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Discussions

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

Measurement uncertainties are included in the data set only for the SAGE II portion of the data sets. In this regard for each lat/time/altitude bin, we include the standard deviation of the measurements used (so a combination of geophysical variability and measurement noise) and the median measurement uncertainty. Generally aerosol extinction in the lower stratosphere has uncertainties less 10% and, during Pinatubo, often less than 5%. We are currently in the process of adding the same uncertainty parameters into the GloSSAC data set for OSIRIS and CALIPSO (and for new potential data sources SCIAMACHY and SAGE III/ISS). They will be included in the next

GloSSAC release planned for mid-2018. The process we used to scale data from OSIRIS, CLAES, HALOE and CALIPSO to the long-term 525 and 1020 nm data sets nominally eliminate bias from between the data sets and the spread of measurements is more or less the combined measurements uncertainty combined with geophysical variability. However, this is only the case where the data sets actually overlap. For CLAES/HALOE, their use in GloSSAC coincides with the massively volcanic Pinatubo period and their sole use is for altitudes/latitudes where SAGE II data does not exist. For CALIPSO/OSIRIS, their use is solely for the period after the SAGE II mission ends. It is also relevant to note that the overlap period between SAGE II and OSIRIS was fairly quiescent in a volcanic sense while much of the period after 2005 consists of a steady drum beat of smallish but significant volcanic events. There are few robust measurements for either the Pinatubo period (particularly in the Tropics) and the post-SAGE II period with which to evaluate how well the conversion works, the potential for significant bias is possible. The discussion on how small volcanic events manifest themselves differently than large ones is potentially relevant to this discussion. We hope with the new SAGE III mission having overlap with both OSIRIS and CALIPSO in this mildly volcanic period will give insights into the potential for bias as well as mechanisms for migrating any issues. This is a focus for new developments in GloSSAC. We have included a new paragraph in section 5 (Additional GloSSAC Components) discussing measurement uncertainties and plans to address them (better) in the future.

Regarding the color figure image quality. I freely admit that I have struggled a bit to make the figures fully readable. I have spent some time trying to improve this and think they are much better now. First, I changed all the contours to log10, this is shorter than listing them in scientific notation and allowed larger fonts to be used. In addition, I figured out how to save the postscript versions to a higher resolution PNG format than I used before that has significantly improved readability. I will also ask the editors to split figure 2 between two full pages so that are not as small as they are in the manuscript. Also, I found two errors in the figures which I have corrected. The color bar on figure 9 was mislabeled and has been repaired. I also had a different color scale between

figures 15a and 15b but only one bar. I made them the same so they are properly rendered. There is a noticeable qualitative 'color' change in 15a which is the result of fixing the scaling.

Originally, we intended to compare with the GISS optical depth record. Several of us have been involved with that data set and were aware that its behavior after 2000 was not representative of stratospheric variability. We decided to add the AVHRR to this set since we were well aware of the large difference between GloSSAC (and all previous versions of this data set) and AVHRR and didn't feel like we could avoid addressing it. AVHRR estimates total aerosol optical depth which is usually dominated by tropospheric aerosol and generally it is not possible to infer a stratospheric component for all but the most volcanic periods (El Chichon and Pinatubo) of the long record. The comparisons are pretty decent in the mid-latitudes suggesting even the inelegant approach for removing the tropospheric component is working fairly well. On the other hand there is clearly about a factor of 2 difference between AVHRR and GloSSAC optical depth during Pinatubo. The reviewers suggestion of using a temporal local background would help if we used the leading period where it would drop the AVHRR optical depth to about 0.3, a difference that could be mostly tropospheric in origin. On the other hand, the optical depth in the 1992-1994 period drops far below the levels reported prior to the eruption and could suggest an optical depth as high as 0.5. It is possible that there is a 'break' hidden within the Pinatubo recovery period but it is difficult to tell. It is also worth noting that this is a new version (as opposed to the Stowe/Long version) and it lacks a formal publication beyond a pair of technical reports. We have been unable to contact the responsible party to discuss the issues we see in the tropical data. It is hard us to feel confident that this is a high quality data set. For the purposes of the manuscript. We have added text AVHRR stratospheric optical depth for the periods from 1982 to 1983 (El Chichon) and 1991 to 1995 (Pinatubo). The change between CCMI and GloSSAC in the tropics is primarily due to the change in the filling process replacing subtropical lidar data (Mauna Loa and Camaguey) with tropical scaled CLAES data. There is an additional effect due to tightening the require-

ments for the use of SAGE II data from simply 5 data points to at least 50% of possible data points and at least 5 data points. This means that there is more CLAES fill than lidar-fill between CCMI and GloSSAC data sets.

I have added some material regarding the derived parameters. It is based on the process outlined in Thomason et al 2008 and is included mostly as guidance. The official CMP6 stratospheric aerosol product uses a process developed by Beiping Luo at ETH Zurich rather than this product. There is a short description early in the paper and a little more in Section 5 (Additional GloSSAC Components).

Specific comments P4, l32: "The data are": : : missing word(s) here? Fixed

P6, l6: "by ice clouds those": : : missing word(s)? Fixed

P6, l39: I didn't understand "the use of short duration events: : :" For a brief period following battery issues, SAGE II events were shortened in duration to where suffient exo-atmospheric data was not always collected and these events are generally not used for science analyses. Clarified in the text.

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? Have clarified that HALOE is not as well correlated as CLAES and this is why it has third priority in filling data gaps.

P7, l38: can the authors provide some estimate of the order of this error? Unfortunately there is no data with which to assess the range of possible errors.

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

P9, l4: as->at ? Fixed

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.

Replace 'sorted in' with 'correlated with'

P9, l15: "from MERRA"

Fixed

P9, l16: I think the references should be to Figure 10b and 10c. There were some errors in the text both prior to the equation and at the referenced line. These have been corrected.

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

Some of the 'filling' at low latitudes had not been done for this figure. We have corrected this in the updated figure.

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

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

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

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

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

We have clarified this sentence to: For GloSSAC, we use CALIOP version 4 level 1 data where, unlike earlier versions, the backscatter signal is calibrated at the higher altitude range.

P11, l33: represents Fixed

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

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.

This has been made (much) clearer with: Since clouds in the upper troposphere may have a deleterious impact on the measurement of aerosol extinction in the lower stratosphere (characteristic of limb measurements in general), we exclude all OSIRIS data in the lowest 2 km of the stratosphere.

P12, l26: It's not quite clear if the "method used for SAGE II" includes the equivalent latitude method or not. Just the interpolation process as: Some interpolation at mid and high latitudes is required and we follow the interpolation method used for SAGE II observations to fill these gaps.

P12, l28: something about this sentence is a little wonky. Clarified to: Where bins remain unfilled, the lowest measured value in a lat/time column is replicated down to the tropopause. This is rare and rarely for more than 1 or 2 altitude bins.

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?

Assuming the usual particle size range and sulfate aerosol the conversion from backscatter (532 nm) to extinction (525 nm) can be between 30 and 60 str-1 but has a peak near 0.3 microns. The plot has a large number of points and ultimately, I didn't trust the tail (which has many fewer data points than the main blob) sufficiently to use a variable conversion factor. This is another place where we are looking at improvements particularly trying to figure out a robust way to infer something about particle size so that the backscatter to extinction conversion and the 525 to 1020 nm extinction conversion can be improved. I have reworked the end of the relevant paragraph to: I f

the conversion suggested by distribution shown in Fig. 12b were used, large extinction coefficients would tend to increase while smaller extinction coefficient values could be as much as a factor of two smaller. If the relationship is found to be robust then it suggest that some aerosol size information is possible which may improve estimates of extinction at other wavelengths (particularly 1020 nm) and inferences of aerosol size distribution. As a result, it is clear that further study on the conversion of CALIPSO backscatter to extinction coefficient is worth further study and improvements to this part of the GloSSAC product will be included in future versions.

P13, l13: "should be retained" Fixed

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.

We have reworked that section to make the distinction between modeled ratio and measured ratio clearer.

P13, l38: "able to leverage" Fixed

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