Atmospheric
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Discussions

Interactive comment on "Characterization of smoke/dust episode over West Africa: comparison of MERRA-2 modeling with multiwavelength Mie-Raman lidar observations" *by* Igor Veselovskii et al.

Anonymous Referee #3

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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.

Below, I provide a list of changes that would make the paper more attractive for non-lidar readers and would better indicate the new direction to use multiwavelength lidars and aerosol-resolving models in combination.

Major revisions are needed and will improve the paper.

Details:

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

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. . .

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.

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.

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).

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.

Result section:

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:

Figure 1 is ok.

Figure 2 is not needed.

Figure 3 is sufficient so that Fig.2 can be removed.

Figure 4 and 5 are not needed.

Figure 6 is ok.

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

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.

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.

Figure 11 is fine.

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

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.

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.

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

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?

Figure 21 nicely illustrates that the spheroidal model fails when the particles are non-spherical. But this is not discussed. Should be improved.

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

Is Table 1 needed?

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.

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.

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

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

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.

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).

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.

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

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