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Interactive Comment

Interactive comment on "A review of the Match technique as applied to AASE-2/EASOE and SOLVE/THESEO 2000" *by* G. A. Morris et al.

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This paper presents a very good review of the MATCH technique. It is mostly a detailed sensitivity study of MATCH results to key parameters. The methodology is sound and properly described. The paper is well written and clear. The results are compared to the original Rex et al. group results. The results will be useful to scientists using this type of approach in data analysis.

The only part which is questionable is the last section devoted to trajectory mapping (TM). The results are not properly presented and discussed. The tendency to lump together the Morris et al. MATCH results and the TM results in the conclusion section is a bit misleading. The original and the Morris et al. MATCH approaches give different results, but, at least, the paper provides explanations for the differences. In contrast,

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there are surprisingly large differences between TM and the original MATCH results which are not really discussed, especially for the SOLVE/THESEO period. I think the first step is to compare TM results with Morris et al. results. Both approaches use the same trajectory codes, PV and SZA codes, meteorological winds and so on. This comparison would help to identify the merits of the TM approach. I am not convinced that an approach giving a minimum in variance in the results is necessarily the "best" one. Results can be robust and wrong: if there are systematic errors (which is the case according to the results), the minimum variance criteria is not sufficient. One could use the maximum likelihood criteria (which gives the same results as the minimum variance criteria only if the error distributions are Gaussian). The problem is that different approaches give different results and the expected "true" solution in terms of ozone loss is not known. One way of assessing the different approaches and their biases would be to consider trajectories which have not been exposed to sunlight. In that case, we know that the ozone loss should be null. Therefore, one could argue that the best approach should be producing an ozone loss rate close to null. The scatter around the zero line would give an estimate of the random errors.

I have also a few minor comments that the authors may wish to take into account:

The way errors are defined and described is sometimes confusing, especially in Figure 8.

Figure 2: for more than 20 hrs of sunlight, the old and new MATCH results are similar. But, for small exposures to sunlight, the results are rather different: Rex et al. still indicate ozone loss whereas there is no ozone loss in the Morris et al results (for 17 hours of sunlight, the ozone change is null and for 10 hours, the ozone change is even positive with error bars not even covering the 0 line). Is it an indication that the Morris et al. approach gives less accurate results?

Cluster spreading: one could argue that the cut off should be around 700 km. It is rather close to the 1200 km cut off found by Rex et al. for air parcels starting twice as far away from the central parcel.

P 4694: 500 K, 2000 (Fig 12): The ozone loss for TM is close to 0. It is not at all in agreement with figure 8 but there is no explanation. 450 K, 2000: Again the ozone is vastly underestimated compared to Rex et al. or the Newman et al. Some explanations would be helpful.

P 4695: I hope that the differences between TM and Rex et al. are statistically significant. If not, it would mean that the general MATCH approach cannot provide any reliable estimates, even very rough, of ozone losses during the THESEO/SOLVE period.

P 4698: The TM ozone loss are not similar to the Morris et al. ozone loss. Therefore the statement that the ozone losses obtained with theses 2 approaches are consistent with our current understanding of polar stratospheric chemistry is not supported by the results.

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