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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Non-coincident inter-instrument comparisons of ozone measurementsusing quasi-conservative coordinates" *by* L. R. Lait et al.

Anonymous Referee #2

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Review of ACP manuscript

Lait et al., "Non-coincident inter-instrument comparisons of ozone measurements using quasi-conservative coordinates"

General:

The paper presents a comparison of ozone as measured by 4 different instruments/techniques during the SOLVE-2/VINTERSOL campaign. Rather than using standard time/space coincidences a quasi-conservative coordinate method is used to compare non-coincident data. The primary advantage of using such a technique is that it provides many more comparisons, and thus potentially better statistics, in the case

of sparse or non-overlapping data sets.

The paper is generally well written, although quite brief. The PV- technique is described very briefly, but references are provided to previous papers by the same authors which describe it in sufficient detail. One thing that is missing is any quantitative of prediction of the errors inherent in the technique. The authors are straightforward in pointing out these uncertainties - errors in the meteorological fields, limited sampling in PV-space, possible breakdown in the basic assumption of PV-ozone correlations, etc - but make no attempt to quantify these errors and their effect on the derived instrument O3 differences. Instead, cumulative errors are determined more or less empirically by a number of self-consistency tests (self-comparisons of each data set, and comparison of near-coincident with non-coincident data).

Overall, however, the paper is of general interest, particularly for an issue devoted to SOLVE-2/VINTERSOL, and the conclusions appear sound. I recommend its publication in ACP provided the authors address the minor points raised below.

Specific technical comments:

1. This is not a criticism per se, but I am wondering why the study did not include SAGE III data. This would add another satellite data set comparable in accuracy and resolution to the POAM data set. Also, since the primary objective of the SOLVE II mission was SAGE III validation, including the data in this study would make a contribution to that effort.

2. I don't understand the motivation for assigning measurement errors in this way. Both the lidars and the sondes provide error estimates for their data based on instrument-specific error analysis. By simply looking at variances in the retrievals over finite horizontal or vertical regions, you are just as likely to quantify real geophysical variability as retrieval precision. In particular, the method used to quantify the sonde errors seems crude since sondes have very high vertical resolution and can easily capture significant (and real) vertical structure over length scales of 5 km. I would suggest that the analy-

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sis incorporate either error bars archived with these data sets or error bars suggested from the individual instrument teams.

3. Section 4 (after #4389), 1st paragraph. How much data was used for the self-comparison tests? For example, for the AROTAL example shown in Figure 1 (and for DIAL), was the comparison done for one DC-8 flight, or is this the grand average of all flights? Similarly, for POAM and the sondes did you look at the average difference for the entire time period? It would be useful to the reader to make this clear in the text. Presumably, using the 400 km separation criteria significantly reduces the number of lidar profiles used in the analysis - on average, how many profiles per flight are used? Related point: what is the purpose of showing the average AROTAL O3 profile in the right panel of Figure 1? Does this add anything, or are any conclusions drawn from this plot?

4. For the DIAL/AROTAL near-coincident comparisons (Fig 2) why was only one coincidence per flight used? Presumably even when using the 400 km separation criteria there would be several close coincidences per flight. This would give better statistics for what is really the baseline comparison case.

5. For all the different comparison scenarios considered, it would be useful to show the relative O3 differences (in %) in addition to the absolute difference shown currently. While the absolute ppmv difference might be more meaningful physically, most validation studies show relative error too, and it's useful to know that error for comparison to other studies.

6. Figure 3 - the caption refers to a left and right panel (mean difference and profile) but there is only one panel in the manuscript. As mentioned in comment # 2 above, I don't really think the average profile plot would add anything to the results anyway. Also, can the authors comment on why the altitude range is different for the two cases? The "reconstructed-measured" difference extends several km lower than the "measured-reconstructed".

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Interactive Comment

7. If one takes the sonde measurements as a transfer standard, the comparison results seem to suggest that the systematic DIAL/AROTAL differences are an AROTAL issue. This should probably be pointed out explicitly in the conclusions of the paper.

Minor comments:

1. Section 4 (after #4389) line18. I'm not sure "well-mixedness" is a real word.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 4383, 2004.