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Atmospheric Chemistry and Physics Discussions

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7, S6738–S6747, 2007

Interactive Comment

Interactive comment on "SAGE II measurements of stratospheric aerosol properties atnon-volcanic levels" *by* L. W. Thomason et al.

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The authors would like to thank the reviewers for their efforts on this manuscript. We apologize for the delay in returning the revised manuscript. The first author was ill for several months and has only recently been available.

Reviewer 1.

Major concerns

1. Establishment of the errors present in these two parameters is a useful exercise, but it is rarely put into any context. It is clear that, for example, dark current is proportionally greater in SAGE II measurements lacking aerosols than in measurements with high aerosol loading, but at no time is it established exactly what percent difference might be expected as a result of dark current with high aerosol loading. If this percent

difference is previously established, it must be mentioned and cited; if not, it should be calculated in the same manner as the low-extinction measurements to give a basis for comparison. This should be done for each of the aerosol extinction experiments, as well as the SAD calculations. Without that information, the reader has no way of knowing exactly how compromised the SAGE II measurements are with low aerosol loading.

The purpose of this paper is to address issues in the production of the SAGE II aerosol extinction coefficient profiles at low loading. This effort was an outgrowth of the SPARC Assessment of Stratospheric Aerosols (ASAP). In this report, substantial differences between various estimates of aerosol extinction coefficient were found at low aerosol levels while agree at higher levels were substantially better. The SAGE II extinction profiles have been extensively validated in the past but mostly at higher aerosol levels. At high loading, aerosol uncertainties are very small and almost solely the outcome of measurement noise. However, at low loading some retrieval factors may provide an increased uncertainty in the retrievals that is not accounted for in the reported measurement uncertainties. This paper examines those. We have clarified this scope in the introduction.

2. I question the value of the variances in several of the aerosol extinction experiments. Specifically, the altitude registration, temperature profile, and ozone cross-section sensitivities include perturbations that are too low given the uncertainties in those parameters; thus, the final calculated uncertainty is too low. This is especially apparent considering that the authors explicitly state that a “maximum bias” or “potential for bias” is being sought. These three parameters also cause the greatest uncertainties in the aerosol extinction, so a proper perturbation value is vital. Clearly, any underestimation in the error characteristics in this section are further carried into the calculation of SAD. Some more detail about my concerns regarding each of the parameters follows:

In retrospect, we somewhat overstated the conclusions in this section. We were trying

ACPD

7, S6738–S6747, 2007

Interactive Comment

EGU

to get a handhold on the magnitude of the bias potential for reasonable uncertainties of key retrieval parameters. We weren’t really trying to maximize the errors in this section so much as understand the magnitude of the uncertainties from various components. The errors are fairly linear with magnitude so that doubling the error in the parameter more or less doubles the effect on aerosol and changing the sign of the error gives a similar but opposite signed error. We have clarified the goal of this section in the text.

a.) Altitude registration. The authors state that their choice of 100 m perturbations are comparable to the altitude registration uncertainties derived in Wang et al. 2002. I was unable to find that uncertainty explicitly stated in Wang et al. 2002, but that reference did include a separate citation for altitude registration uncertainty in which an estimated error of 200 m is provided (Cunnold et al., JGR, 94(D6), 8447, 1989). Thus, the authors’ choice of 100 m is only half of the previously established uncertainty.

We have updated the reference to Borchi and Pommereau for the use of 100 to 200 m altitude registration accuracy.

b.) Temperature profile. The authors introduce a bias of +3 K in two separate experiments, first at 100 mb, then later at 10 mb. The citation used (Randel et al. 2000) indicates a bias of 3-5 K; thus, the choice of 3 K as the bias is puzzling, since the maximum SAGE II measurement bias would not result from the minimum cited temperature bias. Additionally, the only citation in this section (the aforementioned Randel et al. 2000) only discusses tropopause temperatures; as the authors noted elsewhere in the text, temperatures at higher altitudes are likely more relevant to stratospheric aerosol measurements, so a more comprehensive review of NCEP temperature uncertainties seems necessary (see, for example, Pawson and Fiorino, Climate Dynamics 14, 631, 1998; and Stendel et al., Climate Dynamics 16, 587, 2000). Relatedly, while it is somewhat unclear in the text, it appears as though the temperatures at levels surrounding 100 mb (or 10 mb) are unchanged, leading to some discontinuity in the temperature

ACPD

7, S6738–S6747, 2007

profile. This is an unrealistic scenario, as it would be more likely that a bias would be found along a range of pressures, rather than at one discrete pressure.

We have updated the reference to Randel et al. 2004 which is based on the SPARC Middle Atmosphere Climatologies Assessment. Based on this paper, the 3 K perturbations are reasonable. The tropopause temperatures are the most relevant because of the feature observed in the SAGE II topical tropopause data; we were curious if the temperature uncertainties there could create the observed annual cycle. SAGE II receives temperature profiles from NCEP at fixed mandatory levels (100 mb, etc.).

The temperature at one level is adjusted and the 0.5-km temperature profile used in processing is computed using the adjusted value (thus spreading it over a range of altitudes). We have clarified this in the text.

c.) Sensitivity to ozone cross-section. The paper states that sources report uncertainties in the range of 1-2%, with an additional 5% temperature sensitivity near 525 nm, and yet a perturbation of only 1% was used for the 525-nm channel. A good justification at minimum is required for the low value, especially considering the high sensitivity of the retrieved aerosol extinction to the ozone cross-section.

The 5% temperature sensitivity is the amount it changes over typical stratospheric temperatures and is not a uncertainty. Since this is somewhat confusing and irrelevant, we removed this reference. Again, we used the 1% value because it lies within the expected bounds of the cross section uncertainty and gives the scale of aerosol uncertainty we desired. As with the other parameters, this error scales fairly with magnitude (double the error, double the effect).

3. On p. 6962, lines 14-17, several of the factors that may influence aerosol extinction measurements are listed, but not all are addressed in the following section. For example, measurement noise, cloud clearance, and nitrogen dioxide cross-section are not brought up in Section 2. Any omissions from Section 2 should be justified or included.

ACPD

7, S6738–S6747, 2007

Interactive Comment

The effects of measurement noise and nitrogen dioxide cross-section were extremely small and not included in the manuscript. We have indicated this in the text. Cloud clearing is more of an interpretation issue as opposed to a data quality issue and should not have been included in this list. We have removed it.

4. On p. 6974, line 2, a value of N_total=20 cm-3 is established. Since the record of volcanic quiescence lasted from 2000 to the end of SAGE II operation, and the chosen sample measurement shown in Fig. 4 is from 2003, the one year quoted to halve 10 cm-3 is not necessarily that long a term. In fact, according to Fig. 3 of Deshler et al. 2003, aerosol number density is highly variable, is dependent on size, and is almost always below 10 cm-3, particularly in the stratosphere. The text in that reference also states uncertainties up to +/- 80%. Additional justification of the chosen value of N_total=20 cm-3 is required, along with sensitivity studies addressing this value beyond the lone mention on p. 6977, especially since this value is used in both methods for SAD determination.

The value of 20 cm-3 was chosen to be above the nominal upper end of stratospheric aerosol number density of 10 cm-3. This value, given that increasing N_total monotonically increases the SAD estimate, should provide an upper limit on SAD except in areas where active nucleation is occurring. Nominally, as is noted in the manuscript, the upper limit of SAD is infinity if the number density is allowed to be unbounded. We have added some text on a sensitivity we performed based on varying the base number density and that the model of a two unimodal distributions does produce maximum SAD. We have also clarified that this is a stratospheric application where 10 cm-3 is a nominal value.

Relatively minor concerns

p. 6962, lines 6-7; the paper mentions that 80% of the cycle is related to different amounts of sulfuric acid entering the stratosphere. No suggestion of a possible source is provided, but one would be appreciated.

ACPD

7, S6738–S6747, 2007

Interactive Comment

We don’t know where it originates and it also could be organics or other compositions. We are looking into this and believe it is of tropospheric origin. If it is sulfuric acid, it is likely originates as SO2 from a combination of natural and human-derived sources in the troposphere. We have noted this in the text.

p. 6964, lines 13-16; a single SAGE II event is described and shown in a figure. Why was this particular event chosen? Is it a “typical” event? If so, that should be mentioned in the text.

It is typical and now noted as such in the text. It is a ‘standard’ image we have used for several years to demonstrate what the data stream looks like on an altitude scale.

p. 6975, lines 17-18. Why is this SAGE II measurement in particular chosen? Is it simply a “typical” measurement? If so, why is a different typical measurement chosen for this figure than for Fig. 4?

This is also a typical measurement. It is an event that we used in comparisons with another instrument on an unrelated topic. There was no particular reason that they were chosen and, to our minds, no particular reason for them to be the same.

p. 6978, first paragraph. The text repeatedly warns about cautious interpretation of the results from this experiment, and yet does not indicate why, or what potential pitfalls there are in doing so. Please expand this paragraph to include such a discussion.

The caution has been clarified.

p. 6979, lines 7-8. This is the sole mention of results obtained during periods of higher aerosol loading. Is the SAD determination calculation is still viable for these earlier periods of SAGE II measurements? If so, why limit the scope of the paper to post-2000 measurements? A technique for placing upper and lower bounds on the SAD calculations in high aerosol loading situations would prove useful, and apparently has not been done previously.

We have removed this statement since it distracts from the main thrust of the paper. This methodology can be applied to high aerosol loading except, as is mentioned in the text, at the highest loading where the solution for the solution for the large mode becomes non-unique (since the extinction ratio approaches one and multiple solutions for particle size exist).

Technical Corrections: p. 6960, lines 9-11; this is an incomplete sentence.

Repaired.

p. 6960, lines 19-20; the word “eruptions” is repeated. I suggest removing the first instance.

Done.

p. 6965, line 11; the word “all” is repeated. I suggest removing the second instance.

Done.

p. 6965, line 16; how is “confidence” defined in this instance?

Clarified in the text.

p. 6967, line 5; the units of hPa are used, whereas mb is used elsewhere in the text. Recognizing that the units are identical, the same terms should be used for consistency’s sake.

Done.

p. 6967, line 27; a reference should be provided for the mirror correction uncertainty.

There isn’t a reference available for this parameter. There are a only couple of conference proceedings and internal documentation available at this time.

p. 6968, line 12; the word “extinction” is repeated. One should be removed.

ACPD

7, S6738–S6747, 2007

Done.

p. 6968, line 15; a reference should be provided for the estimated accuracy of the aerosol model.

There isn’t a reference available for this parameter. There are only a couple of conference proceedings and internal documentation available at this time.

p. 6969, line 15; the word “that” should be deleted.

Done.

p. 6971, lines 11-15; this is a run-on sentence that reads very poorly.

Fixed.

pp. 6972, 6974, and 6978. Since Section 3 is called “Surface area density estimation sensitivity”, the headings “Method 1”, “Method 2”, and “Summary of aerosol surface area density” would all fall under that Section. Thus, sections 4, 5, and 6 should be renamed as 3.2, 3.3, and 3.4.

Repaired.

p. 6972, line 23; Fig. 11a should be changed to Fig. 10a.

Repaired

p. 6976, line 15. Please change “For the minimum solution the solution uniformly...” to “The minimum solution uniformly...”

Done.

p. 6984, Fig. 3. The labeling of the curves is somewhat awkward. It may be more visually appealing to begin the curves at 1985 and use a traditional legend to indicate the channels (with a variation of the color for the 5453 or 7386 channel, since they are similar). In addition, the format for curve labeling is unclear. For example, “4, 525 nm” appears at first glance to be a 4525-nm channel. I would suggest a

ACPD

7, S6738–S6747, 2007

format along the lines of “Channel 4 (525 nm).”

We cleaned up the labeling in this figure.

p. 6991, Fig. 10. The dotted line in panel (b) is not described in the caption or in the main body of the text. In addition, I suggest changing “...525-nm channel to the 1020-nm extinction...” in the caption to “...525-nm to 1020-nm extinction....”

Repaired.

pp. 6993 and 6994, Figs. 12 and 13. These two figures can be combined, since most of the information in Fig. 12 is repeated in Fig. 13. Also, in the main body of the text the SAD calculations using the Method 1 452-nm channel and Method 2 two channel technique are immediately discarded due to their similarities to the 525-nm channel and three-channel technique, respectively. Thus, I suggest one figure, with the operational SAGE II SAD, the Method 1 525-nm channel results, and the Method 2 three-channel results. The fact that the other techniques were tested and found to be similar to the ones on the plot can be mentioned in the text, but need not appear on a figure.

We have eliminated the extra figure and simplified the text.

pp. 6996 and 6997, Figs. 15 and 16. Both of these figures can be deleted. Much of the information is redundant from Figs. 12/13. Moreover, the text warns about the need for very cautious interpretation of these results; in light of this, two plots illustrating those results do not seem necessary.

We have eliminated figure 16 but we have retained Figure 15. We believe that the mixed visible and infrared extinction measurement approach simulated in this figure and accompanying text is relevant to planning future measurements of stratospheric aerosol. We have strengthened the discussion of the figure.

ACPD

7, S6738–S6747, 2007

Interactive Comment

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 6959, 2007.