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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.

G. A. Morris et al.

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General remarks This is the first time that this method to determine ozone loss is introduced to the literature. Under these circumstances, the description and the discussion here is too short.

****The reviewer's first remark is mistaken. The subject of the Match technique has appeared in numerous papers (see our reference list). Furthermore, the subjects of trajectory mapping, trajectory modeling, and the application of models to in situ and satellite data have also appeared in numerous publications (again, please consult our reference list).

The most important points where more information is needed are the following:

Ţ Somewhat more detail on how exactly the method works should be given (see below).

****We believe this paper provides substantial detail on Match and consolidates in one place, the various filters and information necessary to reproduce the Match studies.

Ţ The results from the trajectory mapping approach should be compared with those deduced from other studies, most importantly of course with those from the “Morris et al.” version of Match. I believe a clear statement is required whether the two methods are giving consistent results or if there are discrepancies. For example, the ozone loss rate for 500 K in the year 2000 shows a very different behaviour, and very different numerical values for TM and the “Morris et al.” version of Match. Can these differences really be explained by the “taking into account all sources of error inherent in both approaches”?

****We have augmented the text Section 5.2 to provide a more detailed comparison of the loss rates calculated with the TM Match and those calculated with the Morris et al. Match. As for the comment about differences, we have attempted in this paper to come to a better understanding of the total errors associated with the loss rate calculations employed in the Match studies. While at the end of the study, we still believe we have not properly characterized all the errors, we feel we have made progress in developing a better understanding of those errors. Since we also feel that the TM Match approach is statistically defensible, differences between the TM Match results and those of the original Match and our version of Match are indicative of remaining uncharacterized uncertainties.

Ţ For Match some test calculations on the consistency of the results with theoretical expectations were conducted. An important point is whether the method diagnoses ozone loss in darkness. How much ozone loss in darkness is deduced using the trajectory mapping approach?

****We have now conducted a bivariate regression to evaluate the amount of loss occurring in the dark. Using the same period January 1 - February 9, 1992 cited by Rex

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et al. [1998], we find on the 475K surface a that change in ozone per hour of sunlight is -3.25 ppbv while the change in ozone per hour of darkness is +0.01. Rex at al. [1998] found -7.0 +/- 1.5 ppbv for each hour of sun and +0.5 +/- 0.4 for each hour of darkness. Furthermore, we have computed the average ozone change for parcels that have supposedly received no sunlight for 1992 and found an average change in ozone of 10 +/- 160 ppbv, suggesting that we have not inadvertently introduced a bias in our results.

Detailed comments

p. 4963, l. 14. What is the maximum length of the employed trajectories. I assume they could be rather long. Are trajectories of such a length really meaningful for the purpose used here? That is, do they still describe the original air parcel?

****Unfortunately, the reviewer's page numbers do not seem to correspond to the document that is posted on the ACPD web site. The maximum length of the trajectories is as follows: 10 days in the original Match study (p.4672, line 11) and 14 days in our version of Match (p. 4677, line 15).

p. 4963, l. 20-25. I believe a figure of the type of Fig. 9 would be helpful to allow the reader to assess the validity of the method.

****Since we are not sure to which method the reviewer refers due to the citation error, we cannot address this point.

p. 4963, l. 27. How exactly is the ozone loss calculated? Are again the average of 200 subsets of 50 percent of the data used? If yes, is 200 enough given that the sample is presumably much larger than for Match?

****We assume here that the reviewer is confused by the boot-strap technique, as was Reviewer #3. We have augmented the text in Sect. 2.3 to better explain the boot-strap approach to calculating uncertainties.

p. 4964, l. 9. I cannot agree that the results of the trajectory mapping approach always show less variability in the average ozone loss rates than the Match approach. For

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example, in Fig. 12 the ozone loss rate changes rapidly at the end of February and shortly before February 10; it even changes sign shortly before January 20. Is this supposed to be a realistic result? If yes, what could be a reason for such a rapid change in the ozone loss rate?

****We thank the reviewer for this observation and have revised the text.

****Regarding the sign change in the ozone loss rate, while we do not expect to see ozone creation within the vortex, the fact that the sign becomes positive, albeit briefly, is indicative of uncertainties in the technique. We note that our error bars suggest the loss rate around January 20th in Fig. 12 of our original submission could fall anywhere in the range from -3 to +4 ppbv/hour. The fact that actual air parcels will not have positive ozone changes is a separate issue from whether or not such parcels may exist in our model results. Producing positive changes in the model (erroneously) can be accomplished in several ways: incorrect diabatic descent could lead to the comparison of air parcels that, while collocated horizontally, should be vertically separated from one another. Given the sharp vertical gradient in ozone at these altitudes, this error could easily surface in apparent ozone creation. The error could also be found in the measurements from the ozonesondes. Because they are used for matches throughout a 20 - 28 day period, one biased ozonesonde can affect the loss rates calculated over an extended period of time. Again, the fact that such errors occur suggest uncertainties in the technique. We believe that such observations have not been made in previous publications on Match. Yet for a full appreciation of the technique and its limits, we felt it important to include such results here. We note that the January 8, 2000 ozone loss rate from Rex et al. (2002) indicates a positive ozone change as well.

****Finally, we have recomputed the loss rates for the TM Match using a two-parameter fit. In the original version of our paper, we had used a one-parameter fit as was used in the original Match technique and in our version of Match. However, we believe it more appropriate in our TM Match to use a two-parameter fit that allows for possible biases and offsets in the technique. Such a two-parameter fit may be more appropriate in all

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future Match work.

p. 4964, l. 20. Is a constant ozone loss rate expected for this period? Figures: The label 'Morris' is used in the Figures for both The TM-Match and "our version of Match". I suggest to use something like 'Morris-TM' in the Figures in this chapter.

****We thank the reviewer for his comment on the figure legends and have accordingly made changes in Figures 12, 13, and 14. Unfortunately yet again, we cannot address the reviewer's questions directly due to the inaccurate page references. However, more generally, we would say that whether or not the constant ozone loss rate is expected for a given period, the fact that the method yields a constant loss rate suggests something about the limitations and abilities of the technique itself. It may well be, even when properly applied, the TM Match approach is simply inappropriate for use in the calculation of ozone loss rates. In light of the fact that we believe the TM Match approach is statistically more defensible than the original Match, such results suggest something about the limitations of the original Match results and suggest that those results must be interpreted with caution.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 4665, 2004.