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Atmospheric Measurement Techniques Discussions

Interactive comment on "$3+2+\mathrm{X}$: What is the most useful depolarization input for retrieving microphysical properties of non-spherical particles from lidar measurements by assuming spheroidal particle shapes?" *by* Matthias Tesche et al.

Anonymous Referee #1

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The paper is appropriate for AMT and a good contribution to the literature of lidar methods and technologies.

However, improvements are needed. The discussion needs to be extended.

Throughout the paper it is assumed that the fine mode (particles with diameter < 1 micrometer) contains ONLY spherical (anthropogenic) particles. But in reality, the fine mode also contains non-spherical dust particles and may contain also non-spherical

smoke particles (pronounced accumulation mode). This can be concluded from the Burton et al. (2015) paper with a strong wavelength dependence of the depolarization ratio found for dry smoke particles in the upper troposphere. It was confirmed by Hu et al, (2019) and Haarig et al. (2018) for the 2017 stratospheric smoke event. All this is not considered in the paper. Thus, the recommendations given in the paper regarding the configuration of a multiwavelength polarization lidar are of limited use. It must be clearly stated for what scenarios the conclusions hold! Aerosol scenarios with a large non-spherical fine mode fraction are not considered!

And the other important point is that the study here is based on a spheroidal dust model. As long as a trustworthy dust shape model is not available, all the simulations, all the inversion results, are just speculation! This must be repeated more frequently throughout the text. There is simply no solid conclusion what configuration the best is, except 3+2+3!

Some details:

P2, L28-29: All these inversion methods with spheroidal dust particles are not convincing. Yes, the Tesche 2009, 2011 way is much more convincing! Why is the alternative concept (dust/nondust separation by means of the particle depolarization ratio) approach not discussed in more detail? There is also the next-step approach by Mamouri et al (2016 and 2017) to circumvent the spheroidal shape problems. A more complete discussion is needed. What does it help to have even a 3+3+3 system when we need to base the full concept on the questionalbe dust spheroidal model?

P4, L2: In the Hu et al 2019 paper (published in 2019), the extinction profiles are computed from the elastic channels only. No Raman lidar solutions, no 3+2+3. On the other hand, here you kindly provide these references on the dry smoke observations, Hu, Haarig! But then you ignore all these realistic aerosol scenarios in the rest of the manuscript.

P5, L16: Table 1 corroborates my opinion! The considered non-dust part is just spherical fine mode aerosol (urban haze, smoke). The paper does not cover the full reality of aerosol scenarios.

P9, L2-6: We need such statements more frequently!

P9, L7-L17: All this is confusing! Again, the Shin et al (2018) results are based on the spheroidal model. So why should the results agree with observations? And lidars do not measure lidar ratios and depol ratios at 870 and 1020 nm! Gasteiger played around with many shape configurations and failed because there is no realistic shape model for dust available. At least Gasteiger showed how sensitive all the modelled dust optical properties depend on particle shape.

P9, L18-27: The authors performed ... systematic investigations ... with a wrong shape model! ....in contradiction with Gasteiger and Freudenthaler! Sure, what did you expect?

At the end, the discussion gives the impression: Because we do not have a real alternative, we take the spheroidal model, and assume that all this is quite ok! But it is not ok!

Figure 2, and also P11, L15: Tesche et al 2009 is not the end of the street (separation of dust and non dust with one depol ratio). The Mamouri 2014 and 2017 papers extended the approach towards fine dust and coarse dust separation... That should be mentioned! They used different depol values for fine mode and coarse mode. And this is obviously the reality! And then you will get higher dust fractions than the one shown in Figure 2 by using the Tesche 2009 approach. All this is not mentioned in the manuscript. So the discussion is incomplete. Fortunately, it is considered in the inversion models (dependence of the depolarization ratio on particle size). The size dependence was already shown by Gasteiger and in many other papers and are now even measured with polarization OPCs (Tian et al., ACP 2018). However, one has to be careful, POPC measure at 120 deg, lidar at 180 deg. But the size dependence of the depol is similar to what is measured for example by Sakai, Appl. Opt 2010 or in the

AIDA KIT chamber... at 180deg.

Concerning the figures: It would be good to have the legend I: -, II: 355 nm, III: 532 nm always shown in all figures (as a tall table with 8 lines below each other). It will not take much space. I had to take a piece of paper, wrote down all the scenarios to have this information always present when stepping through all the figures....

Confusing Figure 6! The message is obvious, but the scatter in the results is large as well.

Confusing Figure 7: Set II / Set V can easily be interpreted as ratio.... One has to read all the text in the caption and in the figure carefully before looking at the results. And then again, the questions: what was Set I and what was Set V, and so on....?

Then Figure 8, ... even a further decrease in information, no x-axis and y-axis text anymore, just the hint: same as in Figure 7.

So, all in all, it was really not easy to review this paper and these figures... and thus: It was not a pleasure! So, please improve all this significantly.

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