vertical homogeneity which - though not unreasonable - should still be supported by some form of measurement. They continue "This allows to extend extinction profiles to lower altitudes, where they cannot be retrieved, or for use with simple elastic backscatter lidar to derive extinction profiles." Again, lidar operators have been doing pretty well with assuming lidar ratios based on experience. The authors would need to be more specific to support their statement. And conclude "The proposed method could be further applied to predict aerosol optical depth and also might be beneficial for large-scale or global radiation simulations." It is quite customary to simply assume constant lidar extinction coefficients from the lowermost trustworthy measurement height to the surface. This approach generally shows good agreement to Sun-photometer observations of aerosol optical depth. This approach also assumes vertically homogeneous aerosol conditions but is much more straightforward than the authors' work.

In addition to the points highlighted in response 1, we wish to reiterate the significant contribution of our research to the atmospheric science community.

Our study effectively bridges the gap between in situ measurements and remote sensing lidar observations, addressing a notable gap in the literature where few studies have integrated these two independent methodologies for vertical aerosol profiling.

Our approach extends beyond the conventional column-integrated optical depth (Sun-photometer observations) closure to provide a broad exploration of vertical aerosol profiles. This allows for a deeper understanding of aerosol optical properties across different atmospheric layers.

Additionally, we believe that science should embrace multiple perspectives rather than be confined to or satisfied with a single methodology. By integrating different methodologies and independently verifying the results, we can improve reliability of our measurements.

In conclusion, we believe that the integration of in situ and remote sensing measurements is definitely worthwhile, offering a more accurate assessment of aerosol optical properties and their spatial distribution. This methodological innovation aligns with ACP's scope of enhancing our understanding of the atmosphere's composition and its broader impacts.

7. The choice of references regarding the lidar technique in general and lidar ratios in particular is quite unusual. I suggest to consult with the corresponding co-authors to find more suitable references.

We have consulted with our co-authors and have revised our reference list (line 45-59) to include more appropriate and widely recognized sources in the field.

References:

Mensah, A. A., Holzinger, R., Otjes, R., Trimborn, A., Mentel, T. F., Ten Brink, H., Henzing, B., and Kiendler-Scharr, A.: Aerosol chemical composition at Cabauw, the Netherlands as observed in two intensive periods in May 2008 and March 2009, Atmos. Chem. Phys., 12, 4723–4742, https://doi.org/10.5194/acp-12-4723-2012, 2012.