

Interactive comment on “Stratospheric aerosol measurements by dual polarisation lidar” by G. Vaughan and D. P. Wareing

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1) In the algorithm description (chapter 3), equation 1 should also be written for the special case with both polarisers set to parallel for the ease of the understanding. This has been done. The text in section 3 has been modified to accommodate this and explain the derivation of the calibration equation a little more clearly.

2) At the end of chapter 3, there is only a short statement that the value of "y was found to be 0.4±0.1%": a) the method, of how this value was determined must be described more elaborate. Was this a one time measurement under optimal conditions or was this determined from tests done every night? How exactly was the value measured, which was used as reference height etc. The last paragraph of 3 has been modified as follows: “Several estimates were made of y over the three years of measurements

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using equation (8), with improved accuracy as system modifications were made. All the estimates were consistent with a value of $0.4 \pm 0.1\%$. The definitive estimate was obtained on March 7 2004 with a narrow-band filter passing only the Cabannes (elastic) backscatter plus 7% rotational Raman, giving a value of $\delta = 0.385\%$. This makes $S_{||}/S_{\text{perp}}$ in (8) more sensitive to the value of y and enables it to be measured to better precision (although the aerosol measurements themselves, of course, are more precise with the greater S_{perp} of the wider filter). Taking an aerosol-free height on that day as 9.45 km, y was estimated as $0.4 \pm 0.04\%$. This value has been adopted for the results presented in this paper."

b) Is this the value y or rather the value x , defined as the instrumental depolarisation? In the conclusions, it is stated that "the system depolarisation was determined to be around $0.4 \pm 0.04\%$ " Why is the error smaller now? The value is derived using (8) so it is y . We hope the new paragraph satisfies this point too.

c) It would also be interesting to know the pure laser depolarisation, in order to relate this value to contributions from the detection system and from the laser itself. We have not been able to measure this - we lack the equipment to do so.

3) To this reviewer it appears that figure 2 (data of Dec. 11/12 2001) is not necessary to be presented and the data also do not need to be discussed in chapter 4.1. The later data are better in quality and for the presentation of the method. (The authors had S2497 already chosen not to mention Fig. 2 in the text anyway.) This figure is included for comparison with the later data to give the reader an idea of the variability in background aerosol, which is quite considerable. This point is now made in the paper (second paragraph of sect.4).

4) At the introduction, when discussing the Raman method, it would be appropriate to cite a relevant source for this method. Two sources are now referred to: the Leipzig group and the NASA group.

5) Two technical remarks: Chapter 4.2, first paragraph, last sentence says "February

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2004", this should read Feb. 2003 ? No, it's correct. We didn't have measurements in Feb 2003. Chapter 4.3 should make reference to Fig. 7 after the second sentence. Referee was using the original version of the manuscript. This was corrected in the version published on ACPD.

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