Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-236-RC3, 2019

Atmospheric Measurement Techniques
Discussions

Interactive comment on "An experimental study on light scattering matrices for Chinese loess dust with different particle size distributions" *by* Jia Liu et al.

Anonymous Referee #4

Received and published: 17 December 2019

The study presents light scattering measurements of Chinese loess dust. The authors have measured the scattering matrix elements of a single loess sample from the Chinese loess plateau once untreated (pristine loess) and once milled (milled loess) and performed some complementary measurements too. I find the topic very interesting and useful. However, I have some doubts about the paper being published in its current form.

First of all I have to question if the choice of the journal is adequate for the performed study. I might be wrong about this and if this is the case, then please just ignore this comment. However, this journal is called Atmospheric Measurement Techniques,

and on its homepage it is stated that: "The main subject areas comprise the development, intercomparison, and validation of measurement instruments and techniques of data processing and information retrieval for gases, aerosols, and clouds." This paper presents none of them. It shows some laboratory measurements with atmospheric relevance. It does not show a new measurement technique nor a newly developed instrument neither any instrument intercomparison. The only technical part of the paper is the one page section of 3.2 where the measurement apparatus is shortly introduced.

My other main concern is: if the manuscript contains strong enough scientific material to be published in AMT. The scattering matrix element measurements of the two differently treated loess sample come from 6 single measurements, and the manuscript is based completely on this. It would considerably improve the manuscript if more measurements were included. To give you some ideas: include measurements and a comparison of different kind of loess samples collected either on the Chinese Loess Plateau at other places or get loess samples from outside of China. Another idea could be to include some other types of mineral dust and make a comparison. I know well, that it is not always possible to perform more measurements additionally. The manuscript could be improved with much thorougher discussion about comparing existing literature data with your dataset as well, or perform some numerical simulations based on the measured size distribution and shape (e.g. Mie theory and a theory for non-spherical particles) and discuss the results.

You could also improve the paper by stating clearly what your main message is for the reader. You just present the scattering matrix elements but do not draw any further conclusions. How is Chinese loess scattering treated in radiative transfer models? Will there be a big difference if these models are updated with your results? How representative is your single loess sample?

You probably cannot implement all of my main suggestions to improve the manuscript, and it is not necessary either. I just wanted to show you some possible options how it could be done. The data and the work you do is valuable but I only can recommend the manuscript's publication if it is significantly improved.

Other Comments:

1. I suggest a careful English language editing of the manuscript.

2. Page 4, Lines 99-111: Even after a longer search I could not find details about the SALD-2300 instrument and how it exactly measures the particle size distribution and refractive index. Please add details how it exactly works. What I think it does is measuring the light scattering at many angles and trying to reproduce the measurement with a guessed number size distribution and a refractive index using theoretically calculated scattering values. Does it use the Mie theory (which is valid for spherical particles only)? Or how can it calculate the scattering for particles with unknown shape? How does it influence your derived number size distribution and refractive index? What is the uncertainty of this measurement method for non-spherical particles? Please add a discussion on this. Are you sure that the refractive index difference between 1.65+0i for "pristine loess" and 1.70+0i for "milled loess" is real?

3. Page 4, Lines 110-111: "larger particles have relatively larger real part of refractive index": if I understood correctly your method of producing the milled loess sample, it contains exactly the same material (your chemical analysis verifies it) as the pristine loess and therefore one would expect the two samples having the same refractive index. Are you sure, again, that your result is real and are not only a measurement artifact/uncertainty? Or do you think that the milling caused some strange structural changes in the loess sample which homogenized or inhomogenized how the chemical components are distributed within a single particle and/or between the particles?

4. Page 6, Section 3.2: I assume that this is not the first paper which uses this experimental apparatus. Please add a reference to the paper where a more detailed description of your instrument is available. If there is no such paper, please add a more detailed description.

5. Page 6, Lines 59-62: Since your main results are the measured matrix elements, probably it would be worth explaining exactly from which polarization states which matrix elements were derived and how, and not only referencing a paper for it.

6. Page 7, Line 193: "all six non-zero matrix elements are limited to narrow regions, respectively" I don't understand what you mean here. What narrow regions? Angle range? Y-value range? Or do you mean that your error bars are small? Please clarify!

7. Page 7, Lines 199-201: Please comment on the angular behavior of F_22. Next to it: it looks like that the milled loess sample deviates more from unity than the pristine loess sample. Does this suggest that the milled loess has a more irregular shape than pristine loess?

8. Page 7, Lines 206-207: The sentence is very confusingly phrased, please rephrase it. I am not sure if I understood what you wanted to tell the reader but I don't see any significant difference between the 5° relative phase functions.

9. Page 7, Lines 206-208: From F11 it looks like that milled loess has a higher forward to backward scattering ratio than pristine loess. I would expect exactly the other way around because the pristine loess sample contains much larger particles and larger particles usually have a much higher forward scattering compared to the backward scattering value. Please comment on it.

10. Page 8, Lines 119-223: Is there no way to produce samples containing smaller particles than the original without changing their form? Just by sieving the sample (the size distribution of the pristine loess seems to me broad enough)? Would that not work? If it would, then measuring such samples could save you from speculating about, if the measured differences are due to the different size or shape. It would be also very nice to have more samples with different sizes and not only two. You show that the particle size differs much more than the shape between the two samples, and your speculations might be true as well. However, how can you be sure that every component of the scattering matrix is comparably sensitive to the changes in size and

shape? Let's assume, that one matrix component is 1000 times more sensitive to the changes in the particle shape than to the changes in the size? Please provide some proof that such a case is not to be expected, and then your argumentation becomes valid.

11. Page 8, Lines 224-240: It would considerably strengthen the manuscript if numerical calculations based on your measured size distribution and particle shape were added and not only the existing literature was analyzed. If that is not possible, you should show how the size and shape of your samples compare to the size and shape of the particle in the referenced papers. Irregular dust does not necessary mean comparable size distribution and/or particle shape.

12. Page 9, Section 4.2: During calculating the synthetic scattering matrices you follow the works of Dabrowska et al., 2015 and Escobar-Cerezo et al., 2018. They used the very same measurement technique, had only different kind of samples (Lunar and Martian dust). You clearly follow their work, by extrapolating the measurements to the angles you could not measure as well. The extrapolation in the forward direction is based on the Mie theory and is performed for a narrow angle range of 0-3° or 0-5° (in your case). This is for me a justified assumption. However, in the backward region, the extrapolation is based on a polynomic fit and not on any kind of scattering theory. In this case, I can believe that it works well for the very narrow 177-180° angle range in the works of Dabrowska et al., 2015 and Escobar-Cerezo et al., 2018. But you applied it for a much broader angle range of 160-180°, and here I really need some solid proof of this method being justified. The later calculated back-scattering depolarization ratio values cannot be accepted either before your extrapolation is not verified.

Technical Comments: I did not do any language/technical correction because the manuscript needs a bigger revision.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-236, 2019.