Author's response

Dear Referee 1,

We thank you for careful reading of the manuscript and for your fruitful comments. Indeed, all comments have been extensively taken into account in the present form of the manuscript. In what follows, answers to comments are reported just below each related comment.

This manuscript presents the results of fluorescence lidar measurements conducted over Thessaloniki. The fluorescence capacity and depolarization ratio are used as the basis for aerosol typing. Applying this classification scheme, the occurrence of different aerosol types at various altitudes during the 2024–2025 period is analyzed. The results of the fluorescence-based typing are also compared with those obtained using the EMD algorithm.

Overall, the manuscript is well written and suitable for publication in ACP, but several revisions are needed to address specific technical points and improve clarity.

Detailed comments

Table 1. The filter width at 466 nm is likely 44 nm, not 50 nm—please verify and correct.

Thank you for pointing this out. The filter width at 466 nm is indeed 44 nm. The value in Table 1 has been verified and corrected accordingly.

Line 147. The text mentions "different PMT sensitivity, detector efficiency…". Does the calibration constant account for both of these factors?

We agree that the sentence "They include contributions from the optical throughput, IF transmission, different PMT sensitivity, detector efficiency, and electronic gain of each channel" may cause confusion regarding which factors are accounted for by the calibration constant. Therefore, we have removed this sentence from the manuscript. The following sentence in the manuscript already clarifies what the ratio represents "The calibration factors ratio therefore represents the relative calibration factor between the Raman and fluorescence channels; it corrects for their different instrumental efficiencies and is calculated following the calibration procedure described by Veselovskii et. al. (2020b) and analysed by Gast et. al. (2025)".

Table 2. The systematic biases are very low. However, it should be kept in mind that the smoke fluorescence spectrum depends on factors such as origin, altitude, etc. It is not entirely clear whether the authors corrected for differential Rayleigh extinction at the central wavelengths of the filters, or whether they accounted for spectral dependence within the filter transmission band. Ignoring this spectral dependence, especially for a 100 nm filter, could introduce a significant bias—this issue should be addressed.

Thank you for raising this point. The biases reported in Table 2 account only for the effect of the interference-filter (IF) transmission band. We acknowledge that additional sources of uncertainty may affect the retrieved fluorescence spectrum, including the dependence of the fluorescence spectral shape on aerosol type, source region, and altitude, as well as the spectral dependence of the atmospheric transmission within the filter bandwidth. These effects are not included in the bias estimates presented in Table 2 and will be investigated in future work. Regarding differential Rayleigh

extinction, the correction is currently calculated using the central wavelength of each filter, as described in Lines 143–145 of the preprint manuscript.

Fig. 1. The labels in the figure also indicate the wavelengths corresponding to the spectral maxima. This information should be mentioned in the figure caption.

Thank you for the suggestion. The sentence "Altitude and wavelength of the maximum of the fluorescence spectrum are indicated in the figures, and reference height is marked with an asterisk" has been added to the caption of Figure 1.

Fig. 3g. Showing the fluorescence lidar signal is not very informative. Would it be possible to present the fluorescence backscattering coefficient instead?

Thank you for the suggestion. The vertical profile of the fluorescence backscatter coefficient is already shown in the first panel for each case. The fluorescence lidar signal shown in Figure 3g is included to illustrate the temporal resolution of the fluorescence measurements. Therefore, we have retained the fluorescence lidar signal in this panel.

Table 5. The values LR_{355} = 86 sr and LR_{532} = 91 sr are quite unusual for long-range transported smoke. Typically, such values are lower, and LR_{532} is usually greater than LR_{355}. This discrepancy should be discussed in the text.

Moreover, reporting values such as 85.99 ± 32 is unnecessarily precise; it would be more appropriate to present them as 86 ± 32.

The reviewer is right. Aged smoke shows a characteristic ratio of LR532/LR355>1 and this allows a clear identification of smoke layers after long-range transport. Specifically, three aspects of the retrieval procedure were reconsidered: (i) the alpha parameter used in the wavelet-based layer detection was adjusted, resulting in slight changes in the identified layer boundaries; (ii) the reference height was modified to improve the retrieval of the 532 nm particle backscatter coefficient; and (iii) additional smoothing was applied to the lidar-ratio profiles to reduce the influence of noise on the mean values within the aerosol layers. These changes resulted in revised lidar-ratio values that are more consistent with the expected values of aged smoke.

The revised Table 5 is provided below.

Layer	Layer base (m)	Layer top (m)	G_F^{355} (10^{-6} nm^{-1})	$PLDR_{532}$	$BAE_{355/532}$ (Å)	LR_{355} (sr)	LR_{532} (sr)	EMD
1st	1642	2760	2.4 ± 0.2	0.05 ± 0.003	1.81 ± 0.22	86 ± 6	73 ± 4	PC
2nd	3180	3997	1.5 ± 0.2	0.03 ± 0.001	1.74 ± 0.17	81 ± 14	84 ± 7	PC
3rd	5190	5700	8.2 ± 0.7	0.03 ± 0.002	1.90 ± 0.17	78 ± 22	75 ± 11	PC
4th	6247	7357	9.2 ± 0.9	0.03 ± 0.004	1.95 ± 0.18	49 ± 19	83 ± 11	S

Fig. 4e. Why the lidar ratio plot starts from –50 sr? Additionally, the uncertainty is not clear for me. For example, in the middle of the dust layer the lidar ratio appears to be about 20 sr—this is a very low value and needs explanation.

Thank you for pointing this out. All the figures of the lidar ratios are from 0 to 120 sr on the revised manuscript.

The lidar ratio reaches relatively low values of approximately 25 sr in the central part of the identified dust layer, particularly below 2 km. We agree that these values are lower than the typical lidar ratios reported for pure Saharan dust. This relatively low lidar ratio may be related to variations in the aerosol mixture, particularly at lower altitudes, where the dust layer may be mixed with local aerosol particles over Thessaloniki. Above 2 km, the lidar ratio is more stable, with values of approximately 50 sr, which are more consistent with the expected range for mineral dust. Nevertheless, the identification of the layer as dust is not based on the lidar ratio alone, but on the combined analysis of the retrieved aerosol properties, particularly the particle depolarization ratio and the other parameters used in the aerosol-typing procedure. This point has been clarified in the revised manuscript.

Table 6. The depolarization ratio is 18%, which strongly suggests a mixture of dust with other aerosol types.

Thank you for pointing this out. As discussed above, the lower part of the dust layer, below approximately 2 km, can be affected by the presence of local aerosol, resulting in a mixture of transported dust with other aerosol types. This mixing is reflected in the mean particle depolarization ratio of 18% reported in Table 6. Nevertheless, the particle depolarization ratio reaches a maximum value of approximately 23% within this layer, indicating the presence of strongly non-spherical particles and supporting the identification of mineral dust.

Fig. 5c. A characteristic signature of pollen is a high depolarization ratio. However, the value shown in this figure is only about 2%, which is far too low. How can this be interpreted as pollen? The authors should justify this classification.

According to the literature, pollen grains differ in size and morphology among taxa. Alnus and Cupressaceae pollen grains, that are present in Thessaloniki at the end of February, are approximately spherical and have characteristic diameters of around 20 μm, which is consistent with the optical properties of the second layer. To provide additional supporting information for the typing of these layers, we have included in the revised manuscript independent information on the seasonal occurrence of pollen in the Thessaloniki region.

“During the end of February, Alder and Cupressaceae pollen are present in the atmosphere (Komnos et al., 2022). Figure 6 presents the mean and accumulated Alnus (alder) pollen concentrations simulated by the CAMS ensemble median model over the broader Thessaloniki area for 19-20 February 2025. The figure also includes the daily Alnus pollen concentrations measured with a Hirst-type pollen trap in Thessaloniki during 2016-2022 (Charalampopoulos et al. 2021), providing historical context for the seasonal occurrence of alder pollen. Alnus pollen grains are approximately spherical, with diameters of 20-30 μm (Smith, 2000), and therefore fall within the coarse bioaerosol size range. Their presence could consequently have influenced the optical properties of the observed aerosol layer.

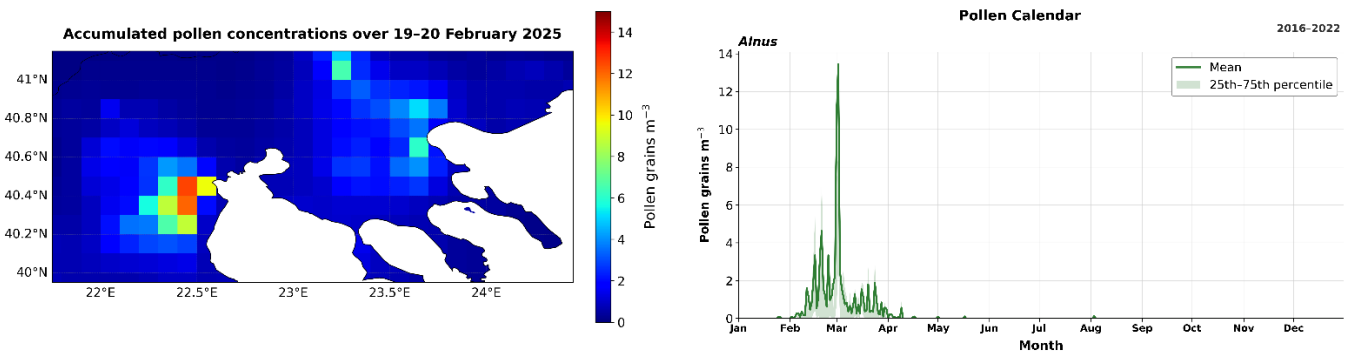


Figure 6. Accumulated Alnus pollen concentrations over the broader Thessaloniki area during 19-20 February 2025 (left) and pollen calendar of daily Alnus pollen concentrations (right). The solid green line represents the calendar-day mean, while the shaded area indicates the interquartile range (25th–75th percentiles).”

Fig. 7. The dashed boxes are said to indicate results from Veselovskii et al. (2022b). However, the values shown in this figure do not appear to match those from the cited reference. For instance, dust depolarization should reach up to 35%, the maximum pollen capacity should be of 3×10^{-4}, etc.

Thank you for pointing this out. Figure 7 was adapted from Miri et al. (2025), where it is stated that the figure is adapted from Veselovskii et al. (2022b). Therefore, the values shown in our Figure 7 correspond to the representation adopted by Miri et al. (2025). We have clarified the source and wording in the revised manuscript to avoid confusion.

Finally, it should also be mentioned that the proposed typing scheme may be influenced by particle hygroscopic growth.

Thank you for the suggestion. The sentence “It should also be noted that the proposed typing scheme may be influenced by particle hygroscopic growth, particularly under conditions of high relative humidity, as water uptake can modify particle size and consequently their optical properties, including the elastic backscatter coefficient and particle depolarization ratio (Miri et al., 2024).” has been added to the revised manuscript before the aerosol-typing figure.

Dear Referee 2,

We thank you for careful reading of the manuscript and for your fruitful comments. Indeed, all comments have been extensively taken into account in the present form of the manuscript. In what follows, answers to comments are reported just below each related comment.

The paper by Peletidou et al. presents fluorescence lidar observations from Thessaloniki, Greece, and introduces a classification scheme that combines the spectral fluorescence capacity, and the aerosol depolarization ratio, and additional contextual information. The results indicate the importance of fluorescence measurement and show how they can elucidate a more accurate assignment of dust, smoke and pollen compared to traditional aerosol typing methods. The methodology and the presentation of the different fluorescence retrieval techniques are well written and help the reader understand the ramifications of the study. The paper fits the scope of the journal and is suitable for publication. However, before final publication, I have listed several general and technical comments below that can help improve the manuscript.

General comments

Line20: Why is there a sub-dataset?

There is a sub-dataset because we stopped performing measurements at 607 nm channel for a certain period in May 2025. This channel is required for the Mahalanobis distance-based aerosol typing procedure.

Line31: It is worth including the work of Miri et al. (ACP, 2025) who similarly introduce an aerosol typing model incorporating fluorescence information.

We agree with the reviewer. The work of Miri et al. (2025) has now been included in the revised manuscript (Line 59) and has also been added to the caption of Figure 7.

Lines22-26: Do the reported ranges refer to the aerosol type assignment methodology?

Yes. The reported ranges refer to the aerosol type assignments obtained from our dataset, following the methodology described in the manuscript.

Lines51-53: Give more context. What is the "Aerosol Remote Sensing requirements"?

Thank you for the suggestion. We have added more context to clarify the meaning of the "Aerosol Remote Sensing requirements." The text has been revised as "In parallel, Veselovskii et al. (2020) extended the operational capabilities of lidar systems compatible with the Aerosol, Clouds and Trace Gases Research Infrastructure (ACTRIS; https://www.actris.eu/) Aerosol Remote Sensing requirements (CARS; https://www.actris.eu/topical-centre/cars). These requirements include lidar systems equipped with at least one-wavelength Raman or High-Spectral-Resolution (HSR) lidar with polarization discrimination, combined with an automatic sun/sky photometer, to which an additional broadband channel can be readily added."

Line54: What is "LILAS"?

LILAS (Lille Lidar for Atmospheric Study) has now been explained in parentheses at its first occurrence in the manuscript.

Line119: What is "LIF"?

LIF (lidar Laser Induced Fluorescence) has now been explained in parentheses at its first occurrence in the manuscript.

Line127: Table 1 is not introduced in the text.

Thank you for pointing this out. We have added the following sentence before Table 1 to introduce it: "The technical characteristics of the IF are presented in Table 1."

Line163: Why not using the Raman backscatter?

The equation used here follows the methodology described by Veselovskii et al. (2021) and other studies that use the elastic particle backscatter coefficient. Although Raman backscatter has also been used for this purpose and provided similar retrievals, we followed the approach mentioned above which is based on elastic backscatter for consistency with these studies.

Lines222-223: You mention that the layer is confided within the boundary layer and its top reaches 3.2 km. Given that this is a nighttime measurement, can you elaborate on this assertion?

Thank you for pointing this out. We agree that describing the aerosol layer as being confined within the boundary layer was not appropriate, particularly for a nighttime measurement, as the layer extends up to 3.2 km. We have therefore revised the sentence to avoid assigning the entire layer to the boundary layer. The sentence now reads: "The aerosol layer extended from 0.65 to 3.2 km."

Line225: Why does not the table report the values for the 14/5/2025 case?

To avoid making the table unnecessarily lengthy and to keep the discussion concise, we initially presented a detailed analysis of the aerosol layers for only one of the two cases. However, following the reviewer's suggestion, the results for the 14 May 2025 case have now also been included in the table.

Date	13/03/2025			14/05/2025		
IF	Mean G_F^{355} (10^{-6} nm^{-1})	Minimum G_F^{355} (10^{-6} nm^{-1}) [Altitude (km)]	Maximum G_F^{355} (10^{-6} nm^{-1}) [Altitude (km)]	Mean G_F^{355} (10^{-6} nm^{-1})	Minimum G_F^{355} (10^{-6} nm^{-1}) [Altitude (km)]	Maximum G_F^{355} (10^{-6} nm^{-1}) [Altitude (km)]
$IF_{466,44}$	0.93	0.31 [2.9]	1.24 [1.3]	2.11	0.75 [2.7]	5.21 [3.8]
$IF_{470,100}$	0.86	0.38 [2.8]	1.14 [1.5]	1.96	0.68 [2.6]	5.10 [3.7]

Lines229-231: In this sentence, you mention that the smoke case is presented in Fig. 2b. Figure 2b shows the absolute difference among the two G_F for either case. Is this simply a typo here or do I miss something?

Thank you for pointing this out. It was a typo. The reference to Fig. 2b has been corrected in the revised manuscript.

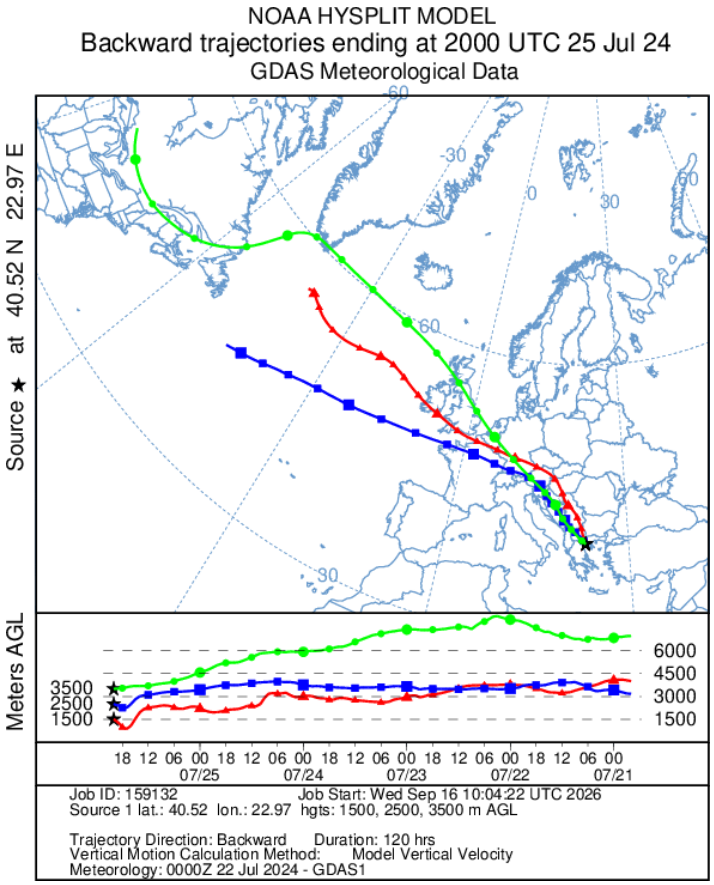
Line275: A reference to support this statement is needed.

The sentence has been revised as follows and the appropriate reference has been added: "During 2024 Canada experienced another severe wildfire season (Parrington and Tomaso, 2024)."

Lines277-280: To me, the text implies that all layers from Figure 3 are included in HYSPLIT, whereas the HYSPLIT plot actually only reflects layers 3 and 4 from Table 5. Also, why not include layers 1 and 2?

Our intention was to focus the HYSPLIT analysis on the smoke layers. Therefore, only layers 3 and 4, which were identified as smoke-related layers, were included in the HYSPLIT analysis. We have clarified this point in the revised manuscript ("The 5-day backward trajectories from HYSPLIT (Fig. 3f), calculated at the altitudes of the identified smoke layers (layers 3 and 4 in Table 5), …") to avoid implying that all layers shown in Figure 3 were analyzed with HYSPLIT.

The HYSPLIT analysis for layers 1 and 2 is provided here. The trajectories indicate that the air masses originated from Canada. Considering the transport pathways and the altitude of the detected layers (below 3 km), where other aerosol types can also frequently occur, the identification of these layers as polluted continental is supported.



Line296 & 300: What is the value in parentheses?

The value in parentheses represents the fluorescence capacity, as defined in Eq. (6). We explain in Lines 169–171 (preprint) that "Both spectral fluorescence capacity (G_F^{355}) and fluorescence capacity (G_F) are presented in the results below to allow comparison with the existing literature. In the text, G_F is given in parentheses." To make this clearer to the reader, we have also indicated the meaning of the value in parentheses at its first occurrence in the Results section. The sentence now reads: "…., the mean spectral fluorescence capacity G_F^{355} value differs substantially, reaching 8.2 × 10^{-6} nm^{-1} (fluorescence capacity, G_F = 5.1 × 10^{-4})."

Line303: Why only 2 days?

Thank you for pointing this out. The back-trajectory analysis has been corrected to use 5-day trajectories instead of 2-day trajectories. The same correction has also been applied to the pollen case shown in Figure 5f.

Line322: My criticism with this section is the following. The typing of the layer is anchored on the layer mean value of the spectral fluorescence capacity by comparing with literature and excluding other aerosol types. Precisely, this is the added value of a fluorescence lidar when it comes to aerosol typing. However, Hysplit runs do not offer the adequate resolution for this kind of investigation. Do you have access to in situ measurements that might indicate the presence of pollen? It might be helpful also to check pollen forecasting model outputs if available.

The reviewer is right. The fluorescence channel provides this additional capability and these are primarily results on the properties of pollen layers. However, at present, there are no direct in-situ measurements available at our site, but only for the years 2016-2022. We also agree that the HYSPLIT simulations alone cannot provide sufficient spatial resolution to independently confirm the presence of pollen.

To provide additional supporting information for the typing of these layers, we have included in the revised manuscript independent information on the seasonal occurrence of pollen in the Thessaloniki region. These results support the occurrence of Alnus pollen during the period of our observations. Furthermore, as already mentioned in the manuscript, Cupressaceae is also known to occur in the atmosphere over Thessaloniki, with high concentrations towards the end of February, but unfortunately CAMS model doesn't count these pollen grains for the retrievals. Both Alnus and Cupressaceae pollen grains are approximately spherical and have characteristic diameters of around 20 μm, which is consistent with the optical properties of the second layer.

The following text and figures have been added to the revised manuscript.

"During the end of February, Alder and Cupressaceae pollen are present in the atmosphere (Komnos et al., 2022). Figure 6 presents the mean and accumulated Alnus (alder) pollen concentrations simulated by the CAMS ensemble median model over the broader Thessaloniki area for 19-20 February 2025. The figure also includes the daily Alnus pollen concentrations measured with a Hirst-type pollen trap in Thessaloniki during 2016-2022 (Charalampopoulos et al. 2021), providing historical context for the seasonal occurrence of alder pollen. Alnus pollen grains are approximately spherical, with diameters of 20-30 μm (Smith, 2000), and therefore fall within the coarse bioaerosol size range. Their presence could consequently have influenced the optical properties of the observed aerosol layer.

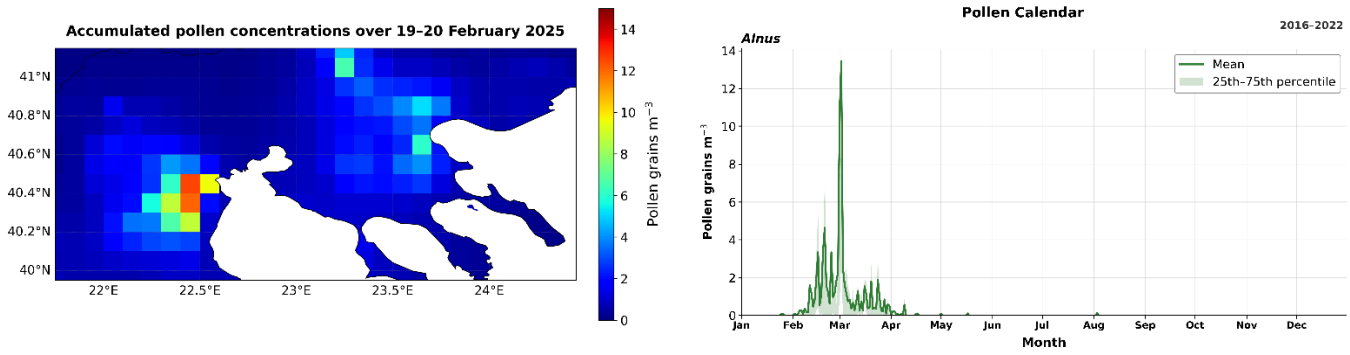


Figure 6. Accumulated Alnus pollen concentrations over the broader Thessaloniki area during 19-20 February 2025 (left) and pollen calendar of daily Alnus pollen concentrations (right). The solid green line represents the calendar-day mean, while the shaded area indicates the interquartile range (25th–75th percentiles)."

Lines357-362: Why not make the colorscale discrete by using the thresholds as discussed in this paragraph? Like this, the cases will be grouped according to the thresholds you highlight in your discussion.

Thank you for the suggestion. In this figure, our aim was to present all measurements and the corresponding fluorescence capacity values for all identified aerosol layers. Applying a discrete color scale based on the proposed thresholds would result in a more specific categorization of the cases, which may not necessarily correspond to a specific aerosol type, since aerosol typing is not based on fluorescence capacity alone. We therefore preferred to retain the continuous color scale to represent the full range and variability of the measured fluorescence capacity values without implying a direct aerosol-type classification based on these thresholds.

Lines363-372: In this paragraph, you describe the typing procedure that is shown in Figure 6b. All the criteria and thresholds invoked need to be justified and relevant references acknowledged, if any. Furthermore, a flowchart or a table would help the readers.

The following table summarizing the thresholds used in the typing procedure has been added to the revised manuscript.

Table 8. Thresholds of fluorescence capacity, spectral fluorescence capacity, particle depolarization ratio and maximum spectral fluorescence capacity used for the characterization of the aerosol layers.

Aerosol type		Fluorescence capacity G_F (10^{-4} nm^{-1})	Spectral fluorescence capacity (10^{-6} nm^{-1})	$PLDR_{532}$	$G_F^{355}{}_{,max}$ (10^{-6} nm^{-1})
Smoke		2.0 – 6.0 [1] 1.0 – 4.0 [2] 2.0 – 7.0 [3]	< 7.0 [4]	2 – 10 [1] 2 – 16 [1]	3.0 – 16.0
Dust		0.1 – 0.5 [1]	< 0.8 [4]	20 – 35 [1]	0.8 – 1.5
Urban	Polluted Continental	0.1 – 1.0 [1]	-	1 – 10 [1]	1.5 – 3.5
Urban	Clean Continental	0.1 – 1.0 [1]	-	1 – 10 [1]	0.5 – 2.0
Pollen		0.8 – 3.0 [1]	-	15 – 35 [1]	2.0 – 7.0

[1] Veselovskii et al., 2022b, [2] Hu et al., 2022, [3] Gast et al., 2025, [4] Reichardt et al., 2025

Lines377-379: Long range transported biomass does not only originate from North America as it is demonstrated by Amiridis et al. (2009). For sake of completeness, it is worth mentioning other source areas.

Thank you for pointing this out. We have revised the sentence to include additional source regions of long-range transported biomass-burning plumes. The sentence now reads: "…long-range transported biomass-burning plumes from North America (e.g., Canada), Russia, and Eastern Europe (Amiridis et al., 2009)."

Lines388-390: If I get it right, figure 7 and table 8 come from the analyzed cases. Why not report a similar table containing the thresholds of your typing procedure?

The table containing the thresholds based on the existing literature is provided before Figure 6.

Line402: You can populate the table with more statistical metrics to help understand better the differences among the aerosol types.

Thank you for the suggestion. We added the mean error of the spectral fluorescence capacity for each aerosol type to provide an additional statistical metric.

Line407: The differences among the boxes relevant to the work of Veselovskii et al (2002b) and your study are not discussed in this section. Please do so.

The reviewer is right. The differences between the values reported in Veselovskii et al. (2022b) and those obtained from the THELISYS measurements are now discussed in the revised manuscript. The following paragraph has been added: "The results obtained from the analysed THELISYS measurement cases (colored boxes) show some differences from the values reported by Miri et al. (2025) (empty boxes) in Fig. 7b. For dust and smoke, consistent typing is observed, taking into account that the measurements were performed at different locations and during different periods. For urban aerosol, higher fluorescence capacity values are observed in the THELISYS measurements, indicating a contribution from fluorescent particles, that may be related to the influence of locally emitted smoke. More pronounced differences are observed for the mixed pollen layers, particularly in the particle depolarization ratio. As discussed in Sect. 3.3, these differences are likely related to the mixing of pollen with urban aerosols observed at the Thessaloniki lidar station and/or to differences in the dominant pollen taxa. Pollen grains from different taxa can exhibit different sizes, shapes, and morphological characteristics, which may consequently affect their optical properties, and particularly the particle depolarization ratio."

Line410: I might have missed the reason behind the selected temporal window (i.e. March 2024 – April 2025) for the analysis. Can you clarify this point? Also, I found in line 245: "… April 2024…" which further confused me.

Thank you for pointing this out. "… April 2024…" has been corrected to "March 2024". The selected temporal window has been explained in the first comment.

Lines412-413: I would leave out the text in parentheses else you need to explain it.

Thank you for the suggestion. The text in parentheses has been removed from the revised manuscript.

Lines421-422: I see no layers over 8 km. Is it maybe a typo?

Thank you for pointing this out. The sentence has been removed from the revised manuscript.

Line445: The term "fluorescence typing" is an oversimplification. I would probably rephrase it.

Thank you for the suggestion. We agree that the term "fluorescence typing" may be an oversimplification. The sentence has therefore been revised as follows: "We conclude that aerosol typing using fluorescence channel capabilities, in combination with existing aerosol properties (e.g., particle depolarization ratio, Ångström exponent, and lidar ratio), could help in clearer separation between dust, smoke and mixed pollen layers."

Technical comments

All the technical comments corrected in the revised manuscript

Line18: Remove "event"

Line82: Consider swapping "Damialis et al. (2017)" with "they".

Line116: Add “the” before “National”.

Line136: It should be “subscripts”.

Line267: The beginning “In addition, in order…” is rather awkward. Consider changing it.

Line 278: Change “six” to “five”.

Line283: Remove “aerosol”.

Line320: Remove the first “of”.

Line389: Remove “with IF”.

Line408: Add “Empty” before “dashed”.