Response to Referee #3

First of all, we would like to thank the Referee for careful reading the manuscript and for useful suggestions.

Answering the Referee comments:

Fig.8a and Page 9, Lines 283-285: "Thus, we can assume that increase of the imaginary part in UV in the first layer is more significant, than in the second one". I noticed from this figure that the Angstrom exponent (both backscatter and extinction related) increases towards higher altitudes, which coincides with a slight decrease of depolarization ratio and a coincidence of the S355 and S532. Could these variations point towards the dominance of smaller dust particles higher in the layer? From laboratory studies we know that smaller dust particles present lower depolarization ratio values (i.e. Järvinen et al. 2016; Sakai et al., 2010), while also the larger S532 values lower in the layer could be attributed to the increased "sensitivity" this wavelength should have to the presence of larger particles. Why is the dominance of smaller dust particles should be excluded here?

Slight increase of the extinction Angstrome exponent (EAE) with height can definitely indicate that particles become smaller, which agrees with decrease of depolarization ratio. But in previous dust episodes for the same EAE and depolarization ratio, we had S355>S532, so we think that change of particle size only, without decrease of the imaginary part in UV can not explain different values of S355/S532 in upper and lower layers. Besides, the AERONET data demonstrate that column imaginary part in UV starts to decrease this day.

Page 8, Line 245: "assuming 35% and 7% for dust and smoke depolarization ratio". Please provide some references on choosing these values specifically. Did you use the same values also for the dust-smoke decomposition you perform for all the cases presented?

35% is the highest depolarization ratio we observed for pure dust. The depolarization ratio of different types of smoke can vary significantly. 7% is the lowest value we observed in elevated smoke layers during SHADOW. We should mention, that depolarization of smoke is much lower than that of dust, thus the choice of exact value of smoke depolarization does not influence significantly the results. Corresponding comment and reference is added to manuscript.

Also Page 14, Line 429: "In principle, we can estimate S532s using Eq.5, because the ratio β532s)/β532 is available". Here the 532s depends on the selected value of δ532s. Can you provide an estimation of the uncertainties of this approach? What could be the effect on the resulting S532s values?

Yes, choice of smoke depolarization ratio introduces uncertainty in estimation. However, due to large difference of dust and smoke depolarization, the choice of exact value of smoke depolarization does not influence significantly the results, especially at the heights where smoke is predominant. Another source of uncertainty is choice of the dust lidar ratio, which can provide uncertainty about 5 sr. The estimations of smoke lidar ratio are qualitative, so in final plot we show values of lidar ratio for the smoke – dust mixture only.

Table 1: Could you add to the table the height intervals chosen for the analysis of the smoke layers and also an estimation of their lifetime?

Unfortunately we can not do it, because in many cases the smoke occurs at all heights, so it is not easy to define the height range. In Table 1 we provide results for the heights, where the ratio β_s/β is maximal, which is usually near the top of the smoke layer. The same is for the smoke layers life time. During December – January, the smoke occurred almost permanently, so it is difficult to define the beginning and the end of smoke episode. Besides, we performed measurements in the night time only.

Could any differences in the smoke properties be related to the age of smoke particles?

The smoke aging may contribute to observations; this is why we say that presented results should be taken as semi-qualitative only. Still results show the general tendency of the lidar ratio increase with RH.

Page 4, Line 113: for the range resolution of particle extinction coefficient it is not clear to me which height intervals are selected. Do you mean 50 m up to 1000m and 125 m from 1000m to 7000m?

Yes, range resolution is 50 m up to 1000 m and then it gradually increases up to 125 m at 7000 m range. Please, keep in mind, that we performed observations at 47 deg to horizon, so to get height resolution it should be multiplied by factor 0.73.

Page 7, Line 217: the authors probably mean "hydrophobic".

Yes, sorry. Computer corrected it automatically. Changed.

Page 11, Line 344: "spectrally independent refractive index". Please provide the selected values for this analysis.

In inversion we consider both real and imaginary parts of CRI in a wide range of values: $0<Im<0.02$; $1.4<Re<1.65$. This procedure was described in details in our previous publications, and we didn't repeat it here, because we did not focus on inversion in this study.

Page 12, Line 369: "so variation of the imaginary" add "part of the refractive index".

Corrected

Section 3.2: Please provide the spatial-temporal evolution of backscatter coefficient, water vapor and particle depolarization for these cases also.

Optical depth of aerosol layer was high for 13-14 and 23-24 April, so to get reliable reference point for backscattering calculation, several profiles should be averaged. Hence, we can't provide spatio-temporal distributions for backscattering as in Fig.3. We can provide range corrected lidar signal, or backscattering calculated by Klett. However, dust layer didn't change much during the night, so we think that no need to add extra figure. Besides, manuscript is already overloaded with figures.