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Atmospheric Chemistry and Physics Discussions

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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****Unfortunately, we note that we have two reviews from "Referee #2" and no comments from "Referee #1." We suspect this is an editorial error in posting as the reviews seem to be substantially different.

Ţ The arguments in the paper regarding the necessity of certain Match filters are based on the sensitivity studies summarised in Figs. 3.-5.... ****While we do not disagree with the reviewer, we note that the original papers do not offer a more quantitative justification for the selection of these filters. Conversations regarding the Match technique with M. Rex over the last several years yielded the justification that the selected criteria seemed to minimize the errors. One of our major points is that such criteria are, as the reviewer points out, somewhat arbitrary. Our sensitivity studies, particularly the "Senstivity to population selection” of Section 3.6 on pages 4685 - 4686, suggest that many of these filters are unnecessary. We have revised the text of the paper appropriate to reflect these considerations.

Ţ Figure 9 is important in the arguments presented in the paper... ****We note that the thick blue (not black) line in the Figures 8, 10, 11, and 12 is precisely the mean of the distribution of black dots, each one of which is produced by the process indicated by Figure 9. The thin blue lines then characterize the standard deviation of that distribution. As a result, we feel that the paper already contains the information in which the reviewer is interested.

****The reviewer is, of course, correct that the result shown in Figure 9 is not very likely, as indicated by the absence of black dots in Figure 8 with such large loss rates. Nevertheless, the figure demonstrates how changes in the subset of data selected can lead to large possible changes in the ozone loss rates calculated. Since Rex et al. and we are using different wind fields and heating rates, our Match subsets are different. Differences in our results may be due, in part at the least, to such a selection effect, which is the point of the figure. We have revised the text in Sect. 4.1 to address this point more clearly.

Ţ An important conclusion from this paper is that there are significantly larger uncertainties in Match than reported in previous papers.... ****The reviewer and M. Rex (in his comment on this paper) both point out this inconsistency between the wording of the paper and the error bars in the figure. We have revised the text throughout to correctly reflect the fact that our random errors are in agreement with those found in the Rex et al. papers.

****As to which set of error bars is appropriate, we feel that the green error bars, which include both statistical (random) errors and estimates of the effect of trajectory errors and uncertainty due to the definition of the vortex boundary, are more representative of the true uncertainties with the Match technique. Using the green error bars, we still find a few periods of time (Days 55 - 75 at 450K in 2000 and around Day 45 at 475K in 1992) at which our ozone loss rates are significantly different than those calculated by Rex et al. (at least at the 1?-level), but not more than would be expected statistically, which gives us some comfort in assessing our error bars.

****The comment that “hardly any information is left in the Match results” is an important observation. We believe that if you include all the uncertainties (random and systematic) appropriately, there are times for which the Match approach seems to provide little information, i.e., the loss rates are not statistically different from 0 (e.g., most of February 1992 at 475K).

Ţ An important check on the validity of the original Match method was the demonstration that there is (within the error bars) no ozone loss in darkness. It would be important to show results for this question based on the Morris version of Match. ****We have performed a bivariate regression analysis - see our response to the other Referee #2.

Finally, this paper that is concerned with two winters... ****We thank the reviewer for his comments. His points are well taken. However, we feel that it is beyond the scope of this paper to review all the winters. Our primary purposes in this paper are to succinctly and in one paper summarize the Match technique, to try to reproduce the Match results for two winters, and to more carefully examine the uncertainties inherent in Match. Adding a third winter to this paper would involve a substantial investment in time and resources. Following up the studies presented in this paper with such a study would make a good future paper to reinforce the results of this paper. However, we believe we have demonstrated the validity of our Match code with the bi-variate regression and the evidence presented in Figure 2 that reproduces Figure 5 from Rex et al. (1998). We would hope the referee would agree with our decision not to perform the study of the 1994/1995 winter for inclusion in this work.

Detailed comments

abstract, l. 20. “less loss” or a smaller loss rate? ****Good point. Thanks. Text

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changed.

abstract, l. 25,26. Mention how well the trajectory mapping and the Match results compare. ****Text changed.

p. 4668, l. 3. Is the implication here that using two tracers could address the issue discussed above? If yes, some explanation should be given. ****The two-tracer correlations of Richard et al. (2001) would be important for studies of loss over extended periods of time during which the relationship between two tracers may change. The results of Richard et al. found a loss of 1.8 +/- 0.3 ppmv from 20 January to 12 March 2000, in good agreement with most of the results found in Table 8 of Newman et al. (2002). While this approach may prove more reliable, as the next paragraph points out, simultaneous measurements of multiple tracers are rare. As a result, while theoretically desirable, the Richard et al. approach proves impractical to apply to most historical data.

section 2.1 Regarding the accuracy of the ozone sondes... ****We have removed the Reid et al. citation, although we feel that the information in that paper on the ozonesondes is appropriate. In its place, we have now cited the more recent paper of Komhyr et al. (1995) on the STOIC ozonesonde performance study.

p. 4672, l. 20. This statement seems to imply that in the original Match technique WMO data files are being used, I do not think that this is correct. Please clarify. ****I will follow up on this point with M. Rex and clarify in the paper.

p. 4676, l. 15. Why is the boundary definition in the original Match technique only approximated - it could have been used precisely as used in the original Match technique. ****Fair point. Since the precise location of the vortex boundary is uncertain by its very nature, the sensitivity of the results to the choice of the vortex boundary is probably the more important result in this paper. As Figure 6 demonstrates, the value of PV used to define the edge appears to have little impact on the loss rate calculations in February and March. In January, the calculation is somewhat more sensitive, which implies that the ozone loss rates calculated will be dependent upon the precise definition used. Results from Figure 6 suggest why we and other groups may not find exactly the same loss rates as Rex et al.: unless you use the same wind fields and the same vortex definition, the loss rates may vary, with an observed variability of ˜2 ppbv/sunlit hour in January during 1992.

p. 4676, l. 23 “Sun on the horizon”: taking into account refraction or not? ****No. However, other uncertainties are certainly larger than the impact of not accounting for refraction correctly - for example, incorrect descent calculations. The text has been revised to correctly reflect that the Sun is approximately on the horizon.

p. 4676, l. 25 Would it be possible to estimate how large the error introduced by the trajectories is? Is it equivalent to say a SZA variation by half a degree? ****We refer to the paper of Morris et al. (1995) in which it is shown that individual trajectories become highly inaccurate in relatively short periods of time (i.e., on the order of a few days). Part of the point of the SZA sensitivity study was to demonstrate that errors in trajectories of on the order of 4 degrees latitude could result in large changes in the calculated ozone loss rates. Since air parcels tend to be stretched out along PV contours, separations by such distances near the vortex edge would not be uncommon. The real question is whether or not a systematic bias can be found in one meteorological data set versus another. This issue was also raised by Referee #3. See also our response to that review.

p. 4677, l. 20-21. Could a statement be made how the Lagrangian estimates depend on the time resolution? For example could there be systematic differences between trajectories computed from data sets with high and low temporal resolution. ****We would refer to the reviewer to the Waugh and Plumb (1994) paper. The fields with lower time resolution are less capable of reproducing filamentation of the vortex edge, a factor in mixing material into and out of the vortex. As in our response to S. Tilmes, less mixing of air from outside the vortex into the vortex would reduce the chances of us overestimating the loss processes.

p. 4678, l. 6,7. Is the diabatic correction applied to both PV and Theta or just to Theta? ****Diabatic corrections obtained from parcel trajectory calculations were applied using the "diabatic coordinate adjustment" method of Lait et al. (2002). Trajectories were run from the actual measurement locations and times towards a common date in the middle of the period being examined. The PV and theta values of the measurements' parcels at that common date were used in the PV-theta analysis, after discarding parcels whose PV values had changed by more than 12.5% (see below). Thus, diabatic corrections were indeed applied to both PV and theta.

p. 4678, l. 10. This is the technique for calculating ozone loss used by Lait et al. (2002) - correct? Perhaps add this citation here again? ****Okay.

p. 4678, l. 17. How is the criterion (12.5%) selected? How sensitive are the results to this criterion? ****If the criterion is set too high, then parcels will be included which crossed the vortex boundary. In that case, the effects of mixing contribute to the change in ozone values along with diabatic descent and chemical loss. The effect shows up as greater scatter in ozone values about a point in PV-theta space. If the criterion is set too low, then too few parcels will be chosen to obtain usable statistics. The 12.5% threshold was chosen as a tradeoff between these two effects, to exclude as many points as possible that crossed the vortex boundary while still providing enough points for good statistics. Its sensitivity to small changes was not thoroughly examined.

p. 4678, l. 20-23. Of course, averaging the loss rate over relatively long periods, as it is done here, means that peak loss rates are smoothed out. This should be taken into account when comparing loss rates from this technique with those from Match. ****Fair point. Since we only use the PV/Theta approach to assess the integrated losses rather than the peak losses, however, the comparisons should still be valid.

Fig2: The greatest discrepancies between the Morris and the Rex version of Match in Fig. 2 appear for sunlit time less than 20... ****We thank the reviewer for catching this error. In fact, the version of the figure that was included for review was an old version of

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the figure that had been created using an older version of our Match analysis routine. We have updated the figure. The new (correct) version of the figure shows much better agreement with the original data of Rex et al. (1998) at the smaller sunlit times, although somewhat less good agreement at longer sunlit times. However, all the data are within the combined one standard deviation error bars of the Rex data.

Section 3.1, Fig. 3: In my opinion, a more quantitative criterion for the cut-off value... ****We concur with the reviewer's desire to be more quantitative. As we pointed out above, however, the initial papers on Match did not provide much in the way of a quantitative justification for these filter choices. It is difficult to identify an appropriate value at which the PV filter should be activated. In both 1992 and 2000, the loss rates appear to change with respect to the change in PV, with decreased loss rates at higher DPV in 2000 and increased loss rates at higher DPV in 1992. Year-to-year differences make selecting a single value difficult. By setting the filter at too low of a value, you do not get enough matches for viable statistics. The reviewer's point is well taken and illustrates the difficulty with using and defining all of the filters employed in the Match technique. It turns out to be hard to justify the precise values selected in a quantitative way. As a result, we prefer an approach that does not rely upon such filters, for example, the trajectory mapping (TM) Match.

Section 3.2, Fig. 4: Similar as in section 3.1, in Fig. 3, it is not clear what the precise criterion is... ****Again, we thank the reviewer for this comment. Just because our results might suggest ozone production in the vortex does not imply that we would defend that result scientifically. Our study is an attempt to evaluate the validity and limitations of the Match technique, not to defend it. We believe that the technique has some limitations that have not been apparent in the published literature. The reviewer has again reacted to a point that is troublesome. The fact that the results can change so much depending upon the precise value of the filters chosen means that by an appropriate manipulation of the filters, one could achieve whatever loss rate one desired. Troublesome indeed.

Based on Figure 4, my impression is that the results for the ozone loss rate are becoming arbitrary for spreading greater than ~ 2000 km. Judging from Fig. 4, I would suggest a spreading cut-off of ~ 800 km. ****Again, it is difficult from the figure alone to identify a consistent distance at which to invoke a filter. We remind the reviewer also that our parcels begin only 50 km from the central parcel, whereas those from the Rex et al. study being 100 km from the central parcel. When our parcels are 1000 km away, we would expect those of Rex et al. to be further away still. As a result, this figure should be viewed as an upper limit for the Rex et al. studies.

Moreover, the authors note some discrepancy between their results and those of Grooss and Mueller (2003)... ****Figure 4 is produced with a 40 pvu limit to the change in PV along a trajectory, as suggested by the results of Figure 3. It would appear difficult to argue for the introduction of a bias in our approach if the limits on parcel separation and PV differences are placed at 2500 km and 40 pvu respectively. We believe that the major difference in our results from those of Grooss and Mueller lie in the fact that we were using ozonesonde data while they were using the results of a chemical model.

p. 4682, l. 26 Is this gradient really stronger in the NH than in the SH? ****We have revised the text to say "particularly in the winter season." Descent is certainly stronger in the NH winter due to increase wave activity.

p. 4683, l. 5-10 Would it make sense to show a calculation that reproduces the Rex vortex definition exactly? ****See earlier comment. Not a bad idea, but in light of our intentions, we do not believe it is necessary.

p. 4683, l. 12-17 A list of possible reasons is given for the observed discrepancy. But can it really be excluded with certainty that neglecting the trajectory spreading criterion could have had an impact here? ****The reviewer makes a fair point. From Figure 4, we would argue that the parcel spreading criteria makes no difference here. We have rerun our analysis and reproduced this figure including and excluding the parcel spreading criteria. We see no statistically significant differences in our results. Therefore, we

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can conclude that the parcel spreading criteria is not responsible for the differences between our results and those of Rex et al. We have updated the text to indicate that adding back in the parcel spreading criterion has no impact on the results.

p. 4684, l. 5 State exactly by which filtering criteria the sonde pairs are eliminated. ****We have modified the text to clearly state that the numbers quoted represent the number of matches eliminated by all filters combined.

p. 4684, l. 7 Could there be any reason why the trajectories would be biased? Is not the filtering procedure introduced to avoid biased trajectories/sonde pairs? ****Rex et al. developed a detailed and elaborate methodology for the elimination of matched ozonesonde observations in hopes of reducing errors introduced by inaccurate trajectory calculations. Since their filters, however, eliminate such a large percentage of the available data, it is quite possible that they have unintentionally introduced a bias by eliminating valid matches. Such a result seems a reasonable explanation for the very high loss rates in January 1992, for example, for which Match yields rates irreproducible with our current understanding of polar stratospheric chemistry.

p. 4684, l. 11 The DPV filter means 40% cut-off, correct? ****It means a 40% cut-off in 1992 and a 25% cut-off in 2000. We have added a reference at this point to the earlier part of the paper in which the DPV filter is defined.

p. 4684, l. 20 "Very little" should be quantified; e.g., is it less than half a degree? ****We have determined that it amounts to less than about 10. The text has been appropriately updated.

p. 4685, l. 9 Would systematic differences be expected? ****ECMWF changed from 19 levels up to 10 hPa in 1992 (first Match study) to ˜60 levels up to 0.1 hPa in 1999. UKMO has provided a consistent stratospheric data set throughout the Match study periods. While we are not aware of any publication comparing these two meteorological data sets, it would not be at all surprising that such a change in the ECMWF vertical resolution would lead to systematic biases. Furthermore, the way in which vertical descent is calculated in Match has also changed with time. Again, systematic differences may have resulted from such changes. In our study, however, we have consistently used the same heating rate calculations.

p. 4685, l. 14,15 It would be of interest to determine how much ozone loss would be attributed to "dark" periods under the assumption of certain SZA cut-off criteria. ****We have performed the bivariate regression for our Match code and ascertained that losses are confined to sunlight hours. At this point in the paper, we do not believe it necessary to calculate bivariate analyses for the sensitivity to the SZA cut-off criterion. The main point of performing this sensitivity study was to examine the impact of small changes in trajectories on the calculated ozone loss rates. We do not mean to suggest that the chemistry occurs at any other SZA, simply to use SZA as a proxy for trajectory uncertainties. We have attempted to emphasize and clarify this point in the text and in our responses to the other reviewers.

p. 4685, l. 27 Add "our version of Match" here. ****Text has been modified.

p. 4686, l. 2 The necessity of the filters in Match is a central point of this paper, therefore I would argue that these results should be shown in the paper. ****At the reviewer's suggestion, we have added a figure for this sensitivity study. The results are essentially the same as the results from the study that includes all of the filters, although, as expected, the uncertainty estimates for the curve with no filters are smaller than those for the case with all the filters.

p. 4687, l. 1 and 6 If potential temperature levels are given it should be clarified which potential temperature range is meant. For example is it 450 ś 20 K? ****We have clarified the paper by adding text at the beginning of Sect. 4 and in the methodology Sect. 2.2 to define the range of potential temperatures included for each surface in our study.

p. 4687, l. 9 I do not believe that this figure is really helpful... ****See earlier comment in response to Referee #2.

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p. 4687, l. 15-17 I am confused here. If such a deviation is extremely unlikely (and how unlikely it is could be determined, if the suggestion above is followed), why is this an indication for an underestimate of the error bars by Match? ****The reviewer makes a fair point. The text has been modified.

p. 4687, l. 23 Why 200? Is this number of subsets large enough for a large number of data points?... ****We chose 200 to have a large number of subsets represented and so that the statistics would be stationary. In fact, we tested 300 subsets and found no change in the statistics. Since the subsets are randomly selected, duplicates are allowed. If we were to select each possible 50% subset exactly once and compute the average, we would have been better served by simply computing the average of the complete data set once, as the reviewer points out. The advantage of the 50% calculation is to demonstrate the sensitivity of the results on the particular subset selected. The variety of different answers that can be found with different subsets is indicated by the spread of the black dots seen in Figures 8 and 10 - 14. Since we used a somewhat different approach than Rex et al. and since we do not know exactly which matches qualified for inclusion in his loss rate calculations, we felt that taking random subsets would provide us a better chance to sample the same variable space that Rex sampled. Even with this approach, however, we found it difficult to reproduce the very large loss rates seen in January 1992. We have attempted to clarify the text in this section to address the reviewer's concerns and questions.

p. 4688, l. 15-17. If one takes the "green lines" serious as error bars, then all one can learn from Figure 8 is that that the ozone loss rate is between roughly zero and the estimates from the Rex version of Match. The question is how much can be learned from this observation. In mid-January, the range of the green lines encompasses some ozone production to loss rates even greater than those reported by Rex et al.! ****Yes! Again, see our earlier response.

p. 4688, l. 25-29. The question of forcing the fit through zero is a potentially important point... ****The reviewer's point is an excellent one. Our first task, however, was to

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repeat the calculation of Rex et al. (1998). In so doing, we followed the same constraint as they did with regards to fitting the data through the origin. The argument for so doing is that no loss should occur at night. Of course, it does leave open the possibility that if your analysis technique somehow biases the data, the best fit line might not go through the origin.

****When we allow our fits to float and do not force them through the origin, we find somewhat larger loss rates (in better agreement with those found by Rex et al.) for the January 1992 period during which the original results are so troubling. However, we do not include a two-parameter fit for the portion of the paper during which we describe our version of Match. We have redone our TM Match calculation (Section 5) using a two-parameter fit since we agree that it is probably the more appropriate approach for a technique with uncertain biases.

****As for using the standard regression routines, we find that our statistical approach should be identical. In fact, our statistical uncertainty estimates are of very similar magnitude as those found and reported by Rex et al. Using the standard routines should not impact the uncertainty estimates, although our approach does highlight the variability of possible results.

p. 4689, l. 15. Of course, another possibility would be that one of the methods has a bias that can be explained and corrected. ****Fair point. We have modified the text appropriately.

p. 4689, l. 19. Never before or never thereafter? ****We have modified the text.

p. 4689, l. 20. Perhaps one should cite a paper like Becker et al. or Rex et al. (2003) here. ****We have added the reference to Becker here.

p. 4692, l. 4. Did Proffitt et al indeed report ozone loss rates in ppbv/sunlit hour? I believe not, so that the original number should be quoted as well and the method to convert to ppbv/sunlit hour should be briefly explained. ****The Profitt et al. paper

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provides in Table 2 ozone loss rates in ppbv/day for select days during the 1991/1992 winter season. That table assumes 6 hours of sunlight. We have updated the text to include this information.

p. 4692, l. 19. But Match does not measure “localised loss rates”. Rather, the average ozone loss along a trajectory is measured. The strongest contribution to the determined ozone loss rates comes from trajectories over several days that sample a reasonably sized part of the vortex. So “localised” can only be true in a Lagrangian sense, that is a particularly strong ozone loss in a particular air parcel. ****In fact, we did mean it in a Lagrangian sense. The text has been modified appropriately.

p. 4693, l. 3. Integrated loss: over which time period? ****We have now specified the time period, although it varies from study to study.

p. 4693, l. 5. Which models? Give some examples? ****The models are listed in the previous paragraphs. Since this paragraph is just a summary, we feel it unnecessary to repeat the list at this point in the text.

p. 4696, l. 7. The agreement is not excellent for the first two points. ****As mentioned earlier, the figure was incorrect. The agreement with the first two data points is quite good with the corrected figure.

p. 4696, l. 17. If the methodology of Schoeberl et al. (2002) is similar, then the method should produce similar results for the winter 1999/2000. If this is the case, it might be worth mentioning here. ****We have now added to the text in Sect. 6 the specific results of the Schoeberl et al. (2002) study.

p. 4696, l. 25. What would be required for a more definite answer to this question would be to have an estimate by how much the sunlit time along a trajectory varies, e.g., when different meteorological data sets are being employed. ****We have not performed our Match study with other meteorological analyses. Even were we to do so, we would need to find matches common to both meteorological data sets and examine the variability of the sunlit time derived from the trajectories. We feel that such a study is beyond the scope of this paper. However, we can look at the variability in the sunlit times for the other parcels in the cluster initialized and advected in the trajectory model. If we look at the difference between the parcel in the cluster with the greatest sunlight and the parcel with the least, we find a mean difference of 2.2 hours with a standard deviation of 2.5 hours. A histogram of the differences shows a sharp peak at 0 hours with a tail extending out to 19 hours difference. We have augmented the text in Section 3.5 to address this point.

p. 4696. On this page various results from 'our version of Match' are reported with error bars. However, given the extensive discussion of error estimates in the paper it is not clear to me here how the error bars reported here are derived. This should be clarified. ****We believe the reviewer is referring to p. 4697. We have clarified the text on this page to state specifically that these are 1 sigma error-bars representing statistical uncertainty based upon the error bars associated with the loss rates themselves.

p. 4697, l. 21. I would not agree that "slowly varying" is a good description of the ozone loss rate for 2000 on 500 K. ****We have modified the text.

p. 4697, l. 28. "may not be representative": this may be true, however at this point is a speculation for which there is no direct evidence. Am I wrong? ****In fact, Rex et al. indicate that the Match approach uniformly samples the vortex in PV space. Without all of their data, however, we cannot establish which matches have been eliminated and which have been kept. We would stress the conclusion of Salawitch et al. (1993) as a possible explanation.

p. 4697, l. 4. "significantly larger uncertainties": but in this section somewhat above, error estimates are given from the Morris version of Match (ś0.2) that are rather small and are very close to those reported for the original Match analysis. Is there no contradiction here? ****We have modified the text to reflect the fact that our statistical errors are in agreement with Rex et all, but that we feel our total errors (the green error bars in the figures) are more representative of the true uncertainty in the Match approach.

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