Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-267-AC2, 2019

Atmospheric Measurement Techniques Discussions

Interactive comment on "Challenges in retrieving stratospheric aerosol extinction and particle size from ground-based RMR-LIDAR observations" *by* Jacob Zalach et al.

Jacob Zalach et al.

zalachj@uni-greifswald.de

Received and published: 13 December 2019

We thank the reviewer for his comments and suggestions. In addition to our general answer from 12. September we would like to respond to the details of the comment published by anonymous reviewer #1 on 05 September 2019.

Referee comment: "The paper is to my opinion not ready for publication. The authors present a lidar retrieval technique which is not new and thus does not justify publication. The presented sensitivity study belongs to the description of the methodology Thus, the sensitivity study also does not justify publication.

However, a new aspect would be if the authors would apply their (updated) lidar method to observations from 2000 to 2019 (they have these observations, this is mentioned in the abstract). Therefore, presently, I recommend rejection. I would accept the manuscript if they concentrate on the data analysis and show long-term measurement results and discuss them. At least if there would be a section with new results (2000-2019 observations)."

Our response: We are sorry that the essential point of our work was hard to find in the text and we hope to have clarified it with our first answer. As a short reminder we can state that the novel aspect lies in the methodology itself which - to our best knowledge - has never been applied to study stratospheric aerosols. One feature of our approach is, in contrast to most studies of lidar measurements of stratospheric aerosols, that we do not need do assume a value of the lidar ratio.

Taking into account the large differences in stratospheric aerosol size published in earlier studies and the overall importance of stratospheric sulphate aerosols (particularly of volcanic origin) for climate, we strongly believe that the manuscript is of relevance for the community and worth to be published.

Based on this we would strongly prefer to separate the publication of the method itself and the results obtained with it.

Referee comment: "Abstract: What aerosol type do you expect in the stratosphere? Nothing is specified. Obviously only aged volcanic sulphuric acid solution droplets (so that Mie scattering in fully applicable). But, volcanic ash or biomass burning smoke (injected by pyrocumulonimbus activity) can be present as well. These particles may be non-spherical and may have quit complex refractive index characteristics."

Our response: We expect stratospheric aerosols with properties listed in section 3.1. Essentially we expect a spherical droplet of water and sulphuric acid without absorption and a log-normal size distribution. All other potential types of stratospheric aerosols are neglected in this study. Non-sulphate aerosols are planned to be considered in future work. We also note that many studies on stratospheric aerosols assume that the stratospheric aerosol population is strongly dominated by suphate aerosols (Bingen at al (2004a), Bingen at al (2004b), McLinden et al. (1999) , Bourassa et al. (2008) , Taha et al. (2011), von Savigny et al (2015)).

Referee comment: "Section 2. P3, L61: The RMR lidar is described in von Zahn et al. (2000). . .. My question: No new aspects, new channels, new receiver design etc. . . since then, no published updates (articles)? Obviously the RMR lidar has no depolarization channels, and thus no information about particle shape and thus aerosol type is available."

Our response: As we hope to made clear, the novelty is the application of this method to stratospheric aerosols. Since the article focuses on the description of the method, the lidar device itself is not described here, we just reference the main work concerning it (Langenbach at al (2019) and von Zahn et al (2000))

Referee comment: "P4, L95: The sulphuric acid content (here of 75%) is not fixed and changes with temperature. "

Our response: This is true but this assumption is made very often. We assume that the change of the sulphuric acid content is considered through its influence on the refractive index, which we vary in section 4 in order to quantify its influence on the results. Our variation of the refractive index corresponds to a sulphuric acid concentration variation of 20%.

Referee comment: "P4, L101-106: What about papers of Jaeger et al., Hofmann et al., Deshler et al. (mostly in GRL and JGR)? They use balloon (in situ) observations of aerosol size dis- tributions over Wyoming and published microphysical properties of measured strato- spheric aerosols . . . Are these papers (and measurements) in agreement with your assumptions?"

Our response: In section 5.1 we compare our results with works of McLinden et al.

(1999), Bourassa et al. (2008), Ugolnikov et al. (2018), Bingen et al. (2004) and Deshler (2008).

Under volcanically quiescent conditions most of this studies are in good overall agreement with our results. Only the work of Bingen et al. (2003, 2004a, 2004b) which analyses SAGE II data yields much larger radii of several hundred nm, even in the late 1990s, when the Pinatubo aerosol has already almost entirely disappeared. These discrepancies may in part be a consequence of different sensitivities to the aerosol particle population in combination with errors in the assumed PSD.

Referee comment: "P5, Eq.(5), why is k_Mie in the equation and not k_Ray?"

Our response: Thank you, this was obviously wrong.

Referee comment: "k_Mie/Ray , P_Mie/Ray is misleading, could be related to a Mie/Ray signal ratio."

Our response: Thank you for pointing out this misleading expression. We have corrected this.

Referee comment: "P5, L121-125: The profile of the particle backscatter coefficient at 1064 nm is not easy to calibrate. How do you do that in practice. Use of cirrus? Assuming same backscatter coefficient at 532 and 1064 nm? Please explain. "

Our response: Please note that we do not make use of the particle backscatter coefficient at 1064 nm to calculate the color ratio from lidar data, we just use the backscatter ratios (First line of Equation 8). The following discussion is to show how the colour ratio depends on the Mie backscatter coefficients and how to calculate the colour ratio for the forward model.

The backscatter coefficient at 1064 nm is calculated from the backscatter ratio, the ECMWF model density and the Rayleigh backscatter coefficient $\beta_{Mie}(z, 1064 nm) = \beta_{Ray}(z, 1064 nm) *(R(z, 1064nm)-1)$. In our case the we normalize the backscatter coefficient at 1064 nm to the Rayleigh backscatter coefficient at 34 to 38 km altitude (See Langenbach et al., (2019) for the normalization procedure).

Referee comment: “So, at the end of subsection 3.3 I already asked myself: What is new?”

Our response: We have tried to make the new aspects more obvious in the revised manuscript. One core aspect is the simplicity of the method. It produces results which compare well to satellite data under the current aerosol load of the stratosphere.

Referee comment: “P7: You computed the monthly mean backscatter coefficient profiles first. Then you calculated color ratio, median radius, etc.! Does that make sense? I was expecting, you make use of single nighttime observations. For one night, the aerosol characteristics may be constant, but over a whole month (30 days)?”

Our response: Your objection is of course right, but in this work we want to focus on the description of the method. The detailed analysis of single night-time measurements with a high sample rate can of course be done and will be made in the future. But such an analysis would require a discussion of the results which would take the focus away from the method applied. We wanted to avoid such a situation.

Referee comment: “Fig.2: Shown is NOT the monthly mean Median Radius! Shown is the Median Radius computed from the monthly mean backscatter coefficient Again, in Fig 3, the extinction coefficient is derived from the monthly mean data of aerosol backscatter proïňĄles . . . and not obtained from individual night-by-night observations and subsequent averaging!”

Our response: The reviewer is absolutely right, our description was not correct. Thank you for pointing this out. The sentences were adjusted accordingly.

Referee comment: “The lidar ratio (Eq. 12) is the solution of an ill-posed problem and thus not just reliable. At the end of section 4, I asked myself again: What is new?”

Our response: As hopefully clarified the novelty of our method is its application on stratospheric aerosols. Concerning Eq.12 we do not claim that this approach is reliable,

but it is often applied and here we compare this approach with our method. We would appreciate if you could be more specific here.

Referee comment: “The presented comparisons in section 5 are not very convincing!”

Our response: Our comparison includes all relevant aspects - but you are right, the agreement is not that good. However, we would like to point out that data sets provided by different satellite measurements also often deviate from each other.

Referee comment: “So, my conclusion is: One should analyse the available 2000-2019 stratospheric lidar data set. If thes key findings are added, then a publication may be justified (provided the presentations, discussions, and conclusions are acceptable). Without these observations, I recommend: rejection.”

Our response: At this point we would like to express our hope again that we were able to better show the essential point of our work, the application of the described method on stratospheric aerosols. Therefore, we would strongly prefer to publish the obtained results in an future, separate article.

Literature used:

Bingen, C., Fussen, D., and Vanhellemont, F.: A global climatology of stratospheric aerosol size distribution parameters derived from SAGE II data over the period 1984-2000: 1. Methodology and climatological observations, J. Geophys. Res., 109, D06201, doi:10.1029/2003JD003518, 2004a

Bingen C., Fussen D., and Vanhellemont F.: A global climatology of stratospheric aerosol size distribution parameters derived from SAGE II data over the period 19842000: 2. Reference data, J. Geophys. Res., 109, D06202, doi:10.1029/2003JD003511, 2004b

Bourassa, A. E., Degenstein, D. A., and Llewellyn, E. J.: Retrieval of stratospheric aerosol size information from OSIRIS limb scattered sunlight spectra, Atmos. Chem. Phys., 8, 6375-6380, 2008

Deshler, T.: A review of global stratospheric aerosol: Measurement, importance, life cycle, and local stratospheric aerosol, Atmos. Res., 90, 223-232, doi:10.1016/j .atmosres.2008.03.016, 2008

McLinden, C. A., McConnell, J. C., McElroy, C. T., and Grif?oen, E.: Observations of stratospheric aerosol using CPFM polarized limb radiances, J. Atmos. Sci., 56, 233-240, 1999

Taha, G., Rault, D. F., Loughman, R. P., Bourassa, A. E., and von Savigny, C.: SCIAMACHY stratospheric aerosol extinction profile retrieval using the OMPS/LP algorithm, Atmos. Meas. Tech., 4, 547-556, doi:10.5194/amt-4-547-2011, 2011.

Ugolnikov, O. S. and Maslov, I. A.: Investigations of the Background Stratospheric Aerosol Using Multi- color Wide-Angle Measurements of the Twilight Glow Background, Cosmic Research, 56, 2, 85-93, doi:10.1134/S0010952518020119, 2018, Original Russian Text, 2018, published in Kosmicheskie Issledovaniya, 2018, 56, 2, 95-102, 2018

von Savigny, C., Ernst, F., Rozanov, A., Hommel, R., Eichmann, K.-U., Rozanov, V., Burrows, J. P., and Thomason, L. W.: Improved stratospheric aerosol extinction profiles from SCIAMACHY: validation and sample results, Atmos. Meas. Tech., 8, 5223 - 5235, 2015.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2019-267/amt-2019-267-AC2-supplement.pdf

---

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-267, 2019.