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Interactive comment on "A global, space-based stratospheric aerosol climatology: 1979 to 2016" *by* Larry W. Thomason et al.

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General Comments

This paper describes the construction of an observation-based stratospheric aerosol data set covering the time period 1979-2016. The methodology of the construction method is clearly described with a sufficiently high degree of detail. The dataset represents a significant update to prior products which have been widely used, through the use of additional data sources and improved gap filling procedures. If anything, the importance of the data set is undersold in the paper's introduction: observation-based volcanic aerosol data provides both the basis of all volcanic forcing reconstructions used in CMIP type experiments (over both historical and paleo- time scales) and is es-

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sential information for validating interactive aerosol model simulations. I find the paper well within the scope of ESSD, and expect it will be an important foundation for future research efforts.

There is one important issue that is largely missing from the dataset and from the description paper: uncertainties. Including some measure of the uncertainties in the extinction data product would be highly beneficial, for example, for accurately assessing consistency of aerosol model results with the data, or for estimating uncertainties in volcanic forcing reconstructions based on the data. Furthermore, it is really only with estimates of uncertainty that we can quantify improvements in data over time, and thus quantify the impact of the immense effort that goes into constructing such observational data sets. A full uncertainty analysis is perhaps beyond the scope of the current paper, but I hope the authors will consider the issue for future iterations. For the present, a critical piece of information which could be included is the authors' expert assessment of the overall systematic uncertainty in the aerosol extinction values, particularly during the Pinatubo period. From a cursory analysis of the new GloSSAC data, I find the global mean SAOD (time integrated) for Pinatubo to be about 20% larger than that of the CCMI version. Comparison of the GloSSAC derived SAOD with that from AVHRR suggest a rather large difference, perhaps even 100% during the Pinatubo peak. Having some knowledge of the systematic uncertainties–whether they are order 10% or 100%–could have important impacts on the way they are used.

The comparison with AVHRR is very important, but seems under-developed. Particularly, the attempt to construct a stratospheric AOD from the AVHRR total AOD measurements is obviously difficult, but subtracting a long-term median climatology doesn't seem to work, as it appears to result in many negative values, especially during the $\sim$2000 SAOD minimum, suggesting that the long-term median is too large as an estimate of the tropospheric AOD. This type of method also assumes that tropospheric aerosol is relatively constant over the time period, which seems unlikely. Past studies (e.g., Russell et al., 1996) have often subtracted the years prior to the Pinatubo erup-

tion to approximate a SAOD timeseries during the Pinatubo period, a method which has shown some degree of agreement with SAGE , perhaps that method could be used here to assess the inter-instrument difference in the peak Pinatubo SAOD, which is probably the most important issue of interest. On the other hand, it would be best to incorporate the obvious high level of uncertainty in the AVHRR SAOD estimates due to the attempt to remove the tropospheric aerosol contribution.

On the many latitude/time contour plots, I find numerical contour labels to be in most cases unreadable and detracting from the overall quality of the plots. In at least one case (Fig 13), the exponent in the labels appears to be missing a minus sign. I would personally rather see the plots use a log-color scale (as they do) but with real valued labels on the colorbar (rather than the log(EXT) values), but I leave that to the discretion of the authors.

The manuscript focuses on aerosol extinction coefficient at various wavelengths, however the GloSSAC dataset which I downloaded contains a number of "derived products" like effective radius and surface area density (SAD). The existence of these products in the data set should at least be mentioned, with reference if possible to descriptions of the methods of their derivation (perhaps to come in a future paper?).

Specific comments

P4, l32: “The data are”. . . missing word(s) here?

P6, l6: “by ice clouds those”. . . missing word(s)?

P6, l39: I didn’t understand “the use of short duration events. . .”

P7, l3: I’m not sure about the phrase “well correlated” with regards to the HALOE/SAGE II comparison, there appears to be a fair amount of scatter. Is the correlation coefficient large enough to support the "well correlated" description?

P7, l38: can the authors provide some estimate of the order of this error?

P8, l6: "...CLAES observations, the..."

P9, l4: as->at ?

P9, l10: does "well sorted" mean monotonic? "Sorting" strikes me as something you do to data, whereas I think the assumption here is that in reality, the aerosol extinction gets smaller with increasing equivalent latitude, which could be more clear.

P9, l15: "from MERRA"

P9, l16: I think the references should be to Figure 10b and 10c.

Fig 10: why is there a gap during the Pinatubo tropical extinction peak in panel b but not the other panels?

P9, l19: "considering that the scale..."

P9, l33: It wasn't clear to me at first that "SAM II events" are measurements, perhaps the language could be improved here.

P9, l38: "reconsider the role of SAM II"

P10, l31: These data are also made at...

P11, l28: I don't understand "where the backscatter signal for nighttime profiles calibrated at higher altitudes".

P11, l33: represents

P12, l3: "below clouds due to..."

P12, l12: Cloud screening was an important part of the methodology during the SAGE II period, there should probably be a clearer motivation for why OSIRIS data is uniformly cut off 2 km above the troposphere.

P12, l26: It's not quite clear if the "method used for SAGE II" includes the equivalent latitude method or not.

P12, l28: something about this sentence is a little wonky.

P12, l38: The scatter in Fig 12b–while blobby–does appear to have a clear structure, with the extinction-to-backscatter ratio depending on the OSIRIS extinction coefficient. The likely impacts of assuming a static extinction-to-backscatter factor should be mentioned–is it likely the derived CALIPSO extinctions are an over-estimate for low extinction values (and vice-versa), or is it the other way around?

P13, l13: “should be retained”

P13, l29: It took me a while to understand Fig 14. It might be worth reminding the reader that before 2005, the extinction ratio is based on actual measurements at those two wavelengths by SAGE II, while afterwards, it is simply reflecting the ratio based on the relationship shown on Figure 7.

P13, l38: “able to leverage”

P15, l20: the Sato et al., reconstruction is at 550 nm.

References

Russell, P. B., Livingston, J. M., Pueschel, R. F., Bauman, J. J., Pollack, J. B., Brooks, S. L., Hamill, P., Thomason, L. W., Stowe, L. L., Deshler, T., Dutton, E. G. and Bergstrom, R. W.: Global to microscale evolution of the Pinatubo volcanic aerosol derived from diverse measurements and analyses, J. Geophys. Res., 101(D13), 18745–18763, doi:10.1029/96JD01162, 1996.

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