Atmos. Chem. Phys. Discuss., 4, S1971–S1979, 2004
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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.

Anonymous Referee #2

Received and published: 5 October 2004

Comments on the Match analysis of the winters 1991/1992 and 1999/2000

Preface

This comment constitutes the main part of my review. Comments regarding the "trajectory mapping" technique introduced in this paper have been submitted earlier as a separate discussion thread. A few technical problems with some of the figures that I had commented on earlier are resolved by now.

General remarks

This paper introduces an independent formulation of the Match technique and applies this new formulation to two Arctic winters. The Match technique has been introduced and described in a series of papers by von der Gathen, Rex and Schulz et al., but all these papers come from the same group. As important conclusions have been reported using Match results, a notable example being the 'early January ozone loss problem', an independent formulation of the technique is a very valuable contribution. Therefore, I recommend that this paper should be published in ACP.

However, I have several points of criticism that should be taken into account into a revised version of the paper. While detailed comments follow below, the major points are:

- The arguments in the paper regarding the necessity of certain Match filters are based on the sensitivity studies summarised in Figs. 3.-5. As this issue is important for the paper, a more objective, more quantitative criterion than a threshold up to which the loss rates are "well behaved" would be desirable (see also detailed comments below).

- Figure 9 is important in the arguments presented in the paper regarding the correct uncertainty estimate for Match results. However, *one* randomly chosen example is not helpful. What if, by accident, the randomly chosen example would have shown an excellent agreement between the red and the black line? Would our conclusions then be different? In my opinion, we need a probability density function of the distribution of the 50% subsets around the mean that is shown in the black line.

- An important conclusion from this paper is that there are significantly larger uncertainties in Match than reported in previous papers. Nonetheless, error estimates are reported here from the Morris version of Match (± 0.2) that are rather small and are very close to those reported for the original Match analysis – this issue needs to be resolved. Furthermore, more discussion is needed how the various error estimates should be interpreted. For example, if the "green lines" are taken as the correct error estimate, often (and in particular for the large ozone loss rate periods) hardly any information is left in the Match results.

- 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.

Finally, this paper that is concerned with two winters will not be able to answer all questions regarding the uncertainties in Match and regarding differences between the Morris and the classic version of Match. Further insights could be gained by studying the winter 1994/1995, for which a particularly extensive Match data set is available and that would allow a detailed study of the Match deduced loss rates in dependence of altitude. Moreover, for the classic version of Match, it was demonstrated that (almost) zero ozone loss is deduced for warm Arctic winters. It would be important to demonstrate this feature as well for the Morris version of Match.

Detailed comments

abstract, l. 20. "less loss" or a smaller loss rate?

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

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.

section 2.1 Regarding the accuracy of the ozone sondes, there should be more recent information than that in the papers cited. It might be worth to take into account the

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results of recent WMO/JOSI intercomparison campaigns. This information might be relevant as manufactures and standard operating procedures for ECC sondes have changed in recent years. The citation of Reid et al., 1996, is on tropospheric ozone and should probably be removed.

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.

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.

p. 4676, l. 23 “Sun on the horizon”: taking into account refraction or not?

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?

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.

p. 4678, l. 6,7. Is the diabatic correction applied to both PV and Theta or just to 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?

p. 4678, l. 17. How is the criterion (12.5%) selected? How sensitive are the results to this criterion?

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.

Fig2: The greatest discrepancies between the Morris and the Rex version of Match in Fig. 2 appear for sunlit time less than 20. The red points showing some ozone loss seem to be more in line with theoretical expectations than the blue points showing no loss and even some ozone production. The authors should comment on this issue.

Section 3.1, Fig. 3: In my opinion, a more quantitative criterion for the cut-off value than “well behaved” would be desirable. Clearly, for the cut-off value greater than 40%, the standard deviation increases substantially. However, for the year 2000, for cut-off at 25% the ozone loss rate is somewhat less than 2 ppb/sunlit hr whereas for a cut-off at 40%, the rate is about 1 ppb/sunlit hr. Why is this not considered to be significant?

Section 3.2, Fig. 4: Similar as in section 3.1, in Fig. 3, it is not clear what the precise criterion is that is used to determine the spreading cut-off. And, again similar as in section 3.1, Fig. 3, it seems that the standard deviation is considered the most relevant quantity. But why is the variation of the loss rate itself not considered relevant. For example, choosing 4250 km as a cut-off, one would obtain an overall ozone production in the polar vortex in the winter 2000. Would the authors be prepared to defend such a result? Based on Figure 4, my impression is that the results for the ozone loss rate are becoming arbitrary for spreading greater than $\approx$ 2000 km. Judging from Fig. 4, I would suggest a spreading cut-off of $\approx$ 800 km. Moreover, the authors note some discrepancy between their results and those of Grooss and Mueller (2003). Perhaps these discrepancies could be due to the strong variation of the average ozone loss rate both with increasing PV differences and with increasing parcel spreading rather than the points discussed in the manuscript?

p. 4682, l. 26 Is this gradient really stronger in the NH than in the SH?

p. 4683, l. 5-10 Would it make sense to show a calculation that reproduces the Rex vortex definition exactly?

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?

p. 4684, l. 5 State exactly by which filtering criteria the sonde pairs are eliminated.

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?

p. 4684, l. 11 The ΔPV filter means 40% cut-off, correct?

p. 4684, l. 20 "Very little" should be quantified; e.g., is it less than half a degree?

p. 4685, l. 9 Would systematic differences be expected?

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.

p. 4685, l. 27 Add "our version of Match" here.

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.

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 $\pm$ 20 K?

p. 4687, l. 9 I do not believe that this figure is really helpful. What can we learn from *one* randomly selected example about the statistics of a particular data set? And the authors will never be able to demonstrate that Figure 9 is indeed the result of a pure random selection, rather than being chosen from a set of certain examples. Rather than this one example, I suggest to show the probability density function of the ozone loss rate deduced from all 50% sets describing how likely a certain deviation from the loss rate deduced from the full data set is.

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?

p. 4687, l. 23 Why 200? Is this number of subsets large enough for a large number of data points? If 200 is enough, the results should not change if the number is increased to e.g. 300 – has this been tested? And perhaps more importantly: If one would calculate the loss rates for all (rather than 200) 50% subsets, and then compute the average over all the 50% subsets, the result should be identical to computing the average of the complete data set once. So I cannot see the advantage of doing the subset calculation.

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.!

p. 4688, l. 25-29. The question of forcing the fit through zero is a potentially important point. It should be discussed in more detail. It would be of interest to learn a) by how much the uncertainty estimates would increase if the fit through zero would not be employed and b) if calculating the slope by standard routines would introduce a bias in the loss rate calculation.

p. 4689, l. 15. Of course, another possibility would be that one of the methods has a bias that can be explained and corrected.

p. 4689, l. 19. Never before ore never thereafter?

p. 4689, l. 20. Perhaps one should cite a paper like Becker et al. or Rex et al. (2003) 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.

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

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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.

p. 4693, l. 3. Integrated loss: over which time period?

p. 4693, l. 5. Which models? Give some examples?

p. 4696, l. 7. The agreement is not excellent for the first two points.

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.

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.

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.

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.

p. 4697, l. 28. “may not be representative”: this may be true, however at this point is is a speculation for which there is no direct evidence. Am I wrong?

p. 4697, l. 4. “significantly larger uncertainties”: but in this section somewhat above, error estimates are given from the Morris version of Match (± 0.2) that are rather small and are very close to those reported for the original Match analysis. Is there no contradiction here?

A few technical issues with the references

One reference (Santee et al., 2000) is missing, the correct abbreviation for MWR is *Mon. Wea. Rev.*, I believe. My understanding is that AGU requires the citation of *both* the article number and the 'doi'. There is one citation (on p. 4667) to Schoeberl et al., (1991) that is not in the reference list. Finally, I found a few spelling errors of names that a spell checker would probably not detect; the correct names are: Kyrö, Müller, O'Neill, and Schoeberl.

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