1. “General: The paper is appropriate for AMT and clearly worthwhile to be published. It brings together state-of-the-art lidar-derived aerosol products with state-of-the-art aerosol modeling. And the agreement between the model and observational results is very good. However, the paper is very long and should be shortened to make it more attractive. Furthermore, I would emphasis the synergy (complementarity) of lidar-derived optical and microphysical aerosol properties and the complementary model-derived product (chemical composition, mixing state, aerosol components), rather than highlight the good agreement. Sure, good agreement is needed, before one can focus on synergy. All this is given in the paper, just need to be re-arranged.”

We are grateful to the Referee 3 for praise of our results and for numerous important suggestions. We tried to follow his recommendations and shortened the manuscript. Five figures are removed.

2. “Abstract should be short: goals, methods used, strategy, main results. Not more. Just facts.”

Abstract is shortened

3. “P2, L8-13: The first sentence should be improved, better separation of the aerosol effects on radiation, clouds, environment, and we need better observations as a function of height to characterize aerosols in terms of optical properties, size distribution, chemical composition (aerosol mixture, components, etc): should be the message.”

It was changed. Now it is:

“Atmospheric aerosols are an important factor influencing the Earth’s radiative budget, though its impact is still highly uncertain due largely to the complicated mechanisms of aerosol – cloud interaction. Aerosol particles serve as cloud condensation nuclei and ice-nucleating particles, providing strong impact on cloud and precipitation formation. However different aerosol types differ significantly in their ability to initiate drop and ice crystal nucleation. There is thus a clear need for a better knowledge on vertically resolved optical, physical and chemical aerosol properties.”

4. “P3, L 5-11: In the discussion of inversion problems, one should not forget the shape parameterization of dust particles as a significant error source.”

Added: “The irregularity of the particles shape can be also a significant error source.”

5. “P3, L24: I do not believe that MERRA-2 can help to improve lidar backscatter modeling, when even optical models usually fail to properly simulate dust backscatter coefficients. I would not concentrate on backscatter. This is a quantity only lidar people are interested in. But sure, the lidar ratio is an important aerosol typing parameter.”

We agree that extinction profiles are more important for users. Still we think that considering of backscattering is also useful. For low aerosol extinction the uncertainty of lidar ratio calculation is high, while backscattering still can be derived and compared with the model. Actually we observed good agreement between measured and modeled dust backscattering coefficient (at least at 355 nm).

6. “P6, L5: At the end of section 2.1 one should briefly describe the inversion technique and the retrievable products (shown in Fig 23 and 24).”

Paragraph with brief description of inversion is added.

7. P6, L21: Visit the papers from Kemppinen et al., 2015 (ACP or AMT, JQSRT) and you will see how complicated the simulation of dust backscatter as retrieved from lidar really is.

Yes, Kemppinen et al. studies point to the importance of internal structure consideration. But such computations are extremely time consuming and can't be done for big particles. So we have to use simplified ellipsoids/spheroids models at a moment.

Result section:

8. "First of all, I would like to make the following suggestions concerning figures. To my opinion one can reduce the number of figures significantly without losing information. Here are my suggestions:"

Five figures are removed.

"Figure 1 is ok."

"Figure 2 is not needed. Figure 3 is sufficient so that Fig.2 can be removed."

Fig.2 shows continuous monitoring of the dust and smoke layers for 4 days. We would like to show this long-term evolution, so would prefer to keep this figure.

"Figure 4 and 5 are not needed."

Fig.4 and 5 show the wind measurements. We think that it is important to show, that the wind in the dust and in the smoke layers has different direction and that the low level jets appearance is accompanied by the increase of the dust backscattering. So we would prefer to keep these figures.

"Figure 6 is ok."

"Figure 7 should be combined with Figure 8 to get a complete overview in just one figure."

Done

"Or combine Figures 8 with 9, and leave out water vapor here. It is not need at this time, later it is shown. That is sufficient. Or even Figure 7,8, and 9 could be combined to one overview figure."

Fig.8 is removed

"Figure 10 is ok, but why are not all three time periods (in Figure 7) shown? The x-axis value ranges should be the same for all curves. It is confusing when the ranges are different and one wants to compare the different profiles."

The scales are modified. Now these are the same. As mentioned in the text, the uncertainties of lidar ratio calculation below 2000 m for other two temporal intervals are two high (due to high gradients). So the results are not very informative and we didn't show these.

"Figure 11 is fine."

"Figure 12 and Figure 13 should be combined (to see the synergy we can have )."

Unfortunately the figure becomes overcomplicated, so we would prefer to keep these separate.

“Figure 13: To my opinion, the backscatter coefficients are not needed. The model uses given lidar ratios (40sr for dust, 80sr (?) for smoke) to convert extinction into backscatter: : : is my feeling.”

We think that backscattering coefficients are still useful. In the layers with low extinction coefficient the uncertainty of lidar ratio calculation is high, while backscattering coefficients still can be calculated and compared with model.

“I would remove Figures 14 and 15, because Figure 16 is sufficient. We should add 532nm solutions in Figure 16. Then this figure is very nice and convincing: : :. And shows the synergy of observations with models, together with Figure 13.”

We agree. The Fig.14,15 are removed and extinction at 532 nm is added.

“Figures 17 and 18 are not needed, to my opinion.”

We agree. Figures 17 and 18 are removed.

“Figure 19 is fine, brings some new results, and shows good agreement, a basic requirement

for the next step: synergy!”

“Figure 20 can be removed. Who is interested in backscatter coefficient comparison?”

As discussed above, at low extinction coefficient the uncertainty of lidar ratio calculation is high, while backscattering coefficients still can be used. So we prefer to keep the figure.

‘Figure 21 nicely illustrates that the spheroidal model fails when the particles are nonspherical. But this is not discussed. Should be improved.”

Actually we discuss, that spheroids model fails to reproduce the depolarization of pure dust. But modeling of backscattering coefficient, in principle, can be improved. In particular, model reproduces backscattering at 355 nm quite well.

“Figure 22 shows the water vapor comparison. This is sufficient for the entire paper.”

“Is Table 1 needed?”

We think it is useful to keep all these values in one table.

9. “After rearranging the remaining figures one needs to update the discussion. To say it again, one should focus on the synergy aspect, i.e., that observations and the MERRA-2 model contribute in a complementary way. The synergy of observations and models is a key to improve the characterization of aerosols. However, in step 1, one has to demonstrate how well observations and models agree and describe the same aerosol scenario.”

We introduced changes in the discussion in revised manuscript.

10. “P10 L3-18: The Angstom values for dust are not alwaysl negative, as suggested, when I am looking at all the field campaign data in the SAMUM and SALTRACE special issues. One should study the lidar papers in these special issues. Also, I miss again the discussion of the impact of the irregular shape of the dust particles as a significant error source.”

We have added comment, that backscattering Angstrom exponent strongly depends on the dust origin and no negative values of BAE were reported during SAMUM campaigns.

11. “P11, L25-26: There is a paper of Nisantzi et al. 2015 on the topic of depolarization increase by soil dust during fires”

Added to references

12. “P12, L5-9: I would not show a new period (for 3 UTC, Fig.13), not needed.”

We tried to show with this new period that lidar and model data are complimentary. Low depolarization ratio at ~2000 m height observed in the measurements can be understand from modeling results, pointing to the increase of organic carbon contribution at this height.

13. “P12, L22: The simulation of the backscattering coefficient is more challenging: : : As already mentioned, I would leave out the backscattering part. MERRA-2 cannot simulate backscatter, as so many other models, including all the optical models focusing on dust. They always fail, it is simply too complicated, see Kemppinen papers.”

Yes, it is challenging, still we think it is useful to show what model provides. And actually for 355 nm the model reproduces backscattering pretty well.

14. “P13, all the discussion should be shortened after re-arranging the figures starting from P13 and then on the following pages. Provide an attractive and short discussion. It is lengthy at the moment. What are the goals, should be the motivating question? And I believe, the demonstration of agreement (first step) is important but more important is the demonstration of synergy (second step)”.

After figures removal we introduced changes in the text and discussions, trying to follow reviewer recommendations.

15. “P16, L9: So both the lidar inversion method and the model are based on the same ‘wrong’ spheroidal model. So, no surprise when the lidar and model products agree here.”

Not exactly. Model provides the mass concentrations without considering any light scattering. So agreement actually demonstrates, that lidar inversion works not so bad.

16. “Conclusions, summary and abstract must be updated after finishing the updated result section.”

Updated

17. Congratulations to the nice results. The paper will be a significant contribution to the literature.

Thanks!