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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.

Anonymous Referee #3

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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 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.

(2) This is a general comment about the handling of the SZA issues within the manuscript. The ozone loss rates depends 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 distribution 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. Therefor 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.

(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. 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.

(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.

(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.

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