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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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Subsequent to the detailed public discussion on the ACPD web site, the authors of the paper submitted a revised version of their paper (not available publicly). This was forwarded to one of the original referees for further comment. In the light of these comments the authors produced a second revision to their manuscript, which has now been accepted for publication in ACP (a link to the "Final Revised Version" will appear on this web page once the paper has been published). In producing this final revision to their manuscript, the authors gave due consideration to the additional set of comments from the referee. Their responses to these comments are reproduced below.

RC = Reviewer's Comments; GM = response from Gary Morris, corresponding author.

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Interactive Discussion

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RC. I think that the sensitivity of the results to the quality of the meteorological analysis needs to be explored further (ECMWF 19 levels analysis in 1992 versus the 60 levels analysis in 2000; ECMWF versus Met. Office analysis).

GM. To address this concern, we acquired the relevant ECMWF meteorological fields and ran a series of diagnostic trajectories for the month of January 1992 using identical initialization schemes with both ECMWF and UKMO meteorological inputs. (Note that we used the ERA 40 ECMWF analyses which have higher vertical resolution than the 19- level analyses used in the original Match study for January 1992.) Heating rates for both sets of data were calculated as in Rosenfield et al. (1994). We used 30 ozonesonde station launch sites in the Northern Hemisphere to initialize profiles of parcels spaced 5 K in potential temperature apart over a range between 450 and 525 K. Parcels were initialized at 12:00 U.T. every day of the month. Matches were identified when parcels passed within 475 km of another station location, were identified to be within the vortex (as defined using the weak vortex boundary condition of Nash et al., 1996), and were found at the 475 K (+/- 10 K) potential temperature surface. Trajectories of duration 0 - 15 days were included in the analysis.

We chose to focus on January 1992 since that is the month with the largest discrepancies in the two years of our study. Furthermore, the diagnostic (and indeed the original match calculations themselves) are rather time consuming. Given time constraints, a more complete assessment comparing results from these two meteorological data sets has not been performed, although such a study may well be warranted.

We examined several diagnostic variables for those trajectories that were found to match over three time scales: 0 - 5 days, 5 - 10 days, and 10 - 15 days. Overall, the ECMWF run produced more matches on all time scales than did the UKMO run. The difference in the number of matches is especially noticeable for the longer trajectories, with UKMO producing ˜90%, ˜75%, and ˜57% of the number of matches as compared to ECMWF for trajectories of duration 0 - 5, 5 - 10, and 10 - 15 days respectively.

Histograms or probability distribution functions of the amount of sunlight exposure along the matched trajectories and trajectory length were nearly identical between the two meteorological data sets. The ECMWF trajectories showed slightly less variability in PV along the trajectories than did the UKMO trajectories. This difference likely is due to the difference in time resolution of the data sets, with the ECMWF fields produced every 6 hours and the UKMO fields only once daily. Finally, the ECMWF matches experienced notably larger descent than their UKMO counterparts, typically on the order of 2 - 5 K more descent (see the new figure 9 in the text). Given the sharp gradients in the ozone profiles near 475 K, such differences in descent could lead to larger "loss" rates when calculating matches with the ECMWF fields as compared to calculations with UKMO fields.

The fact that the loss rates appear to be quite sensitive to the exact meteorological data set used to drive the trajectory model adds yet another element to the uncertainty of the calculated ozone loss rates. Results of the Match-calculated ozone loss rates cannot be reproduced for January 1992 and may be due entirely to a difference in the calculated descent rates. Thus, Match ozone loss rates are not robust under changes in input meteorology.

As a result, we have added an additional section on the sensitivity to meteorological field selection (Section 3.7) to the paper. Furthermore, we have revised subsequent comments regarding a possible explanation for the observed differences between results from the original Match and our version of Match.

RC. The last part is the weakest. The section 5.2 about the TM results is much too short. Just putting very large error bars and finding they could cover the MATCH results cannot represent a proper assessment of the TM approach. An entire paper is required, not just 1 page.

GM. While the reviewer may be correct in stating that the TM Match approach requires more discussion, the primary purpose of this paper is to assess the Match technique as

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originally proposed and to present a possible alternative, the TM Match. Fortunately, the TM match is really an outgrowth of trajectory mapping, a technique that has been discussed in the literature in several papers by this and other authors (e.g., Sutton et al, 1994; Morris et al. 1995; Morris et al., 2000; Danelin et al, 2000). We have added text to Section 5.2 clarifying the purpose of this section in the context of the present work. Perhaps a future, more extensive study expanding on the TM Match approach as applied to ozone loss rates could be developed.

RC. “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.”

GM. The fact that the TM Match loss rates are close to zero is not surprising. Our interpretation would be that this approach to computing ozone loss rates simply cannot identify with statistical clarity any ozone loss at 500 K in 2000. Since we believe that the TM Match approach is statistically more defensible than the original Match approach, we would suggest that the original Match results may be overstating the certainty with which they can compute ozone loss rates.

RC. “The TM ozone losses 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.”

GM. We have modified the text.

RC. Overall, the errors bars on the TM approach are so large that the results become sometimes meaningless, as for 2000. By taking into account as many potential matches as possible without any kind of filtering and without weighting, the statistics for the calculated ozone rates are improved but more errors are introduced.

GM. In fact, the results are weighted, and weighted in a fashion we believe statistically defensible. TM matches that are more likely are given higher weight than those less likely. The likelihood of a match is deduced from the number of time steps over which a match is found between two sondes as well as by the number of surrounding parcels that match. Also, since the TM match approach averages over a volume of space, vertical gradients are naturally and properly included in the ozone loss rate calculations. For example, we know that the ozonesonde profiles themselves have a 100 - 200 m vertical resolution. Yet the original match approach assumes that the match occurs AT the surface identified by the trajectory calculation. Again, given the sharp vertical gradients in ozone in the lower stratosphere (where all the Match studies occur), not examining the vertical sensitivity leads to an underestimate of the uncertainty in the ozone loss rate calculations. The TM Match approach properly includes this uncertainty in the vertical position. Especially in light of the results from our diagnostic study (discussed above), the error incurred by descent rates could be significant.

RC. The gain in useful information from the possible good matches is outweighed by the degradation of information from the erroneous matches.

GM. While the reviewer may well be correct, this statement amounts to speculation as he/she probably is not in a position to test this assertion. We would counter by saying that none of the matches that have been identified is highly probably, and that probability decreases dramatically with the duration of the trajectories. However, the behaviour of the ensemble is more reliable. And by including more matches, the ensemble grows in size. (See Morris et al., 1995.) Including junk in the ensemble will indeed make the ensemble less reliable, but being able to differentiate the junk from the good trajectories is not trivial. The approach of the original Match represents one possible attempt. Yet the sensitivity studies included in this paper suggest that some of the filters used may not be appropriate or necessary. Some of our results appear to be at odds with the earlier study of Grooß et al. (2003), but as we point out in the paper, our study uses the actual data while that of Grooß relies on a model.

RC. The temporal evolution of the TM ozone loss rates looks completely random. They are at odds with photochemical models and with the Rex et al's results and the authors' MATCH results. Not surprisingly, some selective filtering and weighting are needed for this type of approach.

GM. We disagree. The 2000 data at 500 K reveal no statistically significant loss rates over the entire study period, thus the "wandering" of the loss rates looks random. Our interpretation would simply be that the TM Match approach cannot identify loss occurring during this time period at this altitude. The 2000 data at 450 K indicate significant losses in January, decreased loss in early February, and small losses during late February and March. Finally, the 1992 data at 475 K show an evolution similar to that of Rex et al. The only really troubling parts of the time series are the large loss rates toward the end of March, which are difficult to explain yet may serve again only to point out the uncertainty inherent in the Match approach.

RC. The last sentence of the abstract is rather strange: "As compared to loss rates from our version of MATCH, the trajectory mapping approach produced generally smaller loss rates, frequently not statistically significantly different from zero, calling into question the efficacy of the MATCH approach". Surely, the authors must mean "calling into question the efficacy of the TM approach".

GM. In fact, we mean what we said, not the alternative suggested by the reviewer.

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