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Interactive comment on "Balloon-borne stratospheric BrO measurements: comparison with Envisat/SCIAMACHY BrO limb profiles" *by* M. Dorf et al.

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General Comments:

This paper does a nice job of assembling available stratospheric balloon measurements for evaluation of SCIAMACHY BrO profile measurement. It provides a comprehensive discussion of the various BrO measurement techniques, uncertainties, and available balloon flights. The paper uses a trajectory hunting method to connect the balloon flights to near-collocated SCIAMACHY profiles and a photochemical model to scale the profiles to a common solar zenith angle reference. The authors appear to have taken care to represent the balloon observations consistently with the way the samples were obtained, e.g., lines of site, etc., and have done a lot of work to extract meaningful comparisons with the satellite data. Several example comparisons between balloon data, a global photochemical model, and SCIAMACHY limb profile retrievals are presented and discussed.

Global measurement of stratospheric BrO profiles is a key element in understanding halogen amounts, budgets, trends, and their impacts on O3. It is critical that SCIAMACHY BrO data be carefully and quantitatively validated. This is not a simple or monolithic task. This paper presents the correlative balloon data, a useful set of comparison points and methodology, and comparisons with data from a particular retrieval of the SCIAMACHY radiances. The approach is not unique, and perhaps the conclusions could be improved as discussed below, but overall it forms a useful part of the ongoing scientific dialogue on this topic appropriate for ACP.

Specific Comments:

The paper shies away from several issues that should be confronted, at least in discussion. The first is the relation of this work to that of Sinnhuber et al., GRL, 32, L20810, 2005 and Rozanov et al., Adv. Space Res., 36, 2005, who come to quantitatively different conclusions about the SCIAMACHY BrO abundances compared to those apparent here (see below) using the same radiances but a different retrieval. Rozanov et al. find SCIAMACHY BrO mixing ratios to be 2-5 pptv higher than in situ balloon data in one case and nearly identical (+/- 3) in another. Sinnhuber et al., using the Rozanov retrieval, find SCIAMACHY BrO consistent with a photochemical model from 15-28 km using total Bry of 18 pptv. If anything, the model tends to overestimate the SCIAMACHY data. Zonal mean BrO never exceeds 14 pptv in Sept, 2002. In contrast, the results presented here (Figures 3-7), using the Harvard Smithsonian retrieval (publication in review), show consistently higher mixing ratios and rather different profile shape for SCIAMACHY. This paper does not necessarily have to conclude which retrieval approach is better, but the differences should be acknowledged and discussed.

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In a similar vein, one could look at the model profiles presented in Figs. 3-7 and estimate that if Bry were increased another 4 pptv to about 24 or 25 pptv, as suggested by Salawitch et al. [2005], the model would agree with the SCIAMACHY data pretty well at least below 25 km. This would present a problem between the model and balloon data in most cases, but I don't think we are ready to rule out anything at this point. Again, the paper does not need to conclude who is most likely right here, but the discrepancies should be recognized.

Finally, the analysis must become more quantitative. The text is rife with statements like "similar good agreement", "correspondenceĚ is very convincing", "coincide well", and "agreementĚ is warranted." Most of these can be simply transformed into quantitative statements such as "agreement within xx pptv or yy%." These numbers can be related to error estimates as needed. Qualitative statements should be reserved for remarkable events. Looking at the comparisons in this way, one must conclude that the SCIAMACHY BrO profiles using the Harvard-Smithsonian retrieval are systematically high compared to the correlative data and model. There is also an apparent difference in the profile shape above about 25 km. Say it in the conclusions and abstract. I do not agree that the comparisons are worse below 20 km (Abstract, P 13036, and P 13037). Absolute difference there is less, always within error bars, and relative agreement is similar. Statements in text should be revised. What would be most useful is a summary table of quantitative comparison between best-estimate correlative data and SCIAMACHY at various altitudes for each flight in parallel to Table 1.

On a point of clarification, it is not clear exactly how the 1-D/trajectory modeling works. I assume that 1-D refers to a vertical 1-D (column) model, rather than 1-D along the trajectory (P 13025, line 23). Then, is the entire column initialized from the 3-D and/or constrained by balloon-measured NO2 and O3 carried along the trajectory at each potential temperature level? It is not clear why the model should be run at fixed pressure and temperature for each theta level (P13026, line 13) or single SZA for all trajectories (line 18) rather than following the SZA, T and P of each trajectory. It is also not clear why a final SZA scaling (P13027, line 1-3) is needed if the trajectory endpoints are tied to the actual measurement times, unless this is to compensate for the allowable space/time mismatch. This methodology should be made clear.

Technical Corrections:

Abstract, line 2: Harvard ER-2 instrument is also a BrO profiling instrument. Re-word. Abstract, line 24-25: Re-evaluate statement about altitude dependence of agreement as discussed above. P 13014, line 27: skylight should be sunlight if this is referring to GOME. P 13015, line 26: delete 'both'. P 13016, line 10: delete 'all'. P 13016, line 11, 12: delete 'tried and tested'. All models are tried and tested. P 13016, line 21: delete 'regularly'. Section 2.1: This instrument section is highly detailed and much of the detail is available in the cited references. To avoid losing the focus of the reader, I would suggest that the authors pare this down to the material that is essential to assess the data comparisons (uncertainties, sample resolution, etc.) and leave details of the instrument modules, wavelength transitions, and oversampling to the literature. P 13018, line 7: Is this the instrument of Woyke et al., JGR, 104 (D15): 18755-18768, 1999? If so, refer to it. P 13020, line 12: Insert "on" after "km." P 13026, line 16: Rephrase sentence referring to "true evolutionĚ" It's not true even if mean trajectories were perfect, which they are not. Section 3: Best match is chosen here, but there is no reason to restrict profile analysis to one match. Can other matches be used, perhaps further along the trajectories, to fill in the profile comparisons? Section 4: Seems like this section would fit better in the modeling description prior to presentation of the observations. Remove redundancy with prior and succeeding sections on flight details. P 13034, line 4: delete 'much'. Figure 1: Put labels on grid or give scale (lat/lon?) in caption. Explain match with more or less than 50% of trajectories. Is this in the horizontal at a particular theta or among the levels? This is not discussed in the text, so is it needed in the figure? Figure 2: This would be more accessible if the time axis were in a consistent direction, i.e., reverse x axis range in left half of figure. Figure 3: Where is SCIAMACHY data for backward trajectory? This is directly comparable to

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Rozanov et al. [2005] Fig. 3. If no trajectory match can be found, then drop the top two panels and explain. Figures 3-7: Don't need DOAS total Bry lines on each plot - remove to reduce clutter. Use open circles for SCIAMACHY data points at altitudes where no match is found.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 13011, 2005.