REVIEWER#3

In this article, the authors present an algorithm called CONIOPOL, that aims to classify aerosols based on measurements from single-wavelength polarization lidar. They apply this algorithm to one year (feb 2024-feb 2025) of ground-based lidar data at 910.55 nm from the CL61 instrument operated at Uccle (Belgium). They document the output of the classification and compare it with CAMS data.

The subject is important, relevant to contemporary problems, and well within the scope of ACP. The correct identification of aerosol nature, and its documentation as a function of altitude (especially in the boundary layer) is key to making progress on documenting air quality and on quantifying the indirect influence of anthropogenic pollution on climate through aerosol-clouds interactions like the Twomey effect. To reach that goal, fruitful approaches include the development of reliable, well-documented algorithms to classify aerosols that can be applied systematically to easily-deployable operational lidar measurements. The continuous datasets obtained this way provide information essential to build long-term climatologies.

The manuscript is well-written, the figures are generally well-designed and convey a clear message. The algorithm is described with precision, the impact of many choices is discussed and well documented. There are several points that should be clarified before publication.

We sincerely thank the reviewer for the careful and thoughtful evaluation of our manuscript. We greatly appreciate the positive assessment of the relevance and scientific importance of our work, as well as the recognition of the potential of CONIOPOL to provide systematic aerosol classification from operational single-wavelength polarization lidar measurements. We are particularly grateful for the acknowledgement of the clarity of the manuscript, the quality of the figures, and the detailed description and justification of the algorithm. Your constructive comments have been extremely valuable in helping us further improve the manuscript. We have carefully addressed all of the points raised, clarified the methodology where necessary, and revised the manuscript accordingly.

Before addressing the individual comments, we would like to summarize the principal modifications made to the manuscript following the reviewers' suggestions:

- The manuscript has been substantially revised to better **highlight** the role of the **cloud-phase and precipitation classifications,** which are now more clearly presented as an integral component of CONIOPOL rather than as a secondary extension of the aerosol classification.

- The **title** has been **revised** to better reflect the full scope of CONIOPOL by explicitly referring to both **aerosols and hydrometeors**. This modification better represents the capabilities of the algorithm and is consistent with the strengthened presentation of the cloud-phase and precipitation classifications throughout the manuscript.

- The **classification methodology** has been refined by introducing explicit **transitional classes** for aerosols, cloud phase, and precipitation. These intermediate categories provide a more physically realistic representation of observations exhibiting mixed optical characteristics and reduce potential misclassification arising from overlapping depolarization signatures.
- The justification of the depolarization thresholds has been substantially expanded. The selection of the **depolarization thresholds** is now supported by a more comprehensive **review of the peer-reviewed literature**, including recent studies using polarization lidars at wavelengths comparable to the CL61, providing a stronger physical basis for the adopted classification scheme.

- The **CAMS comparison** with CONIOPOL has been substantially **revised**. The analysis is now performed on a **seasonal basis, extended vertically up to 10 km**, and complemented by a more detailed discussion of the physical interpretation, **statistical significance**, and **limitations of the comparison**.

- During the implementation of the newly introduced transitional classes, we identified an error in the classification mask affecting the **Smoke/Marine aerosol** category. In the previous version, this category was only considered when no cloud was detected. Consequently, Smoke/Marine aerosols located beneath or within cloudy conditions were inadvertently excluded from the statistics. This issue has now been corrected, resulting in a substantially higher occurrence of the Smoke/Marine category and a markedly improved correspondence with the CAMS aerosol fields.

- We also revised the treatment of **weakly depolarizing targets under cloudy conditions**. In the previous version, low-depolarization observations within clouds were systematically assigned to the Fog/Drizzle/Mist category, even when they were clearly disconnected from the surface. This **decision-tree branch has been removed**, leading to a more physically consistent classification and an increased occurrence of the Liquid Cloud (Droplet) category.

- To avoid ambiguity in the interpretation of weakly depolarizing aerosols, the **former "Hygroscopic Aerosol" category** has been **renamed "Continental Aerosol"**. This terminology more accurately reflects the physical nature of the classified particles and avoids confusion with the Smoke/Marine category, since both aerosol types are hygroscopic but are associated with distinct atmospheric environments. The revised discussion further demonstrates that these two categories exhibit different vertical occurrence profiles, consistent with their association with the continental and marine boundary layers, respectively.

- The presentation of the **normalized cumulative occurrence profiles** has also been **revised**. Instead of displaying monthly statistics, the revised manuscript now presents **seasonal occurrence profiles up to 15km**, providing a clearer representation of the dominant atmospheric regimes while improving consistency with the revised CAMS intercomparison, which is now analysed seasonally and extended vertically up to 10 km.

- The **showcase figure** has also been **updated**. During the revision process, **several display issues** were identified in the previous version (e.g. the omission of a secondary cloud layer in the graphical representation). These issues only affected the visualization and **did not affect the**

CONIOPOL classification itself or any of the statistical analyses, which were produced directly from the classified data.

Main comments

My main issue with the article is that I can't understand the origins of the values of depolarization ratio used as thresholds for the classification. The article mentions (L. 256-288) a UK field campaign with as reference only this document :

https://www.vaisala.com/sites/default/files/documents/WEA-MET-WhitePaper-CL61-B212267EN-A.pdf

This document is not a peer-reviewed article, it reads more like an advertisement for Vaisala. It does not present the field campaign, explain which experiments made it possible to link specific depolarization ratios with specific aerosol types, or discuss the reliability of CL61 retrievals. Most importantly, I could not find in this document the numbers used in the CONIOPOL algorithm present in figure 1. One figure in the report shows the approximate ranges of typical LDR values for different particle types, but it is not clear where the ranges come from, or even to which wavelength they apply. The document refers to another "full report" being available from Vaisala on request. As long as the experiments and studies which led to these values, which are key to the classification, are not available and peer-reviewed, it will not be possible for me to recommend publication. This effort should involve relating the uncertainties of depolarization ratio measurements with the classification output.

We thank the reviewer for this important comment regarding the origin and justification of the depolarization ratio thresholds used in the classification scheme. We fully agree that the transparency and physical basis of these thresholds are essential for the robustness and reproducibility of the proposed CONIOPOL algorithm.

We agree that the Vaisala white paper (CL61 documentation) is not a peer-reviewed publication and is therefore no longer used as a primary justification for the threshold values. In the revised manuscript, Section 4.4 has been substantially clarified to explicitly document the origin of the depolarization threshold values adopted in CONIOPOL. These thresholds are now supported by peer-reviewed lidar studies spanning wavelengths from 355 to 1565 nm, with particular attention to aerosol-specific spectral dependence. Because much of the available literature reports PDR rather than VDR, the applicability of these values to the VDR measured by the CL61 is now explicitly discussed with respect to wavelength and molecular contribution.

At 910 nm, the molecular contribution to the measured VDR is substantially reduced compared with shorter lidar wavelengths because of the strong wavelength dependence of Rayleigh scattering. Consequently, in particle-dominated layers, VDR is expected to approach the particle depolarization signature, although VDR and PDR are not formally equivalent.

In response to your important remark regarding uncertainty, we have also added a dedicated sensitivity analysis (Sect. 4.7; Figs. A1–A2) to quantify how variations in the measured VDR propagate through the threshold-based classification. Absolute perturbations of ±0.01 and ±0.02 were applied to VDR, and the resulting classifications were compared pixel by pixel with the reference classification. The analysis shows that classification changes occur primarily for observations close to the decision thresholds and predominantly involve transitions between neighbouring categories. These results, together with the introduction of transitional classes, provide an explicit representation of overlapping depolarization regimes and a quantitative assessment of the classification sensitivity to VDR variability.

We believe that these modifications significantly improve the transparency, physical basis, and reproducibility of the classification scheme, and we thank the reviewer for this valuable comment, which has led to a substantial improvement of the manuscript.

My second concern is that I had trouble following some of the reasoning : the article makes the point that LDR by itself does not contain enough information to uniquely identify several aerosol types (for instance distinguish volcanic ash from dust), but still argues that distinct categories of aerosols are mandatory and proceeds to classify separately aerosol types that have overlapping LDR distributions. I would appreciate it if the authors could clarify their reasoning here (see also several minor comments addressing this point). I do not see the point of uniquely labelling detections as ash or volcanic dust, while recognizing that this classification requires additional information (back-trajectory analysis, geographic source attribution) to be interpreted correctly. This opens the door to misinterpretations of the algorithm output. Uncertainties on aerosol identification should be visible in the output, and the algorithm should refrain from providing certain classifications when certainty is not possible.

We thank the reviewer for this insightful comment. We agree that the original wording of Section 4.4 could suggest an apparent contradiction. This reflects our attempt to simplify the presentation of the algorithm while simultaneously minimizing misclassification and maximizing the identification of atmospheric targets using the limited information available from a single-wavelength depolarization measurement.

The objective of the algorithm is not to uniquely identify aerosol species from volume linear depolarization ratio (VDR) alone, as this is not possible given the overlap of depolarization signatures among several aerosol types. Rather, the proposed classification provides the most informative and operational partitioning of the observations permitted by the available measurements. The defined classes should therefore be interpreted as best-estimate aerosol regimes, not as definitive aerosol identifications.

To account for the intrinsic ambiguity of VDR-based classification, transitional classes have been introduced where appropriate (e.g. aerosol mixtures and mixed cloud phases), and the revised manuscript now explicitly states that several aerosol classes, such as dust and volcanic ash, cannot be unambiguously separated using VDR alone. These categories are retained because they correspond to distinct physical aerosol types reported in the literature and are relevant when complementary information, such as air-mass back trajectories, geographical source attribution, or additional optical parameters, is available. The manuscript now emphasizes that such external information is required for a confident interpretation of these classes.

In response to the reviewer's comment, Section 4.4 has been revised to better distinguish between the intrinsic limitations of VDR and the methodological choices made to provide a physically consistent and operational classification framework. The text now explicitly states that ambiguous cases should be interpreted with caution and, where appropriate, in conjunction with complementary observations.

Another issue with the article is that in my view, comparison with output from CAMS is not akin to validation. If we assume that the representation of aerosols in CAMS is close to reality, there is no need for lidar-based aerosol classification anymore, since we can just use CAMS. Actually, the point of lidar-based aerosol classification is to provide constraints to CAMS reanalyses through assimilation. Comparing output of CONIOPOL with CAMS can provide reassurance that lidar-based classifications are overall reasonable, but these classifications should provide additional information compared to CAMS (otherwise, what's the point?), and thus differences with CAMS should be expected. In my point of view, actual validation involves comparison of retrievals with in-situ measurements, for instance. Of course it is significantly harder and expensive to obtain in-situ data, and I do not expect the authors to provide new measurements during the review process. They should, however, refrain from identifying the comparison exercise with CAMS as validation, discuss it as useful comparison, and recognize its possible limitations.

We thank the reviewer for this important comment and fully agree that a comparison with CAMS should not be presented as a formal validation of the CONIOPOL algorithm. We have therefore substantially revised this section of the manuscript to clarify both the objective and the limitations of the comparison.

First, we have removed the terminology referring to "validation" throughout the manuscript when discussing the comparison with CAMS. The revised text now explicitly states that the purpose of the analysis is to assess whether the aerosol environments identified by CONIOPOL are associated with statistically distinct aerosol conditions represented by an independent atmospheric composition model, rather than to validate the algorithm against a reference dataset.

Second, the methodology has been rewritten to emphasize the fundamentally different nature of the two datasets. We now explain that CONIOPOL provides qualitative aerosol classifications, whereas CAMS provides grid-cell averaged aerosol mixing ratios, making a direct comparison of aerosol concentrations inappropriate. Instead, CAMS aerosol mixing-ratio distributions are analysed conditionally according to the corresponding CONIOPOL classifications in order to determine whether each CONIOPOL category is associated with statistically distinct aerosol environments.

To ensure a consistent comparison, the temporal and vertical matching procedures have also been described in greater detail. Because CAMS profiles are available every 3 h, CONIOPOL observations are aggregated over the corresponding 3 h interval, and a CAMS profile is associated with a given aerosol category only when that category is detected during at least 60 % of the observations in at least two of the three constituent hourly periods. Furthermore, the statistical comparison is performed within successive 250 m altitude intervals to account for the different vertical resolutions of CAMS and CONIOPOL. The corresponding sample size is now reported in the figures to facilitate the interpretation of the statistical significance.

The CAMS comparison has also been substantially expanded with respect to the original manuscript. Rather than relying on a single integrated comparison, the revised analysis now evaluates the relationship between CONIOPOL classifications and CAMS aerosol fields throughout the atmospheric column up to 10 km. In addition, a seasonal analysis has been introduced, allowing the behaviour of each aerosol category to be assessed under different atmospheric conditions. These new analyses provide a more comprehensive assessment of the consistency between CONIOPOL classifications and CAMS aerosol environments than was presented in the original version.

Finally, the discussion has been rewritten to explicitly acknowledge the limitations of the comparison. We now emphasize that CAMS does not represent ground truth and remains affected by uncertainties in emissions, transport, aerosol parameterizations, and spatial averaging, while CONIOPOL provides local, observation-based classifications without retrieving quantitative aerosol concentrations. Consequently, differences between the two datasets are expected and should not be interpreted as deficiencies of either product. Rather, the statistically significant differences observed between CAMS aerosol environments associated with the various CONIOPOL categories provide independent evidence of the physical consistency of the CONIOPOL classifications, while highlighting the complementary information provided by high-resolution lidar observations.

The title focuses on the aerosol identification part of CONIOPOL, while the article shows the algorithm classifies atmospheric features between aerosol types, liquid/ice clouds, and precipitation. In my view, the impact of the article would be improved by focusing solely on aerosols and removing the cloud/precipitation results.

We thank the reviewer for this comment.

While we agree that aerosol identification is the central objective of CONIOPOL, the inclusion of cloud and precipitation classes is intentional and fundamental to the design of the algorithm rather than an ancillary extension. From an operational air-quality perspective, aerosols cannot be interpreted independently from cloud and precipitation processes, which strongly modulate aerosol optical properties through activation, scavenging, and signal attenuation. Moreover, precipitation-related processes such as virga can play an important role in enhancing the vertical redistribution of particles, such as mineral dust, through sublimating precipitation and the generation of local turbulence (Karle et al., 2023).

For these reasons, we consider that removing cloud and precipitation results would reduce both the physical consistency and the operational relevance of the methodology. To better represent the capabilities of CONIOPOL, which also classifies hydrometeors, we have revised the title accordingly by explicitly referring to hydrometeors. This modification better reflects the comprehensive yet operational nature of the algorithm while maintaining its emphasis on aerosol identification.

Following your comment, we have also revised the manuscript to better emphasize the role of the cloud-phase and precipitation classifications throughout the paper. In particular, their contribution to the physical consistency of the aerosol classification and to the overall operational framework of CONIOPOL is now more clearly described in both the methodology and the discussion.

Karle, N. N., Sakai, R. K., Fitzgerald, R. M., Ichoku, C., Mercado, F., and Stockwell, W. R.: Systematic analysis of virga and its impact on surface particulate matter observations, Atmospheric Meas. Tech., 16, 1073–1085, https://doi.org/10.5194/amt-16-1073-2023, 2023.

Finally, the CONIOPOL algorithm takes among its input cloud base detections, and apparently assumes they are perfect. The impact of cloud misdetections is not discussed. The algorithm assumes that cloud detection and aerosol detection are independent, which is not necessarily the case. Cloud detections, however, can include aerosols and vice-versa. In other words, the signal ranges of clouds and aerosols do overlap, and misclassifications do happen, for instance in presence of optically thick aerosol layers. The study should try to evaluate the sensitivity of results to errors in the cloud detection and cloud base retrievals.

We thank the reviewer for this valuable comment. We agree that the limitations associated with the CBH detection algorithm were insufficiently discussed in the original manuscript. The Discussion has therefore been substantially expanded to clarify the role of CBH information within CONIOPOL and its potential impact on the classification.

The revised manuscript now explicitly states that the CBH information is obtained directly from the operational Vaisala CL61 cloud-base detection algorithm and is not recalculated within CONIOPOL. We also clarify that CBH information is only one of several decision criteria used by the algorithm, together with attenuated backscatter, VDR, PPOL intensity, and the two-dimensional sliding-window consistency filter. Consequently, cloud-base retrieval errors may influence, but do not uniquely determine, the final classification.

Following your suggestion, we now discuss the two main sources of potential misclassification. First, optically thin liquid and ice clouds that remain undetected by the cloud-base algorithm are processed through the aerosol branch of the decision tree and may therefore be classified as smoke/marine aerosol and dust/ice, respectively. Conversely, dense aerosol layers, such as mineral dust or volcanic ash, may occasionally satisfy the cloud detection criteria, locally activating the cloud branch and leading to ice-crystal classifications within portions of the aerosol layer. Because the decision tree is applied locally, such errors are expected to affect primarily the observations for which the erroneous CBH information modifies the decision-tree branch, rather than necessarily affecting an entire atmospheric layer.

We also added a discussion of the intrinsic limitations of the CL61 cloud-base retrieval, supported by the recent intercomparison study of Looschelders et al. (2025), which showed that CBH detection is generally robust for boundary-layer clouds but becomes more uncertain for optically thin cirrus because of the reduced signal-to-noise ratio at high altitude.

Regarding your suggestion to perform a sensitivity analysis, we agree that this would be valuable. However, a quantitative assessment would require an independent reference for cloud-base detection (e.g. collocated cloud radar or Cloudnet observations), which is not available at the Uccle site for the study period. We have therefore discussed the expected impact of cloud-base retrieval errors and identified a dedicated sensitivity analysis using independent cloud observations as an important direction for future work.

Looschelders, D., Christen, A., Grimmond, S., Kotthaus, S., Fenner, D., Dupont, J.-C., Haeffelin, M., and Morrison, W.: Inter-Instrument Variability of Vaisala CL61 Lidar-Ceilometer's Attenuated Backscatter, Cloud Properties and Mixed-Layer Height, Meteorol. Appl., 32, e70088, https://doi.org/10.1002/met.70088, 2025.

Minor comments

L. 13: Thanks for teaching me the word "coniology"!

We thank the reviewer for this comment.

Figure 1 : why do the colors mean in the diagram? (red, orange, etc.)

We agree that the original colour scheme did not convey any additional information and therefore did not enhance the interpretation of the figure. We have therefore simplified the colour scheme and retained only three colours to distinguish the main categories: orange for aerosols, blue for clouds, and grey for precipitation.

Figure 1, legend: "cloud base height" is not a lidar measurement. It is a retrieval, with all the attached uncertainties and interpretation issues.

We thank the reviewer for this clarification. We have revised the caption of Figure 1 to specify that cloud base height is obtained as a lidar-derived retrieval product, and not a direct measurement, in order to avoid any ambiguity regarding its observational status.

L. 50: The article should explicitly address the difference between volume depolarization ratio (which is affected by the ratio between signal contributions from particles and molecules) and particulate depolarization ratio (only due to particles), and be explicit which one it is referring to. "Linear" depolarization ratio is often assumed to refer to the particulate depolarization ratio (as in Burton et al. 2015) in opposition to the plain "depolarization ratio" often used to refer to the volume depolarization ratio. CALIOP measures volume depolarization ratio, while the HSRL ATLID measures separately Mie and Rayleigh backscatters and thus provides the particulate depolarization ratio.

We thank the reviewer for this comment. We have revised the Introduction to explicitly distinguish between the volume linear depolarization ratio (VDR) and the particulate linear depolarization ratio (PDR), and to clarify their respective physical meanings. To avoid misleading the reader, the term volume depolarization ratio (VDR) is now explicitly used whenever referring to depolarization measurements, and this terminology is consistently adopted throughout the remainder of the manuscript and in the abstract.

L. 200: multiple scattering within the cloud is generally not noticeable in ground-based lidar measurements

We thank the reviewer for this comment. We agree that the original statement was too general and could be misleading. In the revised manuscript, we have clarified that multiple scattering within liquid clouds may induce a slight increase in the measured VDR, as reported by Vakkari et al. However, this effect remains limited and is further minimized by using only the first range gates at the cloud base, in the absence of drizzle or precipitation, for calibration monitoring. The text has been revised accordingly.

L. 215-225: Around this section, it would be helpful to document the variability of depolarization ratios in various SNR and target situations at the spatial and temporal averaging used by the authors. Figure 2 shows that the authors trust their values of depolarization are accurate within ±0.02, which is quite precise. Since the CONIOPOL algorithm is threshold-based, documenting the variability of depolarization would help the readers understand the reliability of classification output.

We thank the reviewer for this important comment. We agree that documenting the variability of the measured VDR is particularly important for a threshold-based classification scheme such as CONIOPOL.

In the revised manuscript, we have therefore expanded the characterization of the VDR variability under the temporal and vertical resolution used by CONIOPOL. In particular, the new Fig. 6 presents the vertical distribution of VDR during precipitation events, including the median, interquartile range, and 90th percentile for both warm- and cold-season conditions. This additional analysis illustrates the observed VDR variability within different hydrometeor regimes and provides the observational basis for the precipitation-phase thresholds adopted in CONIOPOL.

In addition, following this comment and the related recommendation concerning classification uncertainty, we have added a dedicated sensitivity analysis (Sect. 4.7; Figs. A1 and A2) to explicitly quantify how VDR variability propagates into the CONIOPOL classification. Absolute VDR perturbations of ±0.01 and ±0.02 were applied to the measurements, and the resulting classifications were compared pixel by pixel with the reference classification. The ±0.01 and ±0.02 values were derived from the variability observed at the base of liquid-water clouds (Fig. 1), where very low depolarization is expected. We emphasize in the revised manuscript that these values are used as controlled sensitivity scenarios and should not be interpreted as a uniform measurement uncertainty applicable to all VDR values or target types.

The sensitivity analysis shows that VDR is the dominant source of sensitivity among the optical parameters investigated. However, classification changes occur primarily for observations located close to the fixed VDR decision thresholds and predominantly involve transitions between neighbouring classes rather than arbitrary changes across the classification scheme. This behaviour is now explicitly discussed in the revised manuscript and provides a quantitative assessment of the reliability and limitations of the threshold-based classification.

We believe that the combination of the additional characterization of observed VDR variability in Fig. 6 and the dedicated sensitivity experiments now provides a substantially clearer assessment of how depolarization variability affects the CONIOPOL classification.

L234-237: I understand that the threshold of attenuated backscatter used to separate aerosols from clouds is itself based on distributions of attenuated backscatters observed with and without clouds. This sounds circular to me. I don't understand how the presence of clouds is decided in the first place? Is it based on other observations, or on the lidar measurements themselves? If so, cloud detection must be based on some kind of signal threshold. Why not use it to inform the design of the CONIOPOL algorithm?

We thank the reviewer for this valuable comment. We agree that the original wording was ambiguous and could give the impression that the cloud/no-cloud classification was derived using the same threshold as that employed in CONIOPOL, leading to an apparent circularity.

This is not the case. The cloud base height (CBH) used in CONIOPOL is taken directly from CL61 product and is treated as an external input to the algorithm. The cloud/no-cloud separation used to derive the attenuated backscatter threshold is therefore based on the presence or absence of a valid CBH retrieval, rather than on the threshold being defined in this study.

To clarify this point, we have revised the manuscript as follows: “The threshold for distinguishing aerosols from clouds and precipitation is determined empirically from the relative frequency distributions of attenuated backscatter values conditioned on the presence or absence of CBH retrieval from CL61 product. Relative frequency distributions are constructed for both regimes, and the separation threshold is defined where their ratio exceeds unity. This provides a statistically based criterion independent of the CONIOPOL processing chain, which uses CBH as an external input."

L. 278 and below: "LDR... cannot uniquely identify aerosol type on its own (...) aerosols are thus classified into an explicit category". I am confused by this section, and in particular this paragraph. First it states that LDR cannot be used to explicitly identify aerosols, since different aerosols can generate overlapping LDR distributions. It then concludes that aerosols are classified using explicit categories, which I understand to be in total contradiction with what was explained before. I understand that methodological choices are made to minimize misclassifications, but minimizing misclassifications is not the same as eliminating misclassifications. I find this section even more puzzling further down, when it addresses the fact that "dust and volcanic ash exhibit overlapping LDR values" but somehow argues that it is still possible (and even mandatory) to treat them as distinct categories. From these elements I would argue that the algorithm should, instead, treat them as the same category ("dust or volcanic ash"). Please make your point clearer here.

We thank the reviewer for this comment. This point is closely related to the reviewer's general concern regarding the interpretation of the aerosol classification framework, which has been addressed through a substantial revision of Section 4.4.

As clarified in the revised manuscript, the proposed classes should not be interpreted as unique aerosol identifications based solely on PDR. Rather, they represent aerosol regimes defined using physically motivated thresholds, while explicitly acknowledging that overlapping depolarization signatures may lead to ambiguous classifications. Transitional classes have been introduced where appropriate, and the text now emphasizes that complementary information (e.g. source attribution, air-mass back trajectories, or additional optical parameters) is required to unambiguously distinguish aerosol types with overlapping VDR values, such as mineral dust and volcanic ash.

We believe that the revised Section 4.4 resolves the apparent contradiction identified by the reviewer and better explains the rationale behind the proposed classification scheme.

L. 289-294: overlapping signals for different aerosol species could be taken into account by attributing a probability for each species, for instance depending on the LDR distribution of a given aerosol type. The overlapping part could also be labelled as "volcanic ash or dust", while the clearly distinct parts could be labelled "ash" and "dust".

We thank the reviewer again for this comment. As already mentioned, the revised manuscript clarifies that the CONIOPOL scheme explicitly distinguishes between pure and mixed categories when the VDR does not unambiguously correspond to a single aerosol type. These intermediate categories are introduced to account for overlapping depolarization signatures between different aerosol types.

A fully probabilistic attribution of aerosol species is not implemented in the current version of the algorithm, which remains based on deterministic thresholds.

L. 337-345: The article does not include the necessary information to understand what is being explained here. What exactly is the input of DBSCAN? It is "applied to a 2D grid" of what? What is the output of this algorithm? Is the DBSCAN algorithm part of the design of the CONIOPOL algorithm? If so, why isn't it mentioned in Figure 1? What is the principle of that DBSCAN algorithm? Why is it helpful?

Thank you for this comment. We agree that the original description may have suggested a closer connection to DBSCAN than is actually the case. The procedure implemented in CONIOPOL is not a formal DBSCAN clustering algorithm. Instead, it consists of a two-dimensional sliding-window consistency filter applied to the CONIOPOL detections in the time–height domain.

The algorithm operates on successive 5 × 5 windows comprising five consecutive profiles and five adjacent range gates, corresponding to 150 s in time and 24 m in the vertical dimension. Within each window, the occurrence frequency of each aerosol category is computed. Categories with a relative occurrence frequency greater than 0.4 are considered representative of a coherent local atmospheric structure, while isolated detections are suppressed. The maximum density category (MDC), defined as the category with the highest occurrence frequency within the window, is retained as the locally dominant aerosol type. If no category exceeds the threshold, the corresponding observation is considered indeterminate and no category is assigned.

The purpose of this procedure is therefore not to identify geometrically connected clusters, as in DBSCAN, but to improve the spatial and temporal consistency of the CONIOPOL classification by exploiting information from adjacent observations while accounting for the natural heterogeneity of atmospheric aerosol layers. To avoid confusion, we have revised Section 4.5 by removing the explicit reference to DBSCAN and replacing it with a complete description of the implemented sliding-window consistency filter.

L. 399-402: I don't understand the point being made here.

We thank the reviewer for pointing out that these sentences were unclear. In the original manuscript, this discussion was intended to demonstrate that the different aerosol and cloud categories did not exhibit a common background occurrence, thereby suggesting limited overlap between the predefined classes.

However, the revised version of CONIOPOL now explicitly introduces (intermediate classification to account for situations in which multiple categories coexist within the same neighbourhood. This new framework directly addresses potential overlaps between classes, making the previous discussion redundant and potentially confusing.

To improve the clarity of the manuscript, we have therefore removed these sentences from the revised version.

L. 454: "model simulations" my understanding is that CAMS forecast are not mere model simulations, but analyses that merge (through numerical assimilation) model simulations with observations of the atmosphere. One goal of CONIOPOL retrievals could be their assimilation in CAMS.

We thank the reviewer for this valuable comment. We agree that the term "model simulations" was not sufficiently precise in this context. The manuscript has therefore been revised to refer to the CAMS Global Forecast or, more generally, to an independent atmospheric composition model, rather than to simple model simulations. In addition, the CAMS comparison section has been substantially revised.

Figure 8: I don't understand how the frequencies shown here were calculated. Is it the ratio between the number of profiles containing a given particle type, divided by the number of profiles sampled over

the course of a given month? If so, the extremely small number of ice crystals is surprising. Ice clouds are fairly frequent throughout the year.

We thank the reviewer for this comment. For each category, the monthly occurrence was calculated as the percentage of hourly profiles in which the corresponding category was detected in at least 60% of the observations within that hour, relative to the total number of hourly profiles acquired during the month. This value is expressed as a percentage. We have clarified these points in the revised manuscript. We further clarified that multiple categories can be detected within the same profile due to the vertical coexistence of different aerosol and hydrometeor types. As a result, occurrences are not mutually exclusive and the sum of all category occurrences may exceed 100%.

Regarding the relatively low occurrence of ice crystals, this result should be interpreted in the context of the vertical range considered in the original analysis. The occurrence statistics were initially restricted to altitudes below 5000 m a.g.l., thereby excluding a large fraction of high-level ice clouds. In the revised manuscript, the occurrence analysis has therefore been extended to the full CL61 measurement range (up to 15.4 km a.g.l.) to provide a more comprehensive overview of cloud detection throughout the atmospheric column.

Figure 9: It would have been interesting to see the evolution of the cumulative fraction of ice within clouds (compared to ice + liquid) with temperature. One would expect it to remain constant with time.

We thank the reviewer for this interesting suggestion. We agree that analysing the fraction of ice within clouds as a function of temperature would provide an additional and complementary assessment of the cloud-phase classification. In the present manuscript, we chose to assess the physical consistency of the cloud classification through the normalized cumulative occurrence profiles. These profiles already show the expected vertical stratification of cloud phase, with Liquid Clouds predominantly confined to the lower troposphere, Mixed Clouds occupying the transition region, and Ice Crystals extending to the upper troposphere. Since atmospheric temperature generally decreases with altitude, these results provide an indirect indication of the physical consistency of the cloud-phase classification. We agree, however, that an explicit temperature-dependent analysis using collocated temperature profiles would constitute a valuable extension of this work and will be considered in a future study.

Fig. 10 B and D : these subplots are not particularly useful. The small number of values they show could be provided directly in the text, or integrated within subplots A and C.

Figures 10 have been replaced in the revised manuscript by a more detailed altitude-resolved analysis.

Fig. 11: I do not see any significant difference between the values shown in the left column (detection of smoke /marine) and the values shown in the right column (no detection). Please clarify in the text what you mean by "clear differences emerge" or find a way to make these differences clearer visually.

Figures 11 have been replaced in the revised manuscript by a more detailed altitude-resolved analysis.

L. 684 : "EarthCARE's ATLID polarisation lidar will provide vertically resolved particle-type information that single-wavelengths ALC cannot retrieve" -- ATLID itself is single-wavelength. Its added value lies in its high spectral resolution, which is able to separate particulate from molecular backscatter (and thus simplifies lidar ratio retrievals). ATLID being in orbit since june 2024, its aerosol type retrievals are available today, and comparisons between the CONIOPOL output presented here and ATLID products could be made today by identifying a few ATLID overpasses of Uccle.

Thank you very much for the correction. I confused it with CALIOP… We have added the relevant details in the revised manuscript.