

Review for the paper ‘Examining the characteristics of aerosols: a statistical analysis based on a decade of lidar and photometer observations at the Eastern border of ACTRIS’ by Nicolae et al.

The paper examines aerosol characterisation at the RADO-Bucharest station in Romania, part of ACTRIS. The authors use sun/sky/lunar photometer and lidar measurements, combined with the NATALI neural network, to distinguish between aerosol types in the lower troposphere and above the boundary layer, and FLEXPART retro-plume analysis to assess potential sources.

However, the manuscript is difficult to follow, reads more like a descriptive study or technical report than a scientific paper, and requires **major revision**. The introduction is excessively long, ending with only a single sentence that addresses the paper’s objective. Lines 57–73, which provide basic definitions of aerosol properties, should be removed or drastically shortened, as this information is already well known to the community. Up to line 137, the text remains overly descriptive and does not help the reader understand the actual objective of the study. The introduction also ends abruptly, with no clear transition to the methodology or explanation of how the objective will be achieved. Leaving such information entirely to the methodology section is not acceptable.

How are the ACTRIS-CARS protocols relevant to the objectives of the paper, and why were they included? Likewise, the extensive discussion of GARRLiC/GRASP is misleading, since these methods are not applied in the study. As written, the introduction gives the impression that the analysis will rely on a synergy of photometer and lidar measurements processed with GRASP, which is not the case.

The methodology section is quite lengthy and lacks subchapters dedicated to the different instruments or methods used, which would help make it more accessible and easier to follow. Why is so much space devoted to describing the instruments and retrieval methods? Photometers and lidars are well-established, and their detailed characterisation has already been extensively documented in the literature. This level of detail is unnecessary here. You should limit the description to the basics and only add specific information if there are instrumental modifications unique to the RADO-Bucharest site.

Regarding FLEXPART, the resolution of the meteorological input data is not specified and should be clearly stated. Furthermore, given that this paper is submitted to an ACP/AMT special issue, the description of the model setup is far too superficial. Details on the so-called “*unique turbulence model*”, the wet deposition scheme, and the parameterisation of gravitational settling are missing and need to be explicitly described. Lines 290-295 could be removed.

A general issue in the results section is the poor quality of the figures. All figures require improvement, with clearer scales, larger fonts, and properly labelled axes. All figures and

captions should be improved with the use of a), b), c) etc. instead of ‘left panel’, etc. In the text as well, when discussing the figures, they should be in line with the ACP/AMT requirements of Fig. a) in the text, or Figure a) when at the beginning of the sentence.

The use of the term “*polluted*” as an aerosol category is imprecise and should be avoided; more appropriate terminology would be *urban/industrial* or *anthropogenic*. Current literature generally recognises the following aerosol types: marine, dust, smoke (biomass burning), urban/industrial, continental (clean), and mixed (internal/external mixtures of these types). However, your use of *continental* is left to interpretation. In the classic OPAC climatology (Hess et al., 1998), three separate continental classes are defined: continental clean (background continental air with relatively low AOD, dominated by natural aerosols), continental polluted (continental air strongly influenced by anthropogenic components such as sulphate, nitrate, OC, and BC), and continental mixed. Using *continental* without clarification introduces confusion and must be explicitly defined in your classification.

What is Tight Continental?

You mention: *A significant drop in the Continental aerosol type was observed in 2018 (Fig. 3 right upper panel), which can be explained by the higher number of datasets collected during the summer months.* Weighting annual aerosol fractions by the number of measurements per month or year is not recommended, as it introduces bias toward months with more observations. A better approach is to use equal-month or equal-season weighting, which gives each period equal influence, and to report 95% confidence intervals to quantify uncertainty. Additionally, include a table of N per month/season/year so readers can evaluate the support for each estimate. This approach will clarify whether the 2018 decrease in ‘Continental’ aerosols and the pronounced 2020 increase reflect genuine changes or are artefacts, considering that 2020 measurements may have been influenced by pandemic-related reductions in pollution and uneven sampling

Looking at Figure 4, your statement that “*In 2021, a particularly long Marine event was recorded, indicative of air masses originating from oceanic regions, bringing a distinct aerosol composition*” is not supported by the daily time series shown. I cannot identify any extended Marine event in 2021 from the figure. Could you clarify what this conclusion is based on? Did you mean 2024?

For clarification, you remove the HT cases when there’s no layer below the PBL (because you mention ‘*all the layers that are located above the first layer*’)?

In the interpretation of Figure 6, there is an inconsistency in the statistics: you discuss the **median** for LT but the **mean** for HT; please clarify which metric is being used. Additionally, the reported lidar ratios of 48 vs. 49 do not constitute a meaningful separation in aerosol composition. Therefore, the statement that these values “*suggest varying optical properties*” in the abstract is an overstatement and should be revised to reflect the limited distinction.

I can understand dust and smoke as predominant types in the HT, but what about the continental clean? The reported ~50% fraction appears unexpectedly high. Please clarify whether this reflects actual aerosol composition, or if it could result from limitations of the classification algorithm, low-concentration background aerosols, or misclassification of mixed layers. Neural networks like NATALI or other aerosol typing algorithms can sometimes misclassify mixed or low-concentration aerosols as ‘continental clean’, especially at high altitudes where signal-to-noise is lower.

Figure 9 requires improvement. You refer to frequency, but no actual numbers are provided; the current scale leaves too much open to interpretation. Please include counts or percentages to make the data interpretable.

In addition, Figure 10 is presented without a description or interpretation. It is not the reader’s task to disentangle the meaning of the plots. The explanation should clarify how depolarisation is used to differentiate between aged and fresh smoke, and this should be introduced first. Only after that should you discuss the role of altitude, Ångström exponent, and lidar ratio.

The current Section 3.3 is too brief and largely descriptive. Currently, it only paraphrases what is already visible in Fig. 11 and does not sufficiently leverage the potential of the FLEXPART simulations. As a result, the section lacks depth, fails to connect with the observational findings, and does not convincingly demonstrate the added value of FLEXPART in the study. To improve the scientific quality and readability of this section, I recommend the following **mandatory revisions**:

- The text currently states “Europe is the main source” or “Sahara becomes significant in the HT” without numbers. Please provide quantitative results (e.g., mean seasonal percentage contribution  $\pm$  standard deviation) for each source region and for both LT and HT clusters. A summary table in the Supplement would also be very useful. Fig. 11 is not sufficiently informative on its own. The term “distribution” remains qualitative without actual values.
- The choice to split retroplume clusters at 2 km altitude is arbitrary and inconsistent with your own discussion of the PBL, where a climatological mean of ~1300 m was already established. If you intend to use 2 km as a threshold, this requires justification. For instance:
  - Why is 2 km chosen rather than the mean PBL height (1.3 km)?
  - Does 2 km correspond to a standard practice in FLEXPART studies? Please cite.
- Similarly, the definition of source regions is not clear. Please show a **map with the spatial masks** used for Europe, Sahara, North Africa, etc., so that the classification is reproducible.

- The methodology section describes improvements in FLEXPART physics (turbulence, wet deposition, ERA5 input), but none of these are discussed in the results. How did including wet deposition or the new turbulence scheme affect their retroplumes compared to earlier FLEXPART studies?
- FLEXPART analyses cannot be interpreted in isolation. You should discuss the prevailing synoptic conditions (e.g., seasonal circulation, anticyclonic vs cyclonic regimes, vertical transport patterns) that explain the seasonal differences in source contributions.
- At present, FLEXPART results are disconnected from the lidar/photometer data. Please explicitly connect transport simulations with the observed aerosol types. (Example: *FLEXPART indicates HT contributions from the Sahara in spring, consistent with lidar-observed dust layers (high depolarisation ratio)*).

Unless you address these issues with substantial revisions, this section does not contribute meaningful scientific insight and should be removed from the paper.

The final paragraph from Chapter 4 is overly general and does not provide a critical perspective. Please expand it to include a discussion of the broader implications of your findings, potential limitations of your study, and directions for future work. For example, how could this work inform improved aerosol modelling, observational networks, or policy-relevant assessments of air quality and climate?

Specific

comments:

Revise your citations. When citing, correct is '*Nicolae et al. (2023) have found ...*' not '*(Nicolae et al., 2023) have found...*'. E.g. lines 130, 165, 213 etc

Improve the abstract.

Improve all figures.

Line 93: 'from the'

Line 228: cite the OPAC database

Line 244: review the aerosol type classification