

Dear respectful Referee,

Thank you very much for your valuable comments. You raised helpful and precise questions about several parts of the work, especially the important issue of methodology. Our response to each of your comments appeared in a two-row table. The first row includes your comment or question and in the second row we brought our response. We also addressed the changes in the text by the line numbers (L??? - ???) of the current version of the manuscript, at the end of each response. It should be mentioned that some changes are also appeared in the new version of the manuscript based on the comments by the other referee.

We also reviewed once again the whole manuscript for typos and grammatical errors. There are also two small changes in the manuscript that we have to mention them here.

1. The azimuthal angle,  $\varphi$ , direction in Fig. 1 was not correct in the previous version. So, we swapped  $+50^\circ$  and  $-50^\circ$  in Fig. 1 in the current version.
2. The beam expander that we used in this work was a 10x and by a mistake from our previous works it was mentioned as a 5x in the previous version of the manuscript. We made this correction in L 104.

The revised version of the manuscript has now been uploaded. We believe the manuscript is in good shape now, and hope that it would be acceptable for publication in AMT. On behalf of my coauthors and myself, I am very much grateful for your precise review of our manuscript and we are quite open to consider if there will be any further comments or questions.

With kindest regards  
Salar Alizadeh  
Corresponding author

#### Point-by-point response to the comments of Referee #1

Comment #1	<p>Sect. 2.3: Your methodology seems to use a 'mixture' of both, the one-step and the two-step method. The one-step method uses particle depolarization ratios of 0.31 (dust) and 0.05 (spherical, non-dust particles) to characterize the aerosol mixture under investigation, whereas the two-step method uses 0.05 (non dust), 0.15 (fine dust) and 0.35 or 0.39 (for coarse dust) according to Mamouri and Ansmann (2014).</p> <p>Your methodology now uses a dust depol ratio of 0.31 (and not 0.35 or 0.39), a salt dust depol ratio of 0.15. and a wet dust depol ratio of 0.05 in the first step. There is no fine-mode dust contribution (and fine dust depol ratio of 0.15) considered in the first round. Do you think that you removed the ENTIRE dust impact after the first step, when using the dust depol ratio of 0.31 instead of 0.35 or 0.39, so that the remaining backscatter and depolarization information (in the second round) is due to salt dust and wet salt? Please provide a comment on this problem, namely that both, salt dust and fine-mode desert dust produce similar depolarization ratios around 0.15.</p>
Response #1	<p>There are some points that we should point out concerning this question</p> <ol style="list-style-type: none"><li>1- Since in the two-step POLIPHON the atmospheric particles categorized only in three groups, so instead of categorization of particles into coarse dust, fine dust, and non-dust particles as Mamouri and Ansmann (2014), we categorized the atmospheric particles into pure dust, salt-dust, and wet-salt. I have to add we are measuring just above the Urmia Lake surface that is covered with large amounts of salt particle and salt flakes (L 148 – 152). The measurement site also is ~40 km far from the Urmia city (Fig. 1). So, we considered that in September the temperature was ~20 °C during the measurements (L 201), concentration of urban pollution is not so high over the lake.</li></ol>

2- Based on our previous work (Ghomashi and Khalesifard, 2020) and works by other teams (e.g., Nisantzi et al., 2015, Flutsi et al., 2023), linear depolarization of dust particles very rarely goes beyond 0.30. So, in the first step of the POLIPHON we considered all particles having  $\delta_p > 0.31$  are pure dust (not coarse dust). We believe this is a reasonable assumption and stricter on removing the pure dust particle from the calculations compare to putting  $\delta_p > 0.35$ . But it should be mentioned that particles that we called them as salt-dust (L 49) due to mixing with salt and absorbing moisture may have  $0.05 < \delta_p < 0.31$ . Fig. R1 depicts some typical changes on the retrieved  $\beta_i$  and  $MC_i$ , for  $\delta_d = 0.31, 0.35,$  and  $0.39$  (from the recording on Sep. 20, 2022, 07:43 – 08:28 LT). As it shows the corresponding values for sd- and ws-particles don't show considerable changes. To have an overall picture, taking  $\delta_d = 0.39$  instead of  $\delta_d = 0.31$  as the boarder in the first step of the POLIPHON, decreases the total  $MC_d$  for  $\sim 23.8\%$  and increases  $MC_{sd}$  and  $MC_{ws}$ , for  $\sim 3.7\%$  and  $\sim 2.4\%$ , respectively. Where the  $MC_i$  are integrated over the whole detected plume in that recording.

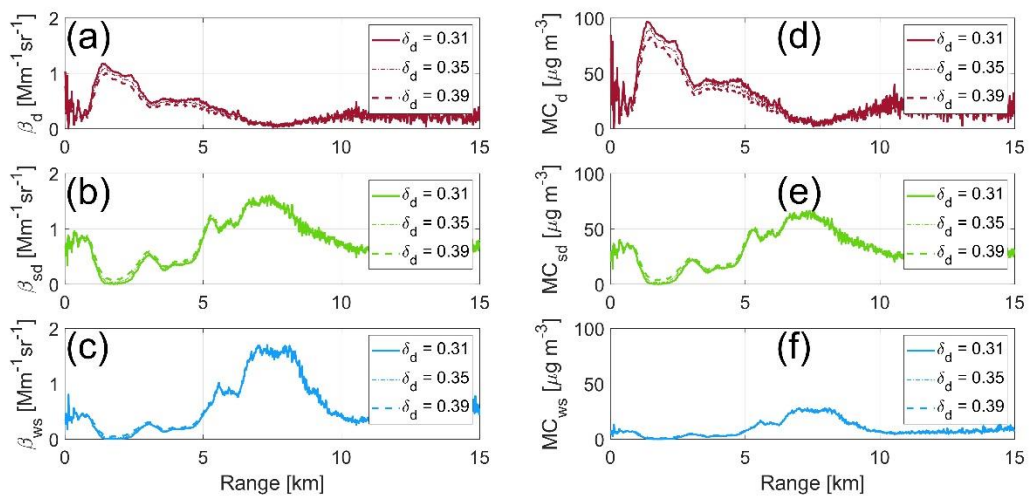


Figure R1. Changes on  $\beta_i$  and  $MC_i$  for  $\delta_d = 0.31, 0.35,$  and  $0.39$ .

3- We have no way to distinguish between fine dust and salt-dust in POLIPHON algorithm but considering the location of the lidar station, azimuthal directions of the lidar scans, and making measurement just over the aerosol plumes that most probably raised from the lake-bed, we considered these three types of the particles.

Comment #2

The discussion of the results is ok. The question is still to what extent the results are influenced by the fact that both, the depol ratios of fine dust and the depol ratio of salt dust are around 0.15 and how this fact influences the presented results.

Response #2	<p>There are some points that should be mentioned here.</p> <ol style="list-style-type: none"> <li>1- The two-step POLIPHON categorize the particles in just three groups, and so instead of “<i>coarse dust, fine dust, and non-dust</i>” we modified it to work on “<i>dust, salt-dust, and wet-salt</i>”.</li> <li>2- The salt-dust depolarization range (i.e., 0.05 – 0.31, see Sect. 2.3) overlaps with that of fine dust and polluted dust. But since; <ol style="list-style-type: none"> <li>a. The lidar scans horizontally just few tens of meters above the dried lake bed,</li> <li>b. The lake bed is covered with salt particles and salt flakes,</li> <li>c. The lidar station is ~40 km far from Urmia the largest city close to the station,</li> <li>d. The campaign was in September when the average surface temperature was ~20 °C,</li> <li>e. Based on reported horizontal visibilities, and HYSPLIT backward trajectories, there was no transregional dust outbreaks for the selected campaign days.</li> </ol> <p>Then we considered that</p> <ol style="list-style-type: none"> <li>a. The detected plumes raised from the lake bed and particles inside them mostly should be mixed with salt except those having <math>\delta_p &gt; 0.31</math>, which are taken as pure dust,</li> <li>b. Due to the distance from Urmia, low elevation of the plumes, and high surface temperature, we don't expect that particles inside the plume got contaminated with urban pollution.</li> </ol> <p>These are the reasons that we considered the mentioned categorization. We added L 210 – 221 to the discussion section, to make this assumption clear. Hope that this would be acceptable.</p> </li> </ol>
-------------	--

Comment #3	Besides backscatter coefficients one should also provide the related extinction coefficients in Table 2, by using the lidar ratios in Table 1.
Response #3	It is done in the 2 <sup>nd</sup> row of Table 2 and L 273 – 276 is added.

Comment #4	Is it possible to provide even a hypothetical conclusion regarding the unhealthy pollution? In the upcoming years and decades, all the particles of the partly toxic composition of the lake sediment will be pushed into the air with strong winds.
Response #4	To get an idea about the influence of raised particle plumes from the dried bed of the Urmia Lake on its surrounding area, other measurement campaigns should be run on other locations in its catchment area. Especially, in the regions between the lake and the city of Tabriz that is the largest city in Northwest Iran hosting about 1.7 million inhabitants and a considerable number of industries are active in its suburbs. In addition to lidar recordings, aerosol sampling and chemical analysis of the collected samples should be done to have an idea about the toxicity of the aerosols. Such

campaigns should be carried on at the east side of the lake but still our team didn't have proper support to run such a project. Hope that this will happen in future.

We added some sentences to the conclusion on lines:

L 325 – 328.

Dear respectful Referee,

Thank you very much for your valuable comments. You raised helpful and precise questions about several parts of the work, especially the important issue of methodology. Our response to each of your comments appeared in a two-row table. The first row includes your comment or question and in the second row we brought our response. We also addressed the changes in the text by the line numbers (L??? - ???) of the current version of the manuscript, at the end of each response. It should be mentioned that some changes are also appeared in the new version of the manuscript based on the comments by the other referee.

We also reviewed once again the whole manuscript for typos and grammatical errors. There are also two small changes in the manuscript that we have to mention them here.

3. The azimuthal angle,  $\varphi$ , direction in Fig. 1 was not correct in the previous version. So, we swapped  $+50^\circ$  and  $-50^\circ$  in Fig. 1 in the current version.
4. The beam expander that we used in this work was a 10x and by a mistake from our previous works it was mentioned as a 5x in the previous version of the manuscript. We made this correction in L 104.

The revised version of the manuscript has now been uploaded. We believe the manuscript is in good shape now, and hope that it would be acceptable for publication in AMT. On behalf of my coauthors and myself, I am very much grateful for your precise review of our manuscript and we are quite open to consider if there will be any further comments or questions.

With kindest regards  
Salar Alizadeh  
Corresponding author

#### Point-by-point response to the comments of Referee #2

Comment	Lines 31 – 32 “Gholampour et al. investigated”: I think the authors should include the year of publication of the cited work to facilitate easier accessibility for the readers. Kindly cross check throughout the manuscript and update all citations for which the year of publication is missing.
Response	Thank you for your suggestion. It is done, we updated all same citations too.

Comment	Lines 38 – 39 “Moderate Resolution Imaging Spectroradiometer (MODIS)”: a suggestion would be to mention that MODIS is an instrument onboard satellite missions.
Response	It is done. We mentioned as: “Moderate Resolution Imaging Spectroradiometer (MODIS, onboard Terra and Aqua satellites)”, L 38 – 39.

Comment	Line 43 "(CALIOP)": similar to previous comment, kindly consider mentioning the satellite mission that was carrying the instrument.
Response	Same as previous comment, it is done.  "the Cloud-Aerosol Lidar with Orthogonal Polarization (CALIOP), onboard Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations (CALIPSO) satellite.",  L 43 – 44.

Comment	Lines 50 – 51 "CALIOP algorithms often misclassify ... Khalesifard, 2020)": I would suggest mentioning, if possible, to which aerosol types the salt particles are usually misclassified over the lake region.
Response	In one of our previous works, we reported that when the observed aerosol plumes were in contact with the lake bed usually, particles are characterized as polluted dust in the CALIOP categorization of the aerosol subtypes (Ghomashi and Khalesifard, 2020).  So, we rewrote the sentence on L 50 – 51 as:  "CALIOP algorithms often misclassify salt particles as polluted dust subtype, and near-surface aerosol layers are not well resolved (Ghomashi and Khalesifard, 2020)".

Comment	Line 53 "particle depolarization ratio ( $\delta_p$ )": I guess the authors refer to the use of particle linear depolarization ratio, please clarify. Also maybe define here that $\delta_p$ and $S$ refer to values for 532 nm in all cited papers and the presented analysis, to avoid confusion with $\delta_p$ and $S$ values at other lidar wavelengths (e.g. 355nm).
Response	Yes, we mean the particle linear depolarization ratio. Throughout the manuscript, "particle depolarization ratio" has been replaced with "particle linear depolarization ratio".  We also added L 71 – 73 "Therefore, all reported parameters in this analysis and cited papers, including the linear depolarization ratios ( $\delta$ ), lidar ratios ( $S$ ), and backscattering coefficients ( $\beta$ ), refer to values for 532 nm; otherwise, the corresponding wavelength is indicated."

Comment	Line 54 "(Freudenthaler et al., 2009; Tesche et al., 2009)": I would suggest adding more relevant studies to strengthen the cited literature.
Response	Thank you for your suggestion. We have added more relevant studies:  "(Freudenthaler et al., 2009; Tesche et al., 2009; Burton et al., 2012; Hofer et al., 2020; Floutsi et al., 2023)",  L 54 – 55.

Comment	Lines 58 – 59 “For pure dust particles, ... Nisantzi et al., 2015).”: I guess the authors focus on the literature values for desert dust from Middle East dust sources, please clarify and discuss that $\delta_p$ and S values can slightly differ for dust particles from different dust sources (e.g. Sahara, Middle East, Asia).
Response	<p>Thank you for the suggestion. We considered it and added following sentences:</p> <p>“There are slight differences in the reported values of particle linear depolarization and lidar ratios for dust particles, depending on their origins. Floutsi et al. (2023), in a valuable work, collected different lidar-derived optical properties of atmospheric aerosols originating from different sources. They found a considerable similarity between the Central Asian and Middle Eastern dust, where <math>\delta_p</math> and the lidar ratio S at 532 nm vary in the range of 26.8 – 33.2 % and 35 – 43 sr, respectively. For the Saharan dust, the former is in the range of 26.7 – 29.3 % and the latter in 45 – 61 sr.”,</p> <p>L 59 – 64.</p>

Comment	Line 63 “emitted by salt dust”: do the authors mean salt dust mixtures? Please clarify.
Response	<p>Yes, exactly we were talking about salt dust mixture, so we rephrased these sentences as:</p> <p>“For 15 % of the 276 cases, they detected layers that most probably contained dry salt, or particles formed of dust and salt mixtures. They mentioned that such particles may be raised from the dried lakes or saline playas in the region. For lidar measurements at 532 nm, they reported very low extinction coefficients (<math>&lt; 30 \text{ Mm}^{-1}</math>), <math>S &lt; 25 \text{ sr}</math>, and <math>\delta_p \sim 0.15\text{--}0.20</math>, for such layers.”</p> <p>Please refer to L 66 – 69.</p>

Comment	Line 66 “at the lake’s southwestern”: please clarify that you mean Urmia lake.
Response	<p>This is applied to L 70 and the sentence following that is added in response to your other comment.</p> <p>“We installed a scanning polarization lidar operating just at 532 nm at the southwestern coast of Urmia Lake from September 2018 to October 2022.”</p> <p>L 70 – 71.</p>

Comment	Lines 69 – 70 “Some dust plumes ... those in Mesopotamia.”: please provide altitude ranges for the observed dust plumes and indicative values of measured $\delta_p$ and S at 532nm.
---------	--

Response	<p>It is done. More detail is added and the sentences are rewritten as following:</p> <p>“From July to October 2020 and 2021, some dust plumes with <math>\delta_p = 0.23 \pm 0.05</math> were observed that originated from the Middle Eastern sources. 20 of them were from Mesopotamia, which were observed at the altitude ranges of <math>2.50 \pm 0.85</math> km AGL, and a single one that rose up from the Arabian desert was detected at <math>0.90 \pm 0.20</math> km AGL (Alizadeh et al., 2022). In October 2021, one notable case involved a lofted dust plume (<math>\delta_p = 0.23 \pm 0.03</math>) from Niger, Africa, detected in Urmia Lake atmosphere at <math>\sim 8</math> km AGL after about 140 hours of transport (Alizadeh et al., 2024b).”</p> <p>Please refer to L 75 – 80.</p>
----------	---

Comment	<p>Lines 70 – 72 “One notable case ... Alizadeh et al., 2024b).”: same as previous comment, please provide indicative values of measured <math>\delta_p</math> and S.</p>
Response	<p>Details are added.</p> <p>“In October 2021, one notable case involved a lofted dust plume (<math>\delta_p = 0.23 \pm 0.03</math>) from Niger, Africa, detected in Urmia Lake atmosphere at <math>\sim 8</math> km AGL after about 140 hours of transport (Alizadeh et al., 2024b).”</p> <p>Please refer to L 78 – 80.</p>

Comment	<p>Line 72 “During winter, ...”: it is not clear to me if the discussed findings in the previous lines (66 - 69) refer to summer or spring season and then from line 72 and on the aerosol climatology during the winter season is being discussed. Please elaborate and clarify.</p>
Response	<p>L 80 – 82, Instead of seasons we added the months names and rephrased the sentence.</p> <p>“From November 2021 to February 2022, the type of atmospheric particles undergoes marked changes. Lidar measurements indicated that the lower atmosphere near the lake surface is dominated by anthropogenic aerosols, with lower <math>\delta_p</math> values of <math>&lt; 0.10</math>.”</p>

Comment	<p>Line 79 “Tesche et al. (2009)”: if not mistaken, the derivation of particle depolarization ratio was firstly introduced in Biele et al 2000 (<a href="https://doi.org/10.1364/OE.7.000427">https://doi.org/10.1364/OE.7.000427</a>) and the reference therein. Kindly consider acknowledging these publications throughout the manuscript.</p>
Response	<p>Thank you for your comment, we rephrased the sentences as:</p> <p>“For each plume, we retrieved the particle backscatter coefficient (<math>\beta_p</math>) using the well-known technique introduced by Klett (1981, 1985); Fernald (1984), and following the approach introduced by Biele et al. (2000) and used by many other teams, e.g., Freudenthaler et al. (2009); Tesche et al. (2009), we obtained the <math>\delta_p</math> values for different lidar recordings.”</p> <p>L 87 – 90.</p>

Comment	Line 89 “polarization lidar”: is it a linear polarization lidar? Please clarify.
Response	Yes, that’s right. To clarify, we’ll put it this way:  “The IASBS scanning polarization lidar (ISPL) is a single-wavelength (532 nm) linear polarization lidar that is designed and constructed at the Institute for Advanced Studies in Basic Sciences (IASBS) remote sensing laboratory.”  L 100 – 101.

Comment	Line 94 “On the receiver side, ...”: what is the full overlap range (distance from the lidar) of the system?
Response	We added it,  “The full overlap range of the ISPL is at a distance of ~ 3.0 km from the lidar.”  L 105 – 106.

Comment	Line 95 “8-inch Cassegrain telescope”: please clarify to which parameter the 8 inches refer to (is it the diameter of the primary mirror?).
Response	We’ve modified the sentence as follows:  “On the receiver side, a Cassegrain telescope with an 8-inch primary mirror and focal length of 1950 mm collects the backscattered light.”,  L 106 – 107.

Comment	Line 103 “particle classification (Freudenthaler et al., 2009; Tesche et al., 2009)”: same as comment in Line 54, kindly consider citing more studies and maybe also include more recent ones (e.g. Floutsi et al., AMT, 2023).
Response	It is done.  “Using polarization lidar recordings, key aerosol parameters can be derived, including the backscattering coefficient and the linear depolarization ratio. These parameters are essential for particle classification (Freudenthaler et al., 2009; Tesche et al., 2009; Burton et al., 2012; Groß et al., 2013; Kim et al., 2018; Hofer et al., 2020; Wandinger et al., 2023; Floutsi et al., 2023, 2024).”  L 115 – 117.

Comment	<p>Lines 116 – 120: Based on a more recent paper from Freudenthaler, AMT, 2016 the measured volume linear depolarization ratio may be affected from polarizing effects introduced by optical elements in the emission and receiver units. In ISPL lidar, such optics could be the beam expander and less likely the IFF. Do the authors follow the suggested methodology (i.e. the GHK parameters) in Freudenthaler, AMT, 2016 to retrieve the “corrected” volume depolarization ratio? I would suggest the addition of a related discussion in the text.</p> <p>Moreover, the molecular depolarization ratio value that is used in the derivation of the particle depolarization ratio is system dependent (see e.g. Behrendt and Nakamura, 2002, <a href="https://doi.org/10.1364/OE.10.000805">https://doi.org/10.1364/OE.10.000805</a>). However, in this study, the authors adopt a theoretical value (<math>\delta_m = 0.0036</math>) from literature without providing justification for its applicability to their specific system. This assumption may introduce biases and lead to inaccuracies in the retrieved particle depolarization ratio. Could the authors support why do they follow this approach? I would strongly recommend them to use in their analysis an appropriate molecular depolarization value according to the system specs.</p>
Response	<p>Thank you very much for these valuable comments.</p> <p>Concerning the ISPL calibration, we used the technique suggested by Freudenthaler et al. (2009). To investigate the effects of the beam expander on the polarization states of the linearly polarized laser beam, we used the framework that he presented in his 2016 work but simulated the situation for our 10x beam expander in Zemax and found that the total impact of the beam expander on the depolarization of the laser beam is <math>\sim 2.3 \times 10^{-5} \%</math> that is too small to affect the retrieved volume linear depolarization ratios for the horizontal scans that we are dealing with them.</p> <p>These discussions are appeared on L 136 – 144.</p> <p>Fig. R2 is taken from the work by Behrendt and Nakamura (2002). It can be found from this figure that for the 532 nm interference filter that we used and has a <i>FWHM</i> band width of 1 nm, the molecular depolarization ratio is about 0.0042. This is similar to the theoretical value of 0.0036 up to the third digit after the decimal point. Even when we interchanged these two values, <math>\delta_p</math> maximally changed for <math>\sim 0.02 \%</math> for the horizontal scans that we are concerning with them.</p> <p>These discussions are appeared in L 132 – 135.</p> <div data-bbox="548 1171 1133 1585" data-label="Figure"> <p>Figure R2. Courtesy of Behrendt and Nakamura (2002).</p> </div>
Comment	<p>Lines 182 – 183 “each five recorded lidar signals were averaged”: what time interval does this averaging represent? Could the authors clarify this in the text?</p>

Response	<p>The full scan duration is 45 minutes, which includes 1000 signals. Averaging five signals is equivalent to 13.5 seconds. We rephrased these sentences as:</p> <p>“In addition, each five recorded lidar signals (13.5 seconds of recordings) were averaged to reduce the background noise.”,</p> <p>L 209 – 210.</p>
----------	---

Comment	<p>Figure 6 “twenty recorded signals about the red dashed line”: kindly consider rephrasing this part to make it more clear.</p>
Response	<p>Thank you for the suggestion, now the signal averaged region is specified by the area between two red dashed lines on Fig. 6a. The whole caption is changed as following:</p> <p>“Figure 6. (a) RA series of the ISPL RCSs on September 20, 2022, 07:43–08:28 LT; 20 signals between the two red dashed lines were averaged to retrieve the profiles of (b) the volume linear depolarization (<math>\delta_v</math>) and the particle linear depolarization (<math>\delta_p</math>) ratios, (c) backscatter coefficient of dust (<math>\beta_d</math>), salt-dust (<math>\beta_{sd}</math>), wet-salt (<math>\beta_{ws}</math>) particles, and their sum (<math>\beta_p</math>), (d) mass concentration (<math>MC_i</math>) for the same particle types as in c.”</p>

Comment	<p>Figure 6 (a): I find the use of a Range–Range coordinate system not optimal, as it makes it difficult to track the signal across ranges for each scan. One suggestion would be to use radial gridlines with appropriate tick labels for a clearer and more intuitive representation. Same applies for Fig. 7 too.</p>
Response	<p>Thank you for your suggestion,</p> <p>The RR series in Figs. 6 and 7 replaced with Range-Angle (RA) series.</p>

Comment	<p>Figure 6 (b-d): i) What are the uncertainties for each retrieved/calculated profile? Please provide error bars for all plotted profiles.</p> <p>1. ii) The plotted lines hide the axes ticks, making it difficult for the reader to track the values of the plotted parameters. If possible, please update the subplots accordingly.</p> <p>iii) Looking at the RCS around the dashed red line in subplot (a), the RCS is very close to zero (if not zero) after the plume (approx. above 10 km?), so I would expect the <math>\beta_p</math> to be around zero which is not the case (<math>\sim 2 \text{ Mm}^{-1}\text{sr}^{-1}</math>). The overestimated <math>\beta_p</math> could result to an overestimation of <math>\delta_p</math> and, thus, affect the classification and the mass concentration calculations. Could the authors comment on that?</p>
---------	--

Response	<p>i) Done, we added parameters' uncertainties. But they are not put everywhere just to keep the clarity of the graphs.</p> <p>ii) This was also done. Now the ticks are in front of the graphs.</p> <p>iii) Fig. R3 shows that the RCS is not zero after 10 km. This is just because of the similarity of the blue color tones that it seems that after 10 km the RCS is close to zero. Also, it should be mentioned that since we are measuring horizontally, always there are background particles and plumes got formed over such background. Since we have the RCS RA-series in Fig. 6a, we prefer not to add Fig R3 to the manuscript and just brought it here to make our answer clear. We also changed the range of the color bar in Fig. 6a to prevent this confusion.</p>
	<p>Figure R3. RCS and <math>\beta_p</math> averaged over the region between to red dashed lined in Fig. 6a.</p>

Comment	Figure 8: Kindly consider adding error bars with the standard deviation of the plume-averaged values to indicate the variability of the values for each detected plume.
Response	This is done.

Comment	Lines 240 – 257: Regarding the categorization of the aerosol plumes, maybe I am missing something, but why do the authors rely on the plume-averaged $\beta_{sd+ws}$ in their categorization scheme in Table 3 instead of using the total plume-averaged backscatter coeff. ( $\beta = \beta_d + \beta_{sd} + \beta_{ws}$ )? For example, I would expect that the mixed mode would contain cases where the contribution of salt-dust particles is significant compared to wet salt and pure dust particles.
Response	We focused on $\beta_{sd+ws}$ to investigate fractions of particles that may contains salt in any form (eider dry or wet) respect to the rest of the plume, i.e., dust particles. Of course, a plume may contain all three types of particles. Even if we want to use the total particle backscatter coefficient, $\beta$ , to find the contribution of salt-contained particles in the plume, again we have to take in account $\beta_{sd+ws}$ .

Comment	Lines 263 – 264 “presence of both ... the measurement period”: Have the authors used MODIS observations and HYSPLIT trajectories as auxiliary dataset to verify no trans-regional aerosol transport for all cases included in the analysis? Please clarify.
---------	---

Yes, to get sure that the recorded plumes are just raised from the lake bed, we looked at MODIS AOD's over the region, HYSPLIT backward trajectories for each detected plume, hourly horizontal visibility (HV) data reported by the Urmia Met-office, AOD and Angstrom Exponent (AE) recorded by a hand-held sunphotometer. Fig. R4 shows the state of the atmosphere over the Urmia Lake for the 64 cases which aerosol plumes are detected by the ISPL. Fig. R4a shows the recording times of the 64 mentioned cases, Figs. R4b-d are the HV, the AOD, and the AE, respectively. It is clear from these figures there was not any dust event over the region. In addition, Fig. R5 shows the 48 hrs backward trajectories started from the ISPL station at 100 m AGL. Red trajectories are passing over the lake and green ones not (Fig. R5a). Fig. R5b shows how the red trajectories are at lower altitudes and mostly touching the ground so soon, but the green trajectories are at higher altitudes and rarely reach to the surface.

This is how we concluded that we are just measuring on the plumes that raised from the lake bed. But we can't put all of these into the manuscript.

Please refer to L 202 – 206.

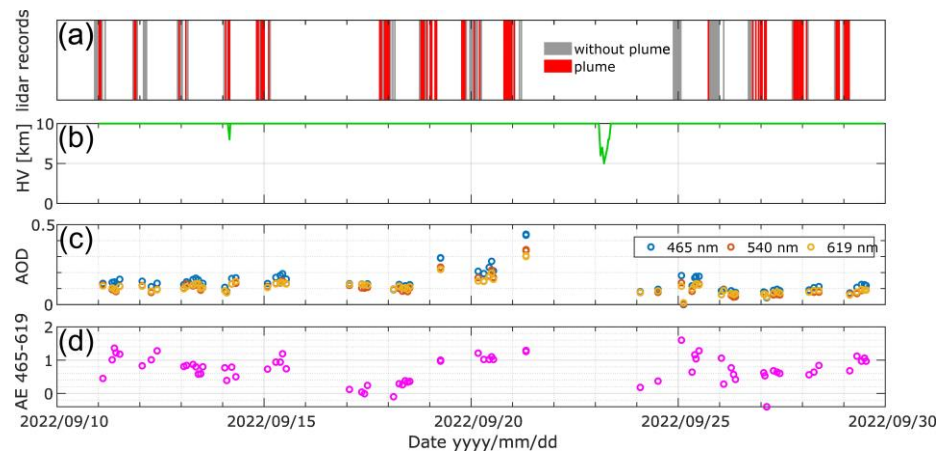


Figure R4. State of the atmosphere over the Urmia Lake during the mentioned 64 cases which aerosol plumes are detected by ISPL, (a) the recording times of the 64 cases, (b) the HV reported by the Urmia Met-office, (c) the AOD at three wavelengths, (d) the AE.

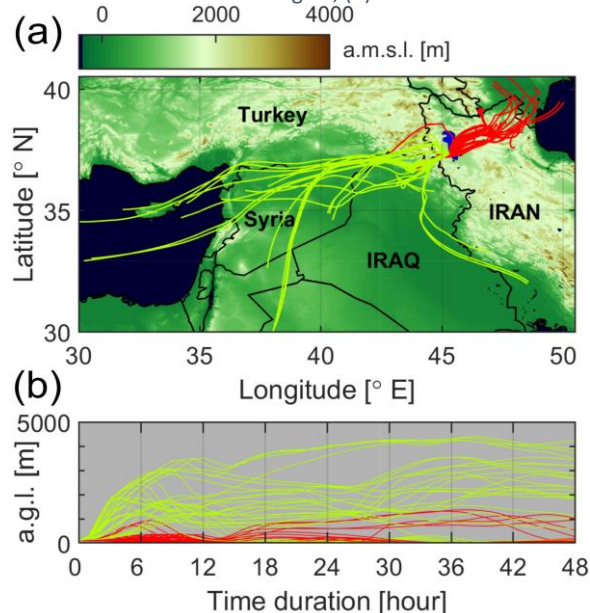


Figure R5. 48 hrs HYSPLIT backward trajectories ended at the ISPL station at 100 m AGL, for the 64 cases, red trajectories are passing over the lake and green ones not.

Comment	Line 283 “showed how salt particles influence the plume albedo.”: Could the authors clarify how their analysis supports this claim?
Response	<p>Actually, we concluded this from Figs. 8b and 8c, where they show small amounts of salt particles considerably increase their corresponding backscatter coefficient. To make it clearer we added the following sentences to the “Overall View” subsection;</p> <p>“Figs. 8b and 8c also show how the salt content of aerosols influences the backscatter coefficient and eventually the albedo of the plume. For example, in case 17, <math>MC_{ws} \approx 40 \mu\text{g m}^{-3}</math> and <math>\beta_{ws} \approx 2.5 \text{ Mm}^{-1}\text{sr}^{-1}</math> but for case 39, when <math>MC_d \approx 82 \mu\text{g m}^{-3}</math> the <math>\beta_d</math> is only <math>\sim 1.0 \text{ Mm}^{-1}\text{sr}^{-1}</math>. This is in complete agreement with the lower lidar ratios obtained for salt particles (<math>\sim 25 \text{ sr}</math>) compared to dust particles (Table 1).”</p> <p>L 269 – 272.</p>

#### Technical corrections

Comment	Lines 101 “particulate backscatter coefficient and depolarization ratio”: suggested change to “particle backscatter coefficient and linear depolarization ratio”?
Response	<p>Done.</p> <p>L 113.</p>

Comment	Lines 104 – 105 “particle backscatter ratio”: do the authors mean particle backscatter coefficient?
Response	<p>Yes, we changed it to:</p> <p>“particle backscatter coefficient”,</p> <p>L 118.</p>

Comment	Line 114 “particulate depolarization ratio”: the $\delta_p$ sometimes appears as particulate depolarization ratio or as particle depolarization ratio (e.g. line 53). Kindly ensure a consistent nomenclature is used throughout the text.
Response	Thank you very much for your precise comment, we put everywhere “particle depolarization ratio”.

Comment	Line 190 "RSC": RCS
Response	It is done. L 227.

Comment	Line 227 "δps":is the "s" a typo?
Response	Yes, we changed it to: " $\delta_p$ values", L 264.