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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.

P1L14: Write out abbreviations for the first case use (e.g. SO2)

Corrected

P2L36: The discussion on Lidar Ratio, S, needs improvement and more adequate literature referencing. For instance, there is no discussion of spectral differences between SAGE/lidars and how that may impact the lidar ratio. The value of S greatly depends on the wavelength of laser used. There is also no description on how you will interpolate/extrapolate an altitude varying S.

This paragraph has been updated to point out spectral differences between SAGE and lidar and the references have been updated as well.

P4L80: I agree this effort is likely more important to do with the three selected ground based lidars. However, it woud be useful to have a follow on study utilizing additional data sets from NASA's MPLNet or the European EARLINet. An ideal case (perhaps there is a case study already) would be to use an event where two lidar sites could provide the vertical distribution of aerosols coincident with a SAGE occultation observation.

We agree that a follow up study using MPLNet or EARLIENet or another lidar network would be interesting. However, we note the challenge in using MPLNet and EARLINet data as, from our evaluation of the data from these networks, data collection typically stops just beyond the tropopause. However, this method is being applied to ongoing research which will be the topic of future publications.

P6L125: How can you parse out the vertical distribution of aerosols vs. the horizontal inhomogeneity? There needs to at least be a description of the uncertainty associated with the measurement EBC assumptions. Will water vapor contamination in the longer wavelength bands become a source of further uncertainty?

The reference you provided (P6L125) does not correspond to anything related to this comment. However, this comment is relevant and has been addressed in the updated manuscript. First, the uncertainty associated with the EBC assumptions were addressed in section 3.2 of the original manuscript. Second, to date, similar studies that compared SAGE profiles with sonde or lidar data have taken advantage of the horizontal homogeneity of the atmosphere. This has been explained, with appropriate supporting references, in the updated version of the manuscript. Regarding water vapor, this is not a factor since water vapor's absorption at 1020 nm is insignificant compared to aerosol extinction as stated in Damadeo et al. 2013.

I'm hesitant that a single value will be able to account for these. For a paper that leans so heavily on the assumption of sphericity in particles, I was surprised to not see a single mention of aerosol polarization/depolarization measurements from either ground-based of spaceborne (CALIPSO, CATS) instruments. These have long been known to provide context for optical and microphysical properties of aerosols. This manuscript could benefit from a short case study in which the authors show a proof of concept with a known event, rather than just grab bulk aggregate statistics that have no physical meaning.

We agree that a using both CATS and CALIOP for an extended/detailed examination of this methodology would be interesting and scientifically beneficial. Further, data from the CALIPSO lidar would provide valuable information and an interesting series of case studies. The challenge in using CALIPSO lidar data is the poor signal-to-noise ratio for aerosol backscatter in the stratosphere. This is currently being investigated and will be the subject of a future publication.
The CATS lidar may provide another dataset for evaluation. However, achieving appropriate coincidence is challenging due to both instruments (SAGE III and CATS) being mounted to the ISS.

Figure 3: Does this suggest that there are geophysical differences in the aerosol loading during the SAGE II/III time periods? Should the width be the same in A/C in non-Pinatubo times?

Yes, this indicates a geophysical difference. This is due to differing levels of volcanic activity. Additional comments have been added to the text.

Figure 6: Is the SAGE data noisier towards the end of the record? Are results any different if you remove the last year? In general, is there something geophysical occurring that is decreasing the spread of the S value over time or that simply lack of signal? Also, the legend is obscuring the data.

The last year has no impact on the analysis by itself. The spread in lidar ratios decreased because of changes in extinction coefficients. This was caused for two reasons: 1. the variability of extinction observations was higher early in the record (product of El Chichon and subsequent eruptions) and decreased as quiescent conditions were achieved late in the SAGE II record; 2. as aerosol size distributions changed so too did the extinction ratio. The legend issue was corrected.

Figure 8: Is there an explanation for the discrepancy in MLO BC after âĹij2000? Is this cloud contamination? It looks to be consistent – was there some calibration that was changed? The S value at 15km at MLO is much lower after 2000 than other site, which does not seem reasonable for stratospheric aerosol which would largely be well mixed. I'm sure this was verified but are the altitude layers for SAGE and lidar both in ASL?

We see no discrepancy between panels B and C in Figure 8, so we are unsure of how to address this concern. We can say that some of the difference between MLO and other sites will be due to latitude (MLO being tropical, hence a tropopause on the order of 15 km). Yes, all data sets had altitudes in ASL. From what we see everything in this figure is reasonable.