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Interactive comment on “Toward a combined SAGE II-HALOE aerosol climatology: an evaluation of HALOE version 19 stratospheric aerosol extinction coefficient observations” by L. W. Thomason

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1 Differences are essentially the same at all latitudes. I have added information indicating this in the manuscript.

2 Added ‘after 1996’ to line 8 in abstract.

3 The ‘good match’ criterion isn’t used in a rigorous manner and is really an empirical assessment of how well the data follows the predicted behavior. In this case, I counted as a ‘good’ match anything that was within 20 or 30% of the bounds defined by the

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model. In figure 5, this would mean virtually all the data in frames (b) and (c) are good while much of the data in frames (a) and (d) are poor matches. I have clarified this at the end of paragraph 1 of section 3.

4 I like having the complete time history shown in figures 5 and 6 but to clarify the 1996/1997 differences, I have replotted the data such that the data prior to 1997 is now blue. I hope this is adequate.

5 I also suspected that a bad index of refraction may be influencing my interpretation of the 2.45 micron aerosol data. However, a number of measurements of the index of refraction at or near this wavelength indicate that the value is well known and not significantly in error. While I do not know the details of how these HALOE extinction values are derived, it appears that the 2.45 micron channel has a significant problem that erroneously forces it to look like 5.26 micron aerosol extinction.

6 These plots are based on a log-normal with a fixed width of 1.6 where the difference in extinction is computed using the ratio of 525 to 1020 nm to predict what the ratio should be between a HALOE measurement and the SAGE II 1020 nm value. The difference in ratio is then converted into a difference in extinction value and averaged as a function of altitude. I have clarified this in the text.

7 In the early HALOE measurement period, the NO₂ influence was small. Later, (according to E. Remsberg) when it became noteworthy, fixing the NO₂ artifact was not considered a high priority and was thus never included in the processing algorithms.

8 The use of a single mode log-normal does restrict the ratio space somewhat and is almost certainly more of an issue at lower altitudes and is part of the reason I find matches that lie near but not within the model bounds acceptable. However, it is unlikely that any combination of log-normals will significantly expand the space defined by the single mode models. It is possible that changes in composition in the UTLS (e.g., ice or organics) could significantly impact the spectral characteristics and I have noted this in the manuscript.

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9 Done (fixed table references)

10 Done (added missing 'deviation')

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 12, 13933, 2012.

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