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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.

Anonymous Referee #1

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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).

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Discussion paper

Details

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.

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.

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

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 distributions over Wyoming and published microphysical properties of measured stratospheric aerosols . . . Are these papers (and measurements) in agreement with your assumptions?

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

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

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.

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

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)?

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 profiles . . . and not obtained from individual night-by-night observations and subsequent averaging!

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?

The presented comparisons in section 5 are not very convincing!

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.

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