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Interactive Comment

Interactive comment on "Intercomparison of stratospheric ozone and temperature measurements at the Observatoire de Haute Provence during the OTOIC NDSC validation campaign from 1–18 July 1997" *by* G. O. Braathen et al.

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Major Comment

The authors are to be congratulated on an excellent set of lidar intercomparison measurements and a thorough description and analysis. This is a very important topic, because ozone and temperature are fundamental measurements for atmospheric chemistry and physics, because remote techniques offer the best chance for comprehensive measurements of trends, and because lidars have much better vertical resolution than other remote techniques.

However, by calculating the standard deviations of differences between means (Figures 4, 5, 6 and 8), the deviations are dominated by atmospheric variability rather than instrumental differences. This is particularly so below 20 km, as acknowledged later in the text in Section 3.3. If the authors calculated the standard deviations of the means of the differences, rather than differences between means, this would be avoided. The differences of means (black lines in Figure 4) are unaffected by the order of the calculation (they commute), but this is not so for the standard deviations (green lines in Figure 4) which would then be smaller. Some of the differences below 20 km may then turn out to be significant, rather than the apparent lack of significance in the current Figures. More of the differences at higher altitudes may also be significant, because although the atmospheric variability is less at higher altitudes it may still be significant.

It would be easy and quick for the authors to repeat the calculation of standard deviations in this improved way, as all the data must still be accessible (the lead author's institute maintains the data base), and there is no need for any new measurements.

Minor comments

1. The statement in paragraph 4 of the Introduction, that ozone is the most important component for trend studies, may soon be untrue and may even now be out of date. Stratospheric temperatures may change so much in an enhanced-greenhouse world that they assume a much greater importance than today. Already the global average is over 2 K colder than in 1980, for reasons that clearly cannot be explained by well-mixed greenhouse gases alone because of the negligible change between 1960 and 1980.

2. The description of the GSFC system, particularly at the end of Section 2.1.1 and the beginning of Section 2.1.2, are written in a laudably simple style. But if you want the educated non-specialist to understand, you must say that a chopper eliminates the large signal returned form the closer atmosphere in order to prevent saturation of the detector, etc. The description of the CNRS does indeed contain such details but comes

later. I presume these sections are as supplied by the GSFC and CNRS teams, if so perhaps the independent lead author should take executive decisions about their style and flow.

3. In the description of the CNRS algorithm in Section 2.3.2, surely adjusting the scale height is equivalent to a temperature fit over a height range of about 6 km? If so, the educated non-specialist would welcome this less obscure description.

4. Again for the non-specialist reader, you should explain why non-linearity occurs in a photon counting system, in paragraph 3 of Section 2.3.2 - namely that the larger light intensity, when using counting gates of finite duration, the greater the chance of two adjacent photons being counted as one.

5. There are obvious differences of vertical resolution below 20 km in the comparison between ozone lidars in Figure 2, not surprising given their calculated vertical resolutions in Figure 1. It would be easier for the reader to assess their significance if the results were also plotted with CNRS output smoothed to the poorer resolution of the GSFC lidar. It would also greatly improve the intercomparison exercise in Figure 4 if this were done for all the measurements before taking differences. Because of the problem identified in the major comment above, this improvement had no doubt seemed unnecessary, but if the standard deviations are recalculated as suggested, this improvement might give significant reduction in differences.

6. There is no discussion of the vertical resolution and vertical shift of ozonesondes in Section 2.4. These effects arise because of the time constant of the ECC sonde (about 1 minute if 2.5 mL of solution is used, up to twice this if 5 mL is used), which smooths and lags the data, by up to 300 m depending on balloon ascent rate. This is rather smaller vertical resolution than the lidars, so that a similar comment to 5 above then applies - the sonde measurements should be smoothed to the vertical resolution of the lidar. Sonde measurements should also be shifted downwards before taking differences, unless lag compensation is included automatically in the ozonesonde software.

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7. In Section 3.3, the formula given in Equation 1 is for the standard error of the mean, not the standard deviation (Topping 1972 p62, OUP 1985 p251, Barlow 1989 p53). The authors are not alone in this mistake, it also appears in the terminology and definitions of the Excel spreadsheet. The standard deviation describes the dispersion or scatter of the points from the mean, and equals (S(yi - ў)2/N)¡). It is clearly this scatter of the points about the mean that should equal the mean instrument error on one night of observations given in Equation 2. The excellent agreement between green and red lines above 20 km in Figures 9 and 10 shows that the correct ratio between the formulae have been used, not the Equations 1 and 2 in the text. However it is unclear whether the values in Figures 9 and 10 are standard deviations or standard errors, and so which of the two Equations has the incorrect factor √N compared to the formulae used.

8. The argument in Section 3.3 that above 20 km the ozone concentration does not vary much from day to day is circular - with errors this size, how do you know? In fact this uncertainty could be bypassed if you looked at differences between shorter periods in one night - not only must the changes in ozone be smaller during the course of a night than between nights, but the calculated error must be larger, allowing the scatter in the measurements to be dominated by instrument error. This would provide a better confirmation of the error calculation in Figures 9 and 10, and to lower altitude.

Technical Comments

1. Title - should surely include “lidar”?

2. Title - should surely exclude “OTOIC”, and perhaps the days, the month, and the location?

3. Abstract - reads more like a brief introduction, there are no results

4. Sec 1 lastpara - expands GSFC despite the acronym being in the abstract

5. Sec 2.2.2 - replace “derivation” by “derivative”

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6. Sec 2.3.1 para2 - delete “a”

7. Sec 2.3.2 - introduce “MSIS”

8. Sec 2.3.2 end para1 - “above 100 km, where the backscattered signal I small..” would be clearer

9. Sec 2.3.2 para2 - by “the selection of the raw data”, do you mean “when to reject the data”?

10. Sec 2.4 - expand ECC, ENSCI, PTU, JOSIE, KFA

11. Sec 3.1 - describes Fig2 as July 8 when it is 6 July (wrong style for EU journal, wrong date)

12. Fig 4 - show the red through the green, as just about done in Fig 5 for blue through red

13. Figs 4 & 5 - green lines are too faint, not legible on paper copies

14. Sec 3.2 - gets the date of Fig7 wrong

References

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OUP, “Concise dictionary of physics”, Oxford Science Publications (ed. A. Isaacs), OUP, Oxford, ISBN 0 19 866142 8 (1985).

Topping, J., "Errors of observation and their treatment", 4th Edition, Chapman and Hall, London, ISBN 0 412 21040 1 (1972).

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 5303, 2004.

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