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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.

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A quite comprehensive and helpful discussion of this paper has already been submitted by the Referees. Nevertheless I want to add some aspects that have not been mentioned yet. Of course this comment is not meant as a comprehensive review of the paper, which would need to be much longer. I will mainly focus on a discussion of some conclusions of the paper. Before coming to my comments I want to mention that the paper discusses some aspects of work in which I have been involved. So my view on this issue may be biased. I also want to mention that Gary Morris and I have been working together very well on resolving most on the initial large discrepancies that were present between earlier versions of the Morris et al. version of Match and our version of Match. The discussions were always very stimulating and helpful and I want to thank Gary for putting so much energy and effort into this. Although our views on some aspects of the paper may differ, I still feel the work is helpful and makes a valuable contribution to a critical discussion of Match results. Overall this discussion will help to further a thorough and critical review of the technique and will contribute to a responsible use of the Match approach.

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.

First I want to address the statistical uncertainties. Overall we made tremendous progress over the past years on this issue and I have the impression that this is not reflected in the wording of the paper. When the Morris et al. approach was first presented in Palermo in summer 2000, the estimated statistical uncertainties were about a factor of 4-5 larger than the estimates in our version of Match. The bold conclusions drawn from this striking discrepancy was, that the uncertainty of the Match approach is much larger than indicated by our published estimates. However, it soon turned out that part of the reason was that 99% confidence intervals from the Morris et al. approach were compared with one sigma uncertainties from Match (it is clearly stated in all our publications that the error bars in figures from Match represent one sigma statistical uncertainties). This alone explained a factor of three differences in the numbers. Over the next years we worked together on details of the Morris et al. approach, which further reduced the uncertainties significantly. The results presented in fall 2003 in Orlando were much more encouraging. At that time the uncertainty estimates of the Morris et al. work were only about 50% larger than our estimates. The Morris et al. uncertainties were now based on a bootstrap approach. Estimates from a standard regression analysis, which they also showed, agreed well with our estimates from Match. However, the conclusion of the presentation remained unchanged and stated that the uncertainties of Match are much larger than our estimates. But before submission of the present paper it turned out that the estimates from the bootstrap approach were by

ACPD

4, S2118–S2125, 2004

Interactive Comment

a factor of 1.4 too large. The figures of the paper were changed accordingly and now the results from the bootstrap approach agree very well with those from the standard regression analysis (c.f. Figures 8,10 and 11 of the paper), with the exception of very few points on the very left hand side of Figure 8. Furthermore, both estimates of the statistical uncertainty now agree very well with our results. While the uncertainty estimates in the Morris et al. work became smaller and smaller, the text of the paper did not change much and it still concludes that the uncertainties of Match are much larger than published by us. At this point I feel that the wording of the conclusion is no longer in line with the figures and the actual agreement between the uncertainties derived by Morris et al. and our estimates.

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. 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). 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

ACPD

4, S2118–S2125, 2004

approaches.

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. 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 airmasses. 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.

ACPD

4, S2118–S2125, 2004

Interactive Comment

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. 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. 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.

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.markus-rex.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 trajecto-

ACPD

4, S2118–S2125, 2004

Interactive Comment

ries 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.

Finally I want to mention one aspect of the statistical uncertainties that so far has been neglected by us but also by Morris et al. When the density of measurements is very high, it is more likely that an individual measurement is used in more than one match. In this case the individual match events are not completely statistically independent, which has some implications for the uncertainty estimates. Depending on the morphology of the sample of matches, the uncertainties can be smaller or larger than estimated by the standard approach. But in general the estimated uncertainties become somewhat larger when the effect is taken into account. We have a paper in preparation (Lehmann, et al, in preparation) that discusses the impact of this effect in detail. For ozonesonde match analyses the impact on the error bars is very limited. However, for satellite match analyses, where the density of measurements is typically very large, it is important to take this effect into account.

Some specific comments on individual parts of the paper:

page 4666, line 10: Our filters exclude only about 30-50% of the matches, not 99% as stated here.

page 4666, line 11: The finding that most of the filters are not necessary and would not improve the results is in contradiction to our findings that all the filters do reduce the impact of potential dynamical effects. It is also in contradiction to the independent work of Gross et al. (2002), who showed that the trajectory divergence criterion is indeed important to reduce the uncertainty of the results and most importantly also to eliminate a bias due to mixing effects. Since this filter is not used in the Morris et al. work, differences between our results and the Morris et al. results could be due to this bias in the latter analysis.

page 4674, line 22: I do not get the meaning of the sentence: "Each tracked air parcel is only permitted a single match with each other sonde on a given day, ..." Of course each air mass can only match once with any other sonde - not only on a given day, but in general. How could any individual air parcel match twice with the same sonde?

Figure 9 and black dots in Figure 8: using only half of the data in each subsample will increase the uncertainty of the results from the subsamples by a factor of 1.4 (when the uncertainty of the individual match events is constant, the uncertainty of the fitted slope depends on the square root of the number of match events in the sample). Hence, the scatter of the black dots overestimates the real uncertainty of a fit that is based on the complete data set.

page 4696, line 8: "..., although our data seem systematically to reveal less ozone loss than the data from Rex et al. (1998)." Looking at the figure I cannot see the basis for this statement. For me the only difference, if any, is that the Morris et al. point for sunlit times below 10 hours seems to suggest some ozone production during darkness, which I would not call "less ozone loss". I doubt that this slight ozone increase is significant. But if anything it would suggest that some systematic effect tends to increase ozone during darkness along the Morris et al. trajectories. This could be further explored by using a bivariate regression to separate ozone change during darkness from ozone change during sunlit times. Any significant change of ozone during darkness would suggest the presence of a systematic bias in the analysis. In our version of

ACPD

4, S2118–S2125, 2004

Interactive Comment

Match the change of ozone during darkness is virtually zero. In principle a significant ozone change during darkness could arise from an artificial systematic drift of the trajectories in PV or theta space (on top of the small natural drift due to diabatic effects), or from mixing effects. The latter could be an issue in the Morris et al. work because they do not filter out trajectory clusters that are highly divergent. Gross et al. (2002) showed that this is important to avoid air masses that were impacted by mixing.

For the references, please see Comment #2.

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ACPD

4, S2118–S2125, 2004

Interactive Comment