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Atmospheric Chemistry and Physics Discussions
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Interactive comment on "A Backscatter Lidar Forward Operator for Particle-Representing Atmospheric Chemistry Models" *by* Armin Geisinger et al.

Anonymous Referee #1

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Review of:

A Backscatter Lidar Forward Operator for Particle-Representing Atmospheric Chemistry Models by A. Geisinger et al.

1 General comment:

The paper describes a lidar forward operator based on basic lidar equations for calculating the backscatter and extinction coefficient profiles from a atmospheric chemistry model (ACM). This forward operator can than be used to compare aerosol concentration in the model and at a later stage assimilate lidar backscatter profiles into ACMs. Beside the method, one special case study namely the ash plume of the Eyjafjalla-jökull volcanic eruption is used in the manuscript to demonstrate the applicability of the forward operator.

I appreciate the approach of calculating backscatter and extinction coefficients directly from the particle size distributions. Thus this is an appropriate and scientifically relevant contribution to ACP in general. The manuscript is also well organized and the analysis and results are clearly communicated and contextualized. However, I recommend major revisions due my major concerns listed below, i.e. the issues should be clarified and partly revised before the manuscript can be accepted for publication.

2 Major concerns:

1. In the manuscript, a very prominent but also very special case is selected for the case study. Earlier studies already showed that these volcanic ash particles were very aspheric and are therefore not the optimum case to test the forward operator under "normal" atmospheric aerosol loadings. Especially with the assumption of spherical particles. Would it be better to have chosen a more simpler case with more spherical aerosol particles available ? Can you please comment on the motivation for using this case study.

2. As you mentioned on page 12 l. 27. Schumann et al. 2011 found a mean aspect ratio of about 1.8 for particles smaller 500 nm and about 2 for larger particles. This particles where sampled in-situ with the Falcon aircraft and should provide the most realistic parameters for volcanic ash particles in the atmosphere. That means that the ash particles observed where more aspheric with higher aspect ratio than it was chosen in this study. I'm sure, if you would enhance the aspect ratio up to 2 in the T-matrix calculations which is actually the mean for larger

particles than 500 nm, it produces even larger deviation in comparison to the Mie calculations. Maybe the impact is smaller than I think, but this has to be shown in a more extended sensitivity study covering the whole range of aspect ratios. The seconds questions concerning the T-matrix calculation is, why do you have chosen a smaller aspect ratio of only 1.25/0.8 for the cylindrical shape? This should also be extended to higher aspect rations of about 2 to cover the range of the observations. Maybe it would also be good to add one comparable DDA simulations to the sensitivity study to see the full range of uncertainties even though it is only possible for sizes smaller 6.2 μm. However, this particles constitute the largest contribution to the total number concentration. Maybe it is also not worth to generate the same Figure 17/18 for one type of aspherical scatterers with the T-matrix method.

3. To make a fair comparison of ACL data with the model/forward operator all major uncertainties, i.e shape, refractive index, calibration constant, have to be estimated and combined to show if the statement concerning overestimation of particle number in the model is justified. As the authors mentioned in the text, the backscatter coefficient is much more affected by the particle properties than the extinction coefficient. Thus, the low lidar ratio with most values around 10-15 in comparison to the other studies by Kokkalis et al, 2013; Ansmann et al. 2010 and Mortier et al., 2013. is a strong indication for an incorrect/overestimated backscatter coefficient and could also explain the large discrepancy between modeled and measured backscatter coefficient profile. Only if both backscatter and extinction coefficient are higher in the forward operator simulated profiles in comparison to the ACL data, than this is an indication for a too high particle number concentration in the model. But this cannot be checked with the ACL data, because of missing independent extinction data. In summary, the conclusion of a wrong number concentration cannot be drawn from the case study with so many simplifications and without considering the errors/uncertainties of the forward operator.

I recommend to provide a comprehensive estimation of the combined uncertainties with consideration of point 2 above.

4. Some statements are to strong and cannot be drawn from this case study and from the implementation of the forward operator with it's huge uncertainties regarding refractive index and particle shape. For example in the conclusion section (p. 19, l. 13-14): How do you want to constrain or correct the model regarding shape and refractive indices ? Your forward operator use the refractive index as input and larger differences can also be expected from the simplicity of assuming spheres. I don't see a chance to retrieve refractive indices or shapes with the forward operator in combination with the ACL measurements, and thus this statement is not reasonable. In addition, the forward operator needs always additional information like the refractive index. So it is hard to apply the forward operator as a general/operational tool or even for assimilation without having these information available. So that the statement of a "crucial step" written in the abstract or similar in the conclusion is little over emphasized.

3 Specific comments/questions:

- Why are you using the Mie code with the assumption of spherical particles? Instead you could create a similar lookup table for aspherical particles with different aspect ratios and refractive indices.
- p. 11, l. 21-25: Calipso is used for estimating the calibration constant of the ACL lidar systems. How is this estimation performed? Usually, there are larger differences in space and time between local and satellite observations beside the viewing geometry. The brings me to the question how large is the uncertainty of this method also regarding to point 3 and also point 2?

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- p. 13, l. 13-15: Why should an ellipsoidal distribution lead to less realistic scattering calculation than spherical scatterers? This statement is a little bit strange, as you showed in the sensitivity study that more aspherical scatterers can better represent the observations. This statement should be removed or at least better explained.
- p. 16-17, l 34-2: The modeled structure can also be strongly overestimated by the forward operator and thus leads to visible structures in the simulated profiles. These structure could be real but just below the detection limit of the ACL measurements.
- p. 18., l. 8: It is written that both backscattering and extinction are higher compared to ACL observations. But the ACL cannot measure extinction profiles directly. So how did you compare the extinction profiles to the ACL observations ?

4 Technical comments:

- In the text and the especially in the figures a mixture of diameter and radius is used for the particle size. Particle size is sometimes written without mentioning either radius or diameter. I suggest to stay with one notation to avoid any confusion.
- Figures 8-11: I suggest to combine Figure 8 with 10 and Figure 9 with 11. There are not large differences in the outcome of the each figure pair and therefore the benefit of splitting the figures is low. This would reduce the number of figures which makes the manuscript more clearer. In addition, I recommend to scale the extinction cross section from 0-300 and the backscatter cross section to 0-25. The small zoom plots can be moved to the white spaces in the lower left corner

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or below the legend. This would enhance the visibility of the differences between the different types.

- p.11 l 18.: How large is the measurement error of the diameter to state a number with such an accuracy ? It seems the number of the aperture of the receiving telescope has to much decimal places. I suggest to reduced the value to only two decimal places. This would account for a measurement error for less than 1mm in diameter.

- p. 8: There are a lot of basic formulas in the manuscript which makes it hard to read. For example the formulas regarding molecular scattering can be referenced and thus removed from the text. The formulas 14-17 can be combined etc. This would make the text more clearer.

- p. 14 l 30: spelling error: with a diameter

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