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Interactive comment on "Ozone observations by the Gas and Aerosol Measurement Sensor during SOLVE II" *by* M. C. Pitts et al.

Anonymous Referee #3

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This is a very interesting paper that contains new measurements of O3 in the Chappuis and Wulf bands obtained using the "Gas and Aerosol Measurement Sensor" (GAMS) that was operated onboard a DC-8 aircraft in early 2003. The authors use this data to assess the accuracy of currently available O3 cross sections, namely those of Shettle and Anderson (1995) and those from SCIAMACHY (Bogumil et al., 2003)

The experimental and analysis procedures are well described, and the discussion and conclusions are clear.

I have only a few minor comments.

1. The authors conclude that there might be still errors as large as 5% near the Wulf

band absorption peaks. Is this in agreement with the uncertainties stated by the authors of the SCIAMACHY cross sections? Is it really clear that there is no instrumental effect from GAMS? I believe that GAMS is exceptionally stable, but how was radiometric calibration achieved and validated? How linear is the CCD and the data acquisition chain? How was the dark current obtained and corrected for (or was it negligibly small)?

2. There is a paper by Enami et al. (JGR 109, doi: 10.1029/2003JD004097, 2004) on T-dependent O3 cross sections in the 760 nm region. The authors should include this paper and its results in the discussion.

3. On page 9962 the authors write that the "uncertainty in the Chappuis ozone cross sections is on the order of a few percent". Is this consistent with Orphal's review (J. Photochem. Photobiol. A 157, 2003)? There is also another paper by Borchi et al. on O3 validation in the VIS (ACPD 4, 4945, 2004) that might be interesting in this context.

4. Figs 14-17: it is surprising that the "inferred errors" have such a similar shape for the Shettle and Anderson (1995) and SCIAMACHY (Bogumil 2003) data. I would not expect that for two independent sets of data recorded each 10 years apart. I would much more think that this is an effect in the GAMS data. How can the authors rule out such a conclusion (a short discussion would be very helpful)?

5. A very recent paper by El Helou et al. (J. Chem. Phys. 122, 244311, 2005)proposes a rather significant decrease of the O3 cross sections with temperature over the entire Chappuis and Wulf bands. Is this hypothesis in agreement with the GAMS data and analysis?

6. H2O is an important absorber in the near infrared. How did the authors correct for the H2O absorption for analysing the GAMS data (H2O profiles, convolution, ...)? This should be a difficult procedure?

7. There should be a Ref. for the data shown in Fig. 1.

8. It would be nice to have a scale in Fig. 2 to get an idea how big the instruments

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really are.

In conclusion, the paper deals with an important question that is not only crucial for SAGE-III but also for other atmospheric experiments.

I recommend it for publication after taking into account my comments above.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 9953, 2005.