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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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The Match technique has now been used extensively for determining ozone loss, and it is important to understand the methodology and accuracy of the technique. This manuscript provides a good review of the technique, and some important and interesting sensitivity studies and comparisons. One of the important conclusions is that the error bars for the Match calculation may be larger than previously reported. This supported by sensitivity studies, and the fact that differences arise even when attempting to reproduce the same results, but with only slightly different methods and meteorological analysis. The manuscript is generally well written, and most of my comments pertain to the interpretation of the results.

(1) Section 3 shows the sensitivity of the match results to the choice of several quality

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filters. One of the basis for choosing the filter value in figures 3-5 is the reduction of the standard deviation (thin lines). There is no explanation as to what exactly the standard deviation (thin lines) represent. Is it the standard deviation of the match samples about the linear fit? Is it an estimate of the error of the slope estimate for a linear least squares fit constrained to the (0,0) point? Is it based on the random selection method described in section 4.1? This calculation needs to be described, along with the motivation for using it.

****The error bars seen in figures 3 - 5 represent the standard deviation of the computed ozone loss rates using the subsets of the data selected with the boot-strap technique. The boot-strap technique takes a random subset of n elements from an original data set of n members, allowing duplicates in the subset. A linear regression constrained to pass through the point (0,0) is performed and a slope computed. The process is repeated for a minimum of 200 iterations and continues until a maximum of 20,000 iterations is complete or the change in the standard deviation of the set of slopes so derived is less than 0.001% from its previous value. We have elaborated in Section 2.3 to clarify our procedure for calculating these error bars.

(2) This is a general comment about the handling of the SZA issues within the manuscript. The ozone loss rates depend strongly on SZA, decreasing rapidly at SZA values greater than about 88 degrees. A linear relation between ozone loss and sunlit time for multiple matches can only be expected if the distributions of the SZA values along the different match trajectories are similar. This may often be the case, and is why the linear fit works well for the match analysis. The choice of the exact SZA (i.e. 90-96 deg) threshold for defining the sunlit time only makes a difference when the trajectories spend most of time at these high SZA values, such as in January, as illustrated in Figure 7. The parameter, loss per sunlit hour, is a contrived quantity that is dependent on the choice of the definition of sunlit time. For comparing different match analysis or using the loss rates for other applications, it is important that the same definition of sunlit time be used. For example, if the loss rate is used to estimate a vortex

average loss or compare with a photochemical model, the same definition of sunlit time should be used, as has been done in the Rex et al and Becker et al studies. Therefore a sensitivity of the calculated loss rates to the SZA threshold should not be considered an error source, any more than the approximation that the ozone loss is linear in sunlit time.

****The reviewer's comments are well taken. In our study, however, we use a cut-off of 950 SZA to define the day/night boundary. We arrived at this value after discussions with M. Rex. While he uses a more detailed calculation of the precise SZA at which the sun disappears below the horizon for each air parcel, he felt that (and I confirmed with a calculation that) we could use the 950 cut-off with little impact on the results. From 15 to 25 km, the SZA at which the sun disappears below the horizon ranges from 93.90 to 95.10. If anything, therefore, we are underestimating the hours of sunlight along our trajectories, which would have the effect of raising the ozone loss rates.

****Furthermore, the SZA sensitivity is mainly included to show the importance of getting the trajectories correct rather than getting the precise angle at which the chemistry begins/ends correct, as is pointed out in the manuscript on page 4683 (lines 19 - 26) and page 4686 (lines 17 - 20).

(3) In Section 3.5, the sensitivity of the match result to the SZA threshold is described as a proxy for the sensitivity to trajectory errors. The manuscript correctly notes that changing the threshold is similar to shifting trajectories in latitude (poleward or equatorward). However, trajectory errors caused by errors in the wind flow are likely to simply shift or distort the circumpolar flow, which leads to both positive and negative latitude errors (positive and negative SZA errors) over the course of a trajectory. In such a case, the calculated total sunlit time may be in error, but the errors could be positive or negative. Changing the SZA threshold corresponds to the case in which the sunlit time for all of the match trajectories have an error of the same sign, which is not expected.

****The reviewer makes an interesting argument, suggesting that the errors will likely

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cancel. Let us suggest two ways in which they will not simply cancel. The Arctic polar vortex is often found shifted off the pole (wave 1 disturbance) toward the European continent. During winter, latitudes above the Arctic Circle receive little to no sunlight. If the polar vortex is shifted too far away from Europe in the meteorological analysis relative to its actual location, the amount of sunlight received can be underestimated. The fact that some latitudes will be too low while others will be too high does not necessarily result in cancellation of errors in the calculation of sunlit time. Furthermore, if the vortex in the model is too small or shrinks after the initialization of a trajectory as compared to the actual vortex, the amount of sunlight received by the model air parcels will again be underestimated as compared to the actual air parcels. Therefore, we would argue that it is indeed possible for a bias to exist in the estimation of the amount of sunlight air parcels receive in the model. We have added text in Section 3.5 to delineate these possibilities.

****A better method for simulating the effect of trajectory errors would be to randomly perturb the SZA or the total sunlit time of each match trajectory, and then perform the linear fit. The size of the error may be much less than suggested here. It follows from this comment that the total (green) error estimates in figures 8, 10-11 may not be accurate.

****We thank the reviewer for the suggestion. However, if the SZA errors are not in fact random, a case we suggest may be true in the paper, then this approach is not helpful. In fact, with such a test, the changes in the calculated loss rates will be random, as increases and decreases in SZA will cancel. As for the size of the green error estimates in figures 8, 10, and 11, we believe they are still too small compared to the actual total errors, as evidenced by the remaining differences between our loss rate calculations and those appearing in the original papers by the Rex et al. group.

(4) The goal of the match calculation is to provide an accurate and unbiased estimate of the loss rate. In section 3.5 (and in the sensitivity studies) the emphasis is on reducing the variance (increasing precision) of the calculation. However, it is entirely possible

that increasing the number of matches, by relaxing the match constraints, could decrease the statistical error of the loss rate while increasing the absolute error or bias. If, as the constraints are relaxed and more matches are found, the calculated loss rate systematically changes, it may represent an increasing bias. This may be a concern to the Trajectory Mapping approach in section 5.1 which shows significant differences from the match technique. The tight constraints of the original Match calculation were designed to ensure that it was a Lagrangian measurement, and eliminate unwanted mixing effects. Several publications have suggested there was little cross-vortex mixing during the SOLVE I winter. However, for winters with more cross-vortex mixing, the constraints may be more important. This issue should be addressed in the manuscript.

****If we understand the reviewer's point, he/she is suggesting that if there is more cross-vortex mixing, and if there are more matches with more samples from air masses that cross the vortex boundary in the TM match, we could end up with a different loss rate. Match constraints are designed to eliminate unwanted mixing events (through the PV filter). However the degree to which such constraints are successful is not completely clear. The constraint requires our ability to detect a PV trail for mixing events. Yet numerous trajectory studies have shown the PV trail fades quickly (e.g. Waugh et al., 1994). Furthermore, if it is the PV constraint that identifies such cross-vortex mixing events and if the same PV filter is applied to the TM Match, then TM Match and the original Match should find the same results, all else being equal.

****While we accept the reviewer's point that there appear to be some stark differences in the loss rates calculated with the TM Match as compared to the original Match, we believe that the TM Match is more defensible from a statistical point of view. In particular, we avoid throwing away matches wherever possible. Furthermore, we allow for vertical separation between the matched air parcels (advected from the old ozonesonde compared to new ozonesonde). The heating rate calculations inherently contain errors that lead to vertical offsets in the locations of the modeled air parcels from the actual air parcel trajectories. While globally, you would expect such errors to cancel, using the

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restricted domain of the Match data, it is possible that descent is systematically over- or underestimated. Again, such systematic effects can lead to biases in the ozone loss rates produced by Match. The TM Match includes this uncertainty in a natural way, by considering a cylinder of matched parcels rather than a circle. More discussion of this point is found in addressing the comments of Reviewer #2. Finally, we do not force our loss rate fits through the origin, but instead allow a two-parameter fit.

(5) The "boot strap" method used in section 4.1 is not explained well. In the 3rd paragraph of section 4.1, the text says "computed using the boot-strap technique (described above)". However, as far as I can see, only the random subset method is described. The boot strap method and the motivation for using it should be explained better.

****See the response to point (1) for more detail on the boot-strap method. The description of this technique has been augmented in the paper in Section 2.3.

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