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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 paper focuses on the uncertainty that is connected with Match analyses of ozone loss rates in the Arctic. It discusses both, the statistical uncertainties and possible systematic biases. The distinction between both is sometimes not very clear in the paper.

****We have attempted to clarify in the paper which errors are statistical and which represent possible biases. We have also modified the paper to clearly state that our statistical errors are of the same magnitude as those of the papers by Rex et al.

****We owe a debt of gratitude to Markus. Over the last several years, frequent discussions with Markus have clarified much about the Match technique that we were unable to completely ascertain from the Match publications. As a result, we have now been able to reproduce very closely his results.

I now come to the possible systematic effects that could have an impact on the Match results. The green lines in Figures 8,10 and 11 represent an estimate of the systematic uncertainty or bias of the approach as estimated by Morris et al. By us the possible systematic errors of Match results have been discussed in much detail, separately from the statistical uncertainty (e.g. Rex et al., 1993, 1997, 1998, 1999, 2002). We always clearly state that the error bars in our figures represent one sigma statistical uncertainties. Hence, the green lines cannot be compared with the error bars in our figures, but should be compared with our estimates of the possible systematic bias of the approach.

****We have added text to the paper to reference the work Markus has done in examining the systematic errors associated with Match. Also, we clarify that the blue and gold error bars are the ones that should be compared to the error bars appearing in the papers of Rex et al. We have also added a comparison between our estimate of the total error (green error bars) and the total uncertainty of Rex et al.

Over the years we have put a lot of effort into estimating the systematic error of Match and it would be beyond the scope of this comment to repeat this discussion. Details can be found in the above mentioned papers. In summary our confidence that the systematic uncertainty of Match is much smaller than suggested by the green lines Figures 8,10 and 11 is mainly based on two points: First, we do not find any significant systematic change of ozone during dark parts of the trajectories (based on many bivariate regression analyses; e.g. Rex et al., 2003).

****We have added a bivariate regression analysis. See the response to Referee #2.

Second, we do not find any significant change of ozone during warm winters, when temperatures stayed above the PSC formation threshold (Schulz et al., 2003). Also, Harris et al. (2002) concluded that the systematic error of match is below 20%. This work was based on a comprehensive intercomparison of Match results with results from completely independent approaches.

****While we have not performed a study of one of the warm winters with our code, we have looked at the change in ozone with zero sunlit time along the trajectory for the two years included in this study and found almost no change in ozone in 1992 (10 +/- 160 ppbv) and in 2000 (120 +/- 360 ppbv).

Why does Morris et al. reach a different conclusion? In the Morris et al. paper the estimate of the systematic uncertainty of Match is based on a simple analysis how the ozone loss rate per sunlit hour changes if the definition of the solar zenith angle, that defines the terminator, is changed.

****Actually, the estimate is also based upon the observed variability in the ozone loss rate associated with the definition of the vortex boundary.

As I understand it, the reason behind this procedure is some concern that the trajectories may systematically drift to lower or higher latitudes and therefore see more or less sunlight than the real air masses. Beside many other potential systematic effects that have been discussed by us in the past, this effect would indeed also lead to a systematic bias in the Match results and is an interesting aspect of the paper. However, as long as the trajectory errors are random, the effect would be included in the statistical uncertainty that is calculated from the scatter of the data. So only a systematic drift of the trajectories would be of concern. Since such a drift would be of concern for a number of reasons, we have addressed a potential drift of the trajectories in quite detail in the past. One way to look at it is to analyze the systematic change of PV along the trajectories and assess whether this is consistent with what we would expect from diabatic effects, i.e., from changes in PV based on changes in theta due to radiation, and changes in PV due to wave drag from dissipating waves. Because the latter is difficult to quantify, it is hard to get to very high levels of precisions with this approach. But from the fact that the systematic change of PV along the trajectories is very small, we can absolutely rule out that any systematic drift in latitude can be anywhere close to six degrees over a ten day period. But such a rapid drift would be required to cause the six degree systematic effect on solar zenith angle that is the basis for Figure 7 and the estimate of the systematic uncertainty in the Morris et al paper. From looking at systematic changes of PV along the trajectories, it seems that one degree systematic drift over a ten day period would be a robust upper limit. In general, any systematic drift of trajectories in the order of six degrees latitude over ten days would completely mess up any lagrangian transport calculation, reverse domain filling trajectory study, contour advection approach, and other application of trajectories that have been widely used in the past and have been demonstrated to work well. E.g. a trajectory based CTM like CLAMS would completely fail, if the trajectories would systematically drift by six degrees latitude over a ten day period. But it has been demonstrated that CLAMS does reproduce observed tracer fields very well.

****We do not mean to suggest by Figure 7 that the trajectories are in fact in error by 60. However, they need not be in error by so much in order to result in variability in ozone loss rate calculations. The figure suggests that in January, a one-degree latitude error in the trajectories will result in a 1 ppbv/sunlit hour change in the ozone loss rates. Previous studies of the spreading of air parcel clusters, particularly near the vortex edge, indicate that such parcels quickly are stretched apart over long distances parallel to the PV contours. While the PV filtering and parcel spreading criteria have been introduced to prevent the inclusion of such parcels in the Match calculations, it is possible for an air parcel to match when its cluster members are separated by 2400 km (i.e., ˜22 deg. of latitude or ˜33 deg. of longitude at 50 deg. latitude). We suspect that the separation of these cluster members reflects the uncertainty in the trajectory itself.

****We have seen that on average, the difference in sunlight exposure is about 2 +/- 2 hours for members of a matched cluster. For many parcels, particularly those matched in February and March, such small changes do not have a large impact on ozone loss rates. However, in January, when a number of parcels are exposed to very little

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sunlight, such differences can lead to large changes in ozone loss rates for individual matches. In response to this line of reasoning, we performed an additional sensitivity study using the minimum and maximum sunlight exposures seen by match cluster members. In February and March, there is no detectable impact on the mean loss rates. In January, however, loss rates decrease by about 0.5 ppbv/sunlit hour when switching form minimum to maximum sunlight exposure.

Also, since ozone correlates well with PV, a systematic drift of the trajectories in PV space would lead to a systematic ozone change along the trajectories during darkness. With Match we have demonstrated that this is not the case.

****Again, we have now also performed the bivariate regression. See our response to Referee #2.

Another argument put forward by Morris et al. is, that the filtering of the matches could favor trajectories that have systematically seen more or less sunlight than the vortex average conditions. I want to note that such an effect would not have an impact on studies that compare Match results with ozone loss rates calculated by a box model that runs along the Match trajectories (e.g. Becker et al., 1998; 2000; Rex et al., 2003). Also, we have checked whether our sampling has such a systematic bias and found that this is not the case, as can be seen in Figure 9 of Rex et al. (2002). In conclusion we do not think that Figure 7 or the green lines in Figures 8, 9, and 10 are a realistic representation of the systematic error that is connected with Match. Furthermore, these lines represent the impact of only one possible systematic effect (and we think they largely overestimate this effect) but on the other hand ignore all other possible biases that have been discussed by us in the past.

****We certainly did not mean to ignore the previous work of Markus and have added a section to the paper listing the systematic effects discussed in his previous. However, we disagree with his conclusion about our error bars. I do not want to go into a detailed discussion of the trajectory mapping approach here, partly because it is not fully described in the paper. But there is one aspect which I feel is important and. The figure http://www.markusrex.de/Figure_for_comment_on_Morris_etal.pdf shows one example of a highly divergent cluster of trajectories from our Match analysis. In our analysis we define a match as a situation where all trajectories of the cluster are close to the second ozone measurement within certain limits. I.e. we make sure the concept of an "air parcel" applies to the situation and that the whole air mass is close to the second measurement. We are convinced that Match does not work in a situation as the one shown in the figure. In this situation mixing occurs and will have a large impact on the results - the whole concept of an undisturbed "air parcel" fails. Furthermore the accuracy of the trajectories in the figure will be much worse than in a situation were the cluster stays closely together. We feel that the cluster divergence filter is an indispensable part of a proper Match study, a notion that is supported by the independent work of Gross et al. (2002). The trajectory mapping approach is also based on large clusters of trajectories that are initialized close to the first ozonesonde measurement. But in this approach a match is defined as a situation where any of the individual trajectories comes close to a second measurement, no matter where the other trajectories are at that time. This approach favors highly divergent clusters. The chance to make many matches is much larger for clusters where the trajectories disperse rapidly. The situation in the figure would have resulted in two matches, one from the trajectory that comes close to Reykjavik (RE), and another from a trajectory that approaches Moshiri (MO). I am really concerned that this approach favors situations where mixing has a large impact on the results and where the trajectory calculations are not reliable.

****While Markus' points are well taken, we feel that the trajectory mapping approach properly accounts for the inclusion of “weak” and “strong” matches. By a “weak” match, we mean one in which the trajectories are highly divergent. In such a case, fewer parcels will match between the pair of sondes, and the statistical weight given in computing the loss rates will be correspondingly low. By a “strong” match, we mean one in which the trajectories remain well confined and sample the same space. In such a case, more parcels will match between the pair of sondes, and the statistical weight

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given in computing the loss rates will be correspondingly high.

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