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Atmospheric Measurement Techniques Discussions

Interactive comment on "Evaluation of a Method for Converting SAGE Extinction Coefficients to Backscatter Coefficient for Intercomparison with LIDAR Observations" *by* Travis N. Knepp et al.

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We thank the reviewer for reading this manuscript and providing feedback. Below are our responses to the reviewer's comments. Reviewer's comments are in black, our responses are in red.

Line 24: "This technique allows for high-precision measurements on the order of 5% for aerosol extinction..." - This statement requires attribution.

There is no single publication to reference this statement. However, the precision is reported in the data product. The text has been updated to reflect this.

Line 123:Assuming that the PSD is single-mode log-normal definitely reduces the problem to a manageable solution space, but was any analysis done to estimate how your conclusions might change if you assumed someother model? In particular, in-situ stratospheric aerosol observations (seehttp://www.atmosp.physics.utoronto.ca/SPARC/index.html, for example) frequentlyshow multiple modes, and the different particle sizes represented in those modes clearly have the potential to affect the extinction and phase function differently.

Particle size distributions (PSDs) with more than one mode were not evaluated within this work. We agree that using single-mode distributions greatly simplifies/reduces the solution space. The intent of this work was to evaluate how well derived backscatter coefficients agree with lidar products within the confines of our assumptions, one of which was single-mode distributions. We agree that most stratospheric, non-background, PSDs will have more than one mode; this was certainly the case after Pinatubo. Further, recent work by von Savigny & Hoffmann (AMT, 2020) may be indicative of backscatter being less sensitive to multi-modal PSDs than extinction. However, this certainly merits further analysis to reach a non-speculative conclusion.

Line 127: The symbol r_m is frequently called the "mode radius" in the aerosol literature, but it actually represents the median of the distribution (See Johnson, NormanL.; Kotz, Samuel; Balakrishnan, N. (1994), "14: Lognormal Distributions", Continuousunivariate distributions. Vol. 1, Wiley Series in Probability and Mathematical Statistics:Applied Probability and Statistics (2nd ed.), New York: John Wiley Sons.)

This is a great semantics point. I (TNK) have found the reference to "mode radius" in the literature to be confusing because, as the reviewer points out, this actually refers to median radius. We refer to r_m as mode radius to be consistent with the literature. However, clarification has been added to the text to aid the reader.

Line 129: "...a new log-normal distribution as r_m took on each value within r." Is this accurate? If so, then you considered distributions for which r_m occurred at the smallest (and largest) possible particle size? This seems ill-advised from a mathematical perspective (& doesn't really yield a "log-normal" distribution in any meaningful sense). Your solutions lie comfortably in the middle of the range given, so this is probably a minor point, but perhaps the description should be re-written?

There are two points that need addressed, neither of which impact the results of this work in any way. First, the radius range stated in the paper was incorrect. The range used to generate all tables, figures, etc. extended from 1 to 1500 nm. This was just a typing error in the manuscript (not the analysis code) and has been corrected. Second, the mode radii that were actually *needed* for this analysis (i.e. to regenerate the SAGE-based extinction ratios) were in the range of 50-500 nm, well away from these bounds. This has been clarified in the text.

Line 159: Were any comparisons made between the derived beta_355 and the SAGE measurement of aerosol extinction at 385 nm? That SAGE product is generally understood to be lower in quality than the aerosol extinction at longer wavelengths, but it isa bit strange not to use it, or even mention its existence as an option.

We are unsure of what the reviewer is asking here. The 385 channel was used in the analysis (used 385/1020 extinction ratio to estimate backscatter at 355). However, if

the reviewer is asking about a *direct* comparison of extinction at 385 to backscatter at 355, then no, we did not perform this comparison. This type of comparison is outside the scope of this manuscript since we were evaluating the performance of an extinction-to-backscatter algorithm.

Figure 2 is a bit blurry

The figure supplied to Copernicus is the same resolution as all other figures, so it should be of sufficient quality (it looks good on my copy as well). However, I will pay attention to this during the final printing to ensure sufficient quality and provide a higher-resolution image if needed. We appreciate the reviewer pointing this out since blurry figures can make interpretation challenging.

Figure 3: I'm not sure that I understand the meaning of the dark red vs. light red vs.gray regions.

The confusion may be caused by the legend only having two colors (light-red and gray). The dark-red region is where the two distributions overlap. The caption has been updated to clarify this.

Line 280 - This "atmospheric opacity and cloud contamination" concerns here clearly-correspond to SAGE measurements, correct?

Correct.

Line 323 - Is there a reference for the details of the “new lidar instrument setup” mentioned here? It might provide helpful context for the sudden change observed in the comparison.

The 1995-1998 period is an early period for the Mauna Loa system. Although technical problems during this period were reported in the public NDACC meta data archive, many minor setup changes were made without being reported in a publication. Therefore there is no specific reference we can provide besides the main [McDermid et al., 1995] publication pertaining to the original setup (we added this publication to the references). These things said, the sudden changes at the lower end of the profiles can only be caused by either slight beam/telescope misalignment, or spectral leakage in the 355 nm or 387 nm filters, or a combination of both.

Line 337 - “Smoke from the pyroCB was visible over OHP, but not over Mauna Loa.”This seems reasonable, but what evidence is it based on?

A citation has been added regarding detection of smoke over OHP. Based on global observations (e.g., GloSSAC, OMPS/LP), there is no clear evidence of significant aerosol loading increase at low latitudes associated with the 2017 PyroCb plume, although Chouza et al. (2020) (we added this publication to the references) showed a very slight increase of sAOD values during that period.

Line 342 - “precision/accuracy...is too limited to make meaningful measurementsduring background conditions.” This is an alarming statement on its face, but I assumeyou only mean to exclude the possibility of meaningful measurements of the beta_355 parameter under discussion.

Correct.

General - The units of the lidar ratio (S) should be presented consistently (sometimesit has no units, sometimes sr).

Updated throughout text.

Line 88 – “Bakckscatter”

Corrected

Line 278 / Equation 5 – Lidar should be subscripted in denominator

Corrected