

Response to Referee 1

First of all, we would like to thank the Referee for careful reading the manuscript and for numerous useful comments. We followed his recommendations in the process of manuscript revision.

The paper is well written and appropriate for AMT. It is an excellent contribution the lidar literature.

I have only minor points. Since, many publications have been written and published by the Lille lidar team on the application of polarization/fluorescence lidar in atmospheric aerosol research during the last few years, the authors should make very clear (in the Introduction) what is the step forward here (not covered by the foregoing papers). Only by 1:1 comparison with the paper of Veselovskii et al. (AMT, 2022), I saw the difference. The layout of many figures in the new publication (2024) has not changed compared to the 2022 paper. The motivation is given in the Introduction, but not well enough. A more contrasting wording would be helpful. Maybe, start to review the 2022 paper and then state what was missing so far, and this gap is now filled with the 2024 publication.

We agree with this comment and in revised manuscript the Introduction was modified, to make clear the novelty of this manuscript comparing to our publication in 2022.

Individual remarks:

Section 2.2 is new, and that must be better highlighted in the Introduction.
Done

After line 142 it becomes quite complicated (without a nice flow chart of all the steps...). One could even start to explain (step by step) the respective three-aerosol-component separation approach before one continues with the discussion of the methodology in case of the four-component system.

We tried to skip the mathematical details of the system solving and in the revised manuscript, we tried to make it easier for reader.

Table 1: The numbers now differ a bit from the ones in Table 1 in Veselovskii et al. (2022). Should be explained!

Yes, the ranges of parameters variation changed a little, comparing to our 2022 paper, because numerous measurements were performed since that publication, providing more information for establishing these ranges. Also, we limited the ranges by the values which we normally observed in the low and middle troposphere. Comment is inserted in the manuscript.

Smoke depol values from 2.0-8.0! Does that cover the full range of values. Baars et al. (2019) or Ohneiser et al (2020) show values up to 20% at 532 nm.

High depolarization ratios of smoke at 532 nm are usually observed in the upper troposphere, while in this manuscript we analyze measurements in low in middle troposphere. For the same reason we limited the maximal value of the fluorescence capacity of smoke at $4.5 \cdot 10^{-4}$, though higher values were observed in upper troposphere.

Depol values of 2-8% in the case of urban aerosol! Does this range of values (up to 8%) include road dust? Why should there be a depol ratio of >5% in the case of a sulfate-aerosol dominating aerosol?

The depolarization of urban particles up to 8% we regularly observe over Lille. Contribution of road dust and soil is possible, but at present stage we are not ready to discriminate it.

Page 8, lines 209-210: Please provide reference to Veselovskii et al. (2022).
Added

Page 8, line 223: Spain? I do not see that! You mean: Italy?
Corrected.

Page 8, line 244: η -S = 0.1 and not 1.0
Corrected

Page 9, line 250 ... from the free troposphere ... By the way, the 1 October 2023 smoke event was a UNIQUE event. It is almost impossible to find North American smoke so close to the ground. I hope there will be another paper on this UNIQUE observation.

Yes, we have plans for such paper

Page 10, line 281-284: I would step forward to mass concentration! Particle densities are 1.15 g/cm³ (smoke), 2.6 g/cm³ (mineral dust), and 1.5g/cm³ (sulfate aerosol). These numbers are given in the referenced papers.
Done

Table 2: regarding aged smoke, I would cite own papers as well (Hu et al.,)
Added

Section 4: Why is that an extra section and not simply a subsection of section 3? Please provide a small introduction why you present and discuss this episode separately.

We agree with reviewer. Section 4 now is subsection of section 3.

Page 12, line 340: When was the heat wave over? Should be mentioned!
Added

And then, please provide mass concentrations instead of volume concentrations in Fig. 17.

We changed Fig.17 and added profiles of mass concentration. Corresponding paragraph is also added to the text.

Acknowledgement: A statement concerning ACTRIS is missing, but required to my opinion.
Added

Fig. 2, caption: 350-2800 m.
Done

Fig. 12, caption: Maybe in line 589: ... depict the total particle volume....
Done

General remark to the figures: There should be always (a) (b) (c) when there are several panels. Sometimes it is written (a) ... and (b) ... in the caption, but no indication of panels in terms of (a) and (b).

Done