Response to the reviewers:

The authors are very grateful to the reviewers for their critical remarks and suggestions which allowed a thorough revision and a substantial improvement of the manuscript. The revised version of the paper with changes highlighted is included at the end of this response letter. Below you can find our point-by-point reply to the reviewers (our responses with green color). We note that both reviewers commented on the use of notation and the Rayleigh calibration and this will be addressed in both responses.

Note that line numbers indicated below refer to the revised manuscript.

Reviewer 1:

R1/C1 One should follow the notation (letters for parameters, definitions, etc.) as used in Freudenthaler (Atmos. Meas. Tech., 9, 4181–4255, https://doi.org/10.5194/amt-9-4181-2016, 2016), Belegante et al. (2018), and Bravo-Aranda (2016). All these papers are given in the reference list. It is confusing for lidar experts, familiar with these fundamental papers, when gamma and epsilon is used here in a different way as in these standard papers. Another example, D is used in the three mentioned papers in the context of diattenuation (consideration, corrections), here, in this manuscript D1, D2, Dref and Dm are used for very different parameters. When the same notation as in Freudenthaler (2016) is used, reading and comparisons are facilitated. Furthermore, it is then much more easy to discuss gaps in the approach presented in this manuscript under review.

Thank you for the suggestions. Please note that our paper doesn't stem from Freudenthaler's 2009 or 2016 approaches, and instead it introduces a simplified model utilizing atmospheric principles, to describe the issues that affect depolarization measurements. It is important to acknowledge also that different terminologies and notations exist among various papers discussing this topic. For instance, in Freudenthaler's work, calibration factor V* in the 2009 publication was later referred to as η* in the subsequent 2016 paper. Similarly, different terminologies referencing the same topic can be observed in studies authored by e.g. Cairo F. 1999, Roy G. et al 2011, Marenco and Hogan 2011, Chazette et al 2012. There is therefore no uniformly accepted notation and even a same author changes the symbols used when publishing different papers. We approach here the problem from a top-down perspective (from what we observe and how we can address it, and without a full knowledge of the internals of the lidar system) as opposed for example to the bottom-up approach of Freudenthaler (based on detailed knowledge of each instrumental component). On the other hand, we agree on the importance of terminology and notation: consistency could facilitate readability and comparisons. The notation that we use in the manuscript reflects the top-down approach to the cross-talk problem. We however agree that the way we were using the symbol D in the previous version was confusing, and we note that a majority of papers use δ for volume depolarization ratio: thus, we have adopted this convention in the newly submitted version and accordingly modified the content of our paper (see equations 3- 10 and 16). In addition as letters γ and ε are used in Freudenthaler 2016 paper to describe orthogonal orientations of individual elements in the lidar set-up, we change these symbols in our paper to g and e, respectively (see equations 5-12, and 15-16). We also provide two appendices showing how our notation relates to Freudenthaler 2009 (appendix A) and 2016 (appendix B, newly added) bottom-up treatment of the same problem.

R1/C2 The +/-45 deg calibration is now commonly denoted as Δ 90 deg calibration. All in all, please follow the way (and notations) given by Freudenthaler (2016) as much as possible. Freudenthaler et al (2009) is no longer a good guide. Meanwhile , the number of influencing quantities and parameters has significantly increased.

Thank you for pointing this out. As +/-45 deg calibration is now commonly denoted as Δ 90 deg calibration we adopted this notation throughout the paper. See, e.g. lines 166,278, Table 2 etc.

R1/C3 I have the feeling, it would be good if the authors study again these fundamental papers of Freudenthaler, Belegante et al., and Bravo-Aranada et al., and consider the main messages in the introduction or later in the discussion section. Even in case of a quite poor depolarization lidar (as this CIMEL lidar of the Cyprus Institute), one should at least discuss the full framework of a properly designed and properly working depolarization lidar, i.e., one should not ignore the state-of-the-art. For example, have a look into the Belegante et al paper in which the diattenuation of the receiver optics is nicely described. Please do not omit all these features in your manuscript with the argument: It is not really necessary to mention that in case of observations with the 'black box' CIMEL lidar. The approach presented here is quite simple (basic) and describes the way to handle a poor and simple lidar with many unknown features so that the lidar cannot be characterized properly. One could ask, do we need a manuscript that describes the status of depolarization ratio observations performed 20 years ago?

We thank the reviewer for suggesting this. We revisited the works by Freudenthaler, Belegante et al., and Bravo-Aranada et al., and we have highlighted the significance of these studies in our introduction (lines 69-73). While acknowledging the importance of these studies in detailing depolarization lidar calibration techniques, it's important to note that our proposed method aims to address retrospective correction for previously acquired data rather than offer solutions for real-time data acquisition.

We note here the comments of the reviewer about our lidar system in the following terms: "a quite poor depolarization lidar as this CIMEL lidar of the Cyprus Institute", "the black box CIMEL lidar", "a poor and simple lidar with many unknown features so that the lidar cannot be characterised properly". The reviewer finally goes to ask " do we need a manuscript that describes the status of depolarization ratio observations performed 20 years ago?" May we say that we disagree with the reviewer, see our detailed response to this point under C13 below. We disagree because with the present paper we demonstrate that we can characterise this lidar system and we can extract good observations out of it. Yes, we recognise that there is some internal of the lidar system that is unknown, and this is why we chose the present approach: but it is the role of science to address unknowns in the most efficient way, and not to judge those unknowns as "something bad". We believe that the comment of the reviewer adds additional reasons why our paper is useful: we demonstrate that a lidar system can be characterised through atmospheric observations and in retrospect, using a top-down approach (i.e. on what we observe), as opposed to a bottom-up approach based on detailed knowledge of its internals. This could save other campaigns facing such issues with other lidar systems. And the reviewer’s surprise ("well and carefully performed analysis, the authors did", see R1/C13) shows that our technique deserves publication.

R1/C4 The figures are not just in a good quality. E.g., many text parts (x-axis and y-axis) are almost not readable (rather small).

Thank you for your comment. In the revised document we have improved the Figures which were not clear (See also R1/C20). For example we added colorbars in Fig.4 and improved the text quality in Figs. 7 and 9

R1/C5 Concerning the Polly lidar, a reference to the basic PollyNET paper of Baars et al. (ACP, 2016) is recommended. The lidar is described by Engelmann et al. (2016), but also by Jimenez et al. (ACP, 2020, part 2). Hofer et al. (ACP, 2017, 2020a, 2020b) used this lidar type for dust studies in Tajikistan.

Thank you for the citation recommendations. We have incorporated the references suggested by the reviewer at lines 201 and 207-209.

R1/C6 Polly seems to be used for dust studies at Cyprus since almost 10 years (since 2015), besides the Raymetrics lidar since 2011-2012, if I follow the literature of Mamouri and Nisantzi. It should be mentioned that there is already expertise in this field of atmospheric science since more than 10 years, respective papers should be cited to corroborate that. / There are many Limassol lidar papers (lidar/photmeter CUT team) on dust observations that could be cited to provide the reader with background information about the long-term experience of Cyprus lidar scientists in this field of dust research.

Thank you for reminding us of the extensive use lidar systems in dust studies in Cyprus. In our revised manuscript, we have incorporated citations authored by Mamouri, Nisantzi, Kezoudi and Mamali showcasing the long-standing engagement and expertise of Cyprus-based researchers in atmospheric science, particularly in dust-related studies using lidar remote sensing. These citations were added in lines 27-37.

R1/C7 The three-parameter approach (and also the two-parameter approach) partly leads to negative results (depolarization ratios). Too much co-polarized signal is obviously subtracted from the cross-polarized channel output (signal), because your gamma is obviously too large.

The minimum should be the Rayleigh depolarization ratio after all corrections. That sounds trivial but this needs to be mentioned. Please comment on this point!

Thank you for your comment. We have noted that the two and three-parameter approach, result in negative values at times, in the non or low-depolarising ranges. We agree with the indication that this probably occurs due to a possibly large gamma (now called g) parameter, and we have added comments in the article to mention it, as suggested by the reviewer. Upon assessing uncertainties on the g and e parameters for both methods (Appendix D) we find that the negative values are compatible with a small positive VDR within the uncertainty bounds. This is further explained in comments R1/C19 and R2/C7. In the revised document we added this explanation in lines: 288-291, 297-304 and 331-339.

R1/C8 'Did you use the Polly Rayleigh depolarization ratios to correct the CIMEL lidar observations? I have the impression, that is the case. The Polly Rayleigh depolarization ratios are too my opinion very high. The 532 nm values should be around 1.1% when I check other papers with Polly data.'

Thank you for the comment. To clarify, our method does not rely on the Polly XT for the Rayleigh-scattering layers but only for the highly depolarizing (dust and/or cirrus) layers. In our analysis, we employed computed values for the Rayleigh depolarization ratios taking into account the instrumental specifications of the CIMEL CE376 (receiver bandwidth), according to Behrendt and Nakamura (2002), thus adopting the volume depolarization ratio of 3.6E-3. This approach is now better detailed in our manuscript, specifically explained at lines 174-175. The use of the molecular value according to Behrendt and Nakamura (2002) is also explained in lines 266-268.

The use of a system-dependent computed value of δ_m (for Eq. 12) is unavoidable because the Rayleigh Volume Depolarization Ratio (VDR) varies across different lidars (depending on the instrument's receiver bandwidth). Only by employing computed values, we can minimize potential errors that could influence the accuracy of our corrections. See also comment R2/C3. Finally thanks to the reviewer suggestion on Polly molecular VDR values, the PollyXT data has been revised and now the Rayleigh VDR is close to 1.1 %, as described in our answer to comment R1/C9.

R1/C9 'In addition, the Polly description is too short. Meanwhile, to my knowledge (from several workshops with Polly presentations) they use diod-pumped lasers with 100 Hz rep rate (Cabo Verde, Tajikistan, Cyprus). It should also be mentioned that the total signal and the cross-polarized signal (Engelmann et al., 2016, Jimenez et al., 2020) are measured, and not the co and cross polarized signal components. It is not needed to explain the way to obtain the depol ratio, one may cite Engelmann et al. (2016), but the reader should know. However, one needs more details to the Polly data analysis (towards depolarization ratios) used in this study: Polly has automated Δ 90 deg calibration units, right? How often (per day? per week?) What is explicitly done in the two shown measurement cases? What v-star is used? As mentioned, the Rayleigh depol value seems to be very high with 2.2%! How was this Rayleigh value obtained (for what v-star, derived from how many delta 90 deg observations)? What about the used transmissivity ratios in the case of the Polly data analysis? Where the TROPOS lidar experts (with their long-term experience) involved in the data analysis?'

Thank you for your insightful questions regarding the PollyXT data analysis in our study. You are right, the PollyXT detects the total scatter light (all polarization planes) and the cross-polarized light. To characterize the depolarization, PollyXT performs automated Δ 90-degree calibration twice per day (at 02:30 and 16:50 UTC). The calibration is automatically analyzed within the PollyNET Processing Chain (Baars, 2016, Yin and Baars, 2021). This information is added in the revised document in lines 204-209.

However, after double-checking following the reviewer comment R1/C8 about the Rayleigh depolarization ratio, we realized that in the previous version of the data, the measured transmission ratios for the total and cross channel that had been used for the calculation of the volume depolarization ratio were not the most optimal. With these measured values, effects in the detection unit could be accounted for, but impurities in the polarization state of the emitted light and/or polarization effects at the telescope mirror could not be accounted for. Thus, we revised the whole PollyXT dataset by applying more optimal effective transmission ratios based on long term observation of the volume depolarization ratio in particle-free regions. The revised values are the ones that have been submitted together with the ACTRIS quality assurance test (made in April 2021) to the ACTRIS Center of Aerosol Remote Sensing (CARS) in the frame of the CAMS2_21b project which aimed for the delivery of lidar data to the Copernicus Atmosphere Monitoring Service (CAMS) in near real time for data assimilation.
We now use these effective transmission ratios for the calculation of the volume depolarization ratio within the PollyNET Processing Chain. As a result of the revision of the PollyXT dataset, the profiles of volume depolarization ratio have changed only slightly, but indeed, the volume depolarization ratio in particle-free regions is now close to 1% as the reviewer has observed in other publications (comment R1/C8).
Thus, we thank the reviewer very much for pointing us to this issue so as to optimize our datasets to the best quality. We note that the message of this publication and the main outcomes (VDR of the CIMEL lidar after characterization) have not changed substantially, but only slightly. Specifically, the VDR at dust layers changed from $\delta_2 = 0.090 \pm 0.008$ to $\delta_2 = 0.083 \pm 0.001$ for the 26 Nov 2021 22:00 UTC and from $\delta_2 = 0.17 \pm 0.03$ to $\delta_2 = 0.16 \pm 0.01$ for 16 Feb 21:00 2022 UTC.

Zhenping Yin, & Holger Baars. (2021). PollyNETHYPERLINK "https://doi.org/10.5281/zenodo.5571289" /Pollynet_Processing_Chain: Version 3.0 (v3.0). Zenodo. https://doi.org/10.5281/zenodo.5571289

Baars, H., et al.: An overview of the first decade of Polly[NET]: an emerging network of automated Raman-polarization lidars for continuous aerosol profiling, Atmos. Chem. Phys., 16, 5111–5137, https://doi.org/10.5194/acp-16-5111-2016, 2016.

R1/C10 ‘If possible, please provide absolute values for D1, D2, Dref, and Dm, not just differences. It would be good to have more absolute values! And this not only for dust layers, but also for Rayleigh height zones.’

Yes, this is a good idea, thank you. We have included the absolute values for δ1, δ2, δref, and δm, encompassing both the dust layers and the Rayleigh height zones, as suggested by the reviewer (lines: 280-281 and 285-287. Note that the symbol δ is used in the revised manuscript, instead of D that was used in the previously submitted version.

R1/C11 ’ There were several field campaigns (CyCARE, ALIFE) with strong contributions by depol lidars. That should be emphasized. Floutsi et al. (Atmos. Meas. Tech., 16, 2353–2379, https://doi.org/10.5194/amt-16-2353-2023) uses CyCARE and ALIFE data and mention the campaigns. If there is no better reference…. then use Floutsi et al.’

Thank you for reminding us of the significance of the CyCARE and ALIFE field campaigns involving depol lidars on the island. As per your suggestion, we have incorporated the mention of these campaigns in our manuscript (lines: 34-37). Additionally, we have cited two papers that discuss these campaigns: 1) Ansmann et al., 2019 (https://doi.org/10.5194/acp-19-15087-2019) and 2) Floutsi et al., 2023 (the latter as per the reviewer’s suggestion, https://doi.org/10.5194/amt-16-2353-2023).

R1/C12 ‘*Measurements were not only conducted within the troposphere, even in the stratosphere over Cyprus (Baars et al., ACP, 2019, Canadian wildfire smoke). Should be mentioned to corroborate the importance of the location of Cyprus in the Eastern Mediterranea*n.’

Thank you for highlighting the importance of Cyprus location. We've combined this comment with R1/C6 and added a paragraph in our manuscript (Line: 27-37), to highlight Cyprus' important location in the Eastern Mediterranean.

R1/C13 ‘*I am surprised that such a bad lidar can deliver still useful results…! Well and carefully performed analysis, the authors did!*’

Many thanks to the reviewer for appreciating our analysis and effort to better characterize the measurements with the CIMEL CE376. We believe, however, that there exists no such thing as a “good” or “bad” lidar, but only good or bad use of an instrument. Instrument limitations are inherent, and the important is to know how to address them: this is the purpose of the current paper, which extensively addresses how to obtain improved depolarization measurements. We know that scientists need to understand their instruments before drawing scientific conclusions. Perhaps the reviewer has it clear in their mind what is a “good” instrument, and perhaps this is an instrument that does everything correctly, by following a standard procedure or in an automated way, so that a lidar scientist is no longer required to interpret the data? Well, if we can make a comparison with everyday life, one can drive a Ferrari, a Citroën 2CV or a bicycle, and we all agree that to ensure that that they will arrive safely at destination, the most important is not the vehicle but the driver. There is no point in having only "Ferrari" lidars, for example, if the number of stations that can be sustained is small. Think of the potential of the ceilometer network (simpler and less characterized instruments) for density of observations, over the sole use of quality-assured and well calibrated instruments but in fewer locations. Both types are clearly needed: the dense network to fill observational gaps and the well characterized network to set a quality assured reference. We believe that in the future smaller and portable lidars will play a greater role, and that papers like ours, addressing their use, will be useful for the advancement of science. What the paper addresses is how to transfer the instrument characterisation from one quality assured system to another which has less quality assurance, i.e. how to bridge the gap between both approaches. This is essential for us so as to be able to characterise the lidar in retrospect, but it has also a great potential for the case of a dense network of simple instruments to be characterised against a well-characterised standard (e.g. a travelling calibration standard).

R1/C14 '*I have trouble to get Eq.(7) from Eq.(6). The jump is too large for me. I recommend to provide more details to the mathematical treatment…., some more words (not equations…) may be sufficient*'

Thank you. To cover the gap between the two equations, we added intermediate equation (7) and renumbered subsequent equations (i.e. equation 7 becomes 8, etc.). See lines 123-126.

R1/C15 '*As already mentioned, the Polly system needs to be better described along the general comments above.*'

By addressing comment R1/C9 we have provided a better description of the Polly system. More information is added in the revised document between lines 204-209.

R1/C16 '*Please keep in mind that there is always some pollution (from all the northern African states at the coast) mixed into the dust plumes reaching Cyprus. Pollution leads to a decrease of the particle depolarization ratio. Furthermore, keep in mind, the contribution of pollution can be rather inhomogeneous so that Nicosia and Limassol observations can be quite different*'

We acknowledge the valid concern regarding the impact of pollution from the northern African states on the measured dust plumes reaching Cyprus (text added in lines: 229-231). We are aware of how the depolarization ratio is modified in such cases (as shown also in the study of e.g. Groß et al. 10.5194/acpd-12-25983-2013).
Regarding the localized pollution differences between Nicosia and Limassol, in this study we have employed filtering criteria to ensure that the measurements have 1) no influence from the

boundary layer (i.e. data above 3 km) and 2) only long-transported plumes for well selected cases. It is unlikely to have different observations for free tropospheric layers having been transported from the same source region for more than 3000 km (as confirmed by backtrajectories of Fig.4) given the short distance (60 km) between the two stations (text added in lines: 238-240).

R1/C17 *'Figure 4 is confusing: What is the arrival height? Different colors for different stations, different colors for different heights, no color scales…. The request of ACP is: (a), (b), (c), (d) ….. if there are 2,3,4 panels… Please improve all figures regarding (a), (b), (c)… if there are more than one panel.*

Thanks for pointing this out. For a better readability, we have added color bars, indicating the altitudes of the different trajectories seen on the plots. In the figure caption we have clarified the arrival heights that were chosen. Also, all figure panels have been identified with letters (a) and (b) as suggested .

R1/C18 *'Figure 5: Please show the same color plots for Limassol in addition: left panels (a), (c), Nicosia observations, right panels (b), (d), Limassol observations! Then we can better see to what extent the different observations can be compared.'*

Thank you for the suggestion. We have now added the PollyXT VDR observations in Figure 5 (b and d subfigures).

R1/C19 '*Figure 6 and 8 (and 10): Polly Rayleigh values of 2% seem to be too high, CIMEL lidar delivers negative depolarization ratios (16 Feb.). Please check the Polly data analysis.*'

Thank you for your comment. The revised PollyXT values seen at the molecular range of Figs. 6,8 and 10 are close to 1% as explained in R1/C9 and thus in agreement with previous publications.
Regarding the CIMEL lidar, it was seen also by the authors that the depolarization ratio values for 2022-02-16 are negative in the lowest ranges. However, please note that the results are compatible with positive depolarization ratios within the error calculation shown in Appendix D and presented as error bars in the revised version of the same figures. This explanation is also added in the document (Lines: 287-291, 297-304 and 331-339). See also R1/C7.

R1/C20 '*Figures 7 and 9: text of (x-axis and y-axis) must be larger.*'

Adjusted. Thanks for pointing this out.

R1/C21 ‘*There is no TROPOS co-author (although they probably need to take care of the Polly lidar all the time), and there is even no word about the TROPOS guys in the acknowledgement.*’

We genuinely acknowledge this oversight on our part. To address this, we have invited Holger Baars from TROPOS, who was actively involved in the PollyXT analysis. We recognize and appreciate the indispensable contributions of the TROPOS team. Their expertise and support have been invaluable throughout the analysis process. Thank you for prompting us on this.

Reviewer 2:

R2/C1 ‘The authors must follow the existing terminology as the one used in fundamental studies, i.e., Freudenthaler (Atmos. Meas. Tech., 9, 4181–4255, https://doi.org/10.5194/amt-9-4181-2016, 2016). For example why K* in equation (3) and (4) already includes the calibration factor?’

Thank you for the suggestion, however please note that our paper doesn't stem from Freudenthaler's 2009 or 2016 approaches, and instead it introduces a simplified model utilizing atmospheric principles, to describe the issues that affect depolarization measurements. It is important to acknowledge also that different terminologies and notations exist among various papers discussing this topic. For instance, in Freudenthaler's work, calibration factor V* in the 2009 publication was later referred to as η* in the subsequent 2016 paper. Similarly, different terminologies referencing the same topic can be observed in studies authored by e.g. Cairo F. 1999, Roy G. et al 2011, Marenco and Hogan 2011, Chazette et al 2012. There is therefore no uniformly accepted notation and even a same author changes the symbols used when publishing different papers. We approach here the problem from a top-down perspective (from what we observe and how we can address it, and without a full knowledge of the internals of the lidar system) as opposed for example to the bottom-up approach of Freudenthaler (based on detailed knowledge of each instrumental component). We also provide two appendices showing how our notation relates to Freudenthaler 2009 (appendix A) and 2016 (appendix B, newly added) bottom-up treatment of the same problem.

Concerning the question expressed "why K* in equation (3) and (4) already includes the calibration factor?”, we note that K* is the ratio of $K_\perp$ and $K_\parallel$ (see lines 111-112 of the manuscript). These equations can be easily derived from (1) and (2). We are not sure that we fully understand what the reviewer call "the calibration factor", but K* is one of the depolarisation parameters that we wish to derive with our method.

R2/C2 ‘Is the PollyXT profile corrected for the G,H parameters?’

Yes, we used the effective G and H values reported to the Center for Aerosol Remote Sensing (CARS) of ACTRIS in the frame of the quality assurance test made in April 2021. Even though we used transmission ratios for the calculation of the volume depolarization ratio in the PollyNET Processing Chain (see comment to R1 above), it can be easily converted to G, H leading the exact the same result.

R2/C3 'Have the authors considered the influence of the interference filter on the molecular calculation?'

Yes, we have taken consideration of the influence of the interference filter type on estimating the theoretical molecular values. Specifically, for the CE376 lidar system under examination, we have accounted for the interference filter's bandwidth, resulting in a calculated molecular value of δ_m = 3.6E-3 (This is explained at lines: 174-175: note that we have slightly reworded this for better clarity). The use of the molecular value according to Behrendt and Nakamura (2002) is also explained in lines 265-268. The use of system-dependent computed values (for Eq. 12) is unavoidable because the Rayleigh Volume Depolarization Ratio (VDR) varies across different lidars (depending on the instrument's receiver bandwidth). Only by employing computed values, we can minimize potential errors that could influence the accuracy of our corrections. See also comment R1/C8.

R2/C4 'Figure 6. There is an offset in the VDR profile from the reference system. If we zoom in, this offset might be high. That makes me wonder how reference is the considered reference system. And how well calibrated is the depolarization ratio.'

Thanks for pointing this out. We believe that the reviewer is referring to the observed offset in layers with low VDR (PBL and free troposphere). Note that we anticipate differences in the Rayleigh VDR between the two lidars due to the different interference filter bandwidths (molecular VDR is affected by the instrument specification, see previous comment). In the revised manuscript we discuss this difference in lines 265-268. Our correction approach for the VDR profile incorporates two essential inputs: 1) PollyXT measurements obtained at high depolarizing layers (δ_{ref}) and 2) the computed molecular VDR (δ_m) based on (Behrendt and Nakamura, 2002). Therefore, the offset that the reviewer has noted does not affect our characterisation.
However, after double-checking following the reviewer comment, we realized that in the previous version of the data, the measured transmission ratios for the total and cross channel that had been used for the calculation of the volume depolarization ratio were not the most optimal. With these measured values, effects in the detection unit could be accounted for, but impurities in the polarization state of the emitted light and/or polarization effects at the telescope mirror could not be accounted for. Thus, we revised the whole PollyXT dataset by applying more optimal effective transmission ratios based on long term observation of the

volume depolarization ratio in particle-free regions. The revised values are the ones that have been submitted together with the ACTRIS quality assurance test (made in April 2021) to the ACTRIS Center of Aerosol Remote Sensing (CARS) in the frame of the CAMS2_21b project which aimed for the delivery of lidar data to the Copernicus Atmosphere Monitoring Service (CAMS) in near real time for data assimilation.

We now use these effective transmission ratios for the calculation of the volume depolarization ratio within the PollyNET Processing Chain. As a result of the revision of the PollyXT dataset, the profiles of volume depolarization ratio have changed only slightly, but indeed, the volume depolarization ratio in particle-free regions is now close to 1% as the reviewer has observed in other publications (comment R1/C8).

Thus, we thank the reviewer very much for pointing us to this issue so as to optimize our datasets to the best quality. We note that the message of this publication and and the main outcomes (VDR of the CIMEL lidar after characterization) have not changed substantially, but only slightly.

R2/C5 ‘Figure 7. The parameters change in time, and that is not a noise effect. A possible reason might be that the calibration factor changes in time and this factor is included in the K parameter. An idea would be to apply the worst values from Figure 7, to calculate and see the difference in the volume depolarization. And add all the calculated profiles in Figure 8 as error bars.’

Thank you for the suggestion. Following the reviewer’s suggestion, to better illustrate the influence of the parameter variability, we have incorporated error bars (calculated based on Appendix D) around the calculated profile in Figure 8 to depict the range of possible VDR values resulting from the parameter variations. In addition we added error bars in Figures 7 and 9 representing the variability of the derived parameters within the comparison ranges h_{δ_m} and h_{δ_d} . We believe that the error bars given in Fig. 7,8 and 9 provide the required information. In the revised document we explain this in lines 295 and 302-304.

R2/C6 ‘The authors claim that in the Three-parameter depolarization characterization «….we can use a second dust layer or/and a high level ice cloud»..The idea of using a high level cloud is risky, as the ice crystal orientation is different for the different systems, the multiple scattering effect is not negligible, the distance between the two lidars is big, different part of the cloud is seen by each system. Authors should rephrase. Are both lidar pointed at the same angle?’

Thanks for pointing this out and we have added a caveat in the paper. We understand the risks associated with utilizing high-level ice clouds due to differences in ice crystal orientation between systems and since clouds have more spatial and temporal variability than long-range transported dust. In fact, in the paper, we show that the two-parameter characterization is more stable to use and that we prefer it, which aligns with the reviewer’s comment. If the 3-parameter calibration is to be used, we recommend prioritizing the use of a second dust layer

whenever feasible, as in the 3-parameter case shown in our paper. We rephrase the above-mentioned sentence in lines 312-316.
Regarding the lidars' angle, CIMEL CE376 points mechanically toward the vertical, and the beams are propagating vertically with a precision of 1-2 microrad. The PollyXT lidar points 5° off zenith to avoid specular reflections. This information is added in the revised paper at lines 161-162 and 199-200.

R2/C7 'Figure 10. The authors claim that "The three-parameter approach generally results in smaller VDR values for the whole profile range."…the profile is below zero. The mean value seems not to be applicable to all cases. Authors must provide a detailed explanation. Maybe the timeseries of the depolarization parameters should be expanded.'

Thank you for your comment. We have noted that in Figure 10 the three-parameter approach results in negative values, in the non-depolarising ranges. We believe that this occurs due to a large estimate of the g parameter. Upon assessing uncertainties on the g and e parameters for both methods (Appendix D) we find that the negative values are compatible with a VDR of 0 (and even of δ_m) within the uncertainty bounds. We have now added the shaded error bar region in Fig.10, representing the uncertainty associated with the three-parameter method. The error bar highlights that the discrepancies observed between the two methods, including the occurrence of negative values, falls within the expected uncertainty bounds of this approach. In the revised document we added this explanation in lines 331-339.
In addition, after using the new PollyXT profiles (see our answer to comment R2/C4) we revised the timeseries of Fig.7 and Fig.9 and included error bars on the depolarization parameters to show how the variability changes between the two and three parameter approach.

R2/C8 'Figure 10 is placed prior to Figure 9.'

Adjusted. Thanks for pointing this out.

We addressed the concerns raised by the reviewers in the manuscript and in our point-by-point response. We think that that the paper has been greatly improved, and we thank the reviewers for this. We submit hereby a revised version.

Thank you once again for your thoughtful review and consideration. We eagerly await your response.

Warm regards,

Alkistis Papetta and the co-authors