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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The ASSET intercomparison of ozone analyses: method and first results" *by* A. J. Geer et al.

A. J. Geer et al.

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Reviewer 1 challenges both the need for this paper to be published in a journal, and its scientific basis. In the first part of this response, we answer these general criticisms. In the second half we explain how we will modify the paper to deal with the more specific criticisms from the reviewer.

Should this paper be published in a journal? The referee suggests it is of "possible interest" to data assimilators only. It is true that it is most relevant to the data assimilation community. For them it should be very useful in helping to build better chemical data assimilation systems. Additionally, the users of gridded ozone datasets should be made aware of the quality of the data they use; this paper provides an initial quantification. We believe that data assimilators should be allowed to share their results via a refereed journal.

The reviewer says that "no distinguising differences are found" between the systems we test. It is true that a main finding of the paper is that a number of very different assimilation systems produce, in the presence of MIPAS data, ozone analyses of generally similar quality. We believe this is a new and useful result. But there are also areas where the systems diverge, though in most of these areas at least one system does compare well to the independent data. In general, these more succesful systems have modelled the ozone more accurately. This means that currently, it is more important to improve the modelling of ozone in data assimilation systems than it is to refine the assimilation technique (whether of CTM, GCM or different data assimilation algorithms). Beyond these broad conclusions, the paper finds and details a large number of specific areas where certain techniques appear to be superior to others.

Were these results predictable from the available literature? The reviewer points out that there are many previous results from the ozone modelling community. But new systems must be validated independently; it is dangerous to assume that what "should" happen does happen. Moreover, the output of a data assimilation system is quite different from that of a model. A useful way to think of this is in terms of the timescales of the different components of the system. With an instrument like MIPAS, observations are repeated on timescales of order a day. The timescales of transport errors are often much longer. While transport errors are important in long-term runs of CTMs, they are much less important if good observations are regularly available to correct them. However, if the timescale of ozone chemistry is very fast, as it is in the upper stratosphere, then even the observations can start to become irrelevant, and the system becomes dominated by the chemistry. Data assimilation systems need to be separately validated. Results from modelling studies, though very valuable, do not necessarily apply equally to data assimilation systems.

There are some cases where the results are "obvious" - no one could expect a very good representation of mesospheric ozone in a system that cannot model the diurnal cycle. However, both data assimilators and the users of their datasets can benefit

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from knowing exactly how large these errors are, and at what levels they begin to be significant.

Many other results in this paper are not "obvious". As one example, who would be able to say in advance that the Cariolle scheme's simplified representation of heterogeneous ozone depletion would, in a data assimilation system incorporating good quality observations, produce results similar to those from a much more detailed chemical model? To compare ozone DA systems has not been done before, but is an important exercise that has provided useful conclusions and has enabled many of the participants to improve their systems. There is a clear need to understand the best strategies for doing ozone data assimilation. We need to do this by looking at the complete system and not just at models.

We now come to the scientific basis of this paper. The best way to look for subtle effects, such as the difference between 4D-Var and 3D-Var, is in well-controlled experiments that change one element of a single data assimilation system. We already say this in the introduction to the paper. Many such well-controlled experiments are described in the literature. But instead, this study is an intercomparison, and intercomparisons between models, such as the AMIP project (Gates et al., 1990, BAMS) and GRIPS (Pawson et al., 2000, BAMS) have been very useful in improving GCMs. We can do the same with assimilation systems, though there are very many more factors that can affect the comparison. Which of these factors can be controlled, and which should be allowed to vary? Our discussion with reviewer 2 examines this issue in more depth. GRIPS was a less tightly controlled intercomparison than AMIP but still produced useful results (See the Pawson article in the July 2006 SPARC newsletter). The controls in this intercomparison, though relaxed, were still sufficient to allow us to make conclusions in many areas.

Our main measure of "success" was to look at the mean and standard deviations of the departures from independent observations. The reviewer appears to be very critical of this. An alternative, exemplified by Austin et al. (2003, ACP), is to look at a selected

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group of metrics or process-based statistics. However, ozone data assimilation systems typically produce output from the surface to the top of the atmosphere, even if in places like the troposphere they have little skill. The comparison to observations throughout the atmosphere enables a comprehensive and rigorous first assessment, and highlights the areas that need further investigation and more focused studies.

The use of a common grid is criticised. Section 4.1 of the paper is devoted to understanding the errors in this technique. We have covered this in more detail in our discussion with reviewer 2. We have quantified the errors of this approach, we see that they are relatively small, and that they do not significantly affect the conclusions.

We hope to add some of the above discussion to the introduction of the paper, so as to better explain the purpose and context of the intercomparison, and the choices we made.

We now address the reviewer's more specific points in the order they are made:

1) "there is no sense of controlled experimentation" - as explained above, and in the introduction to the paper, the best way to do controlled experiments is within a single system, not many. That is not the point of this paper; by its nature an intercomparison is going to be less well controlled.

2) Use of a common grid - We believe that we understand the errors of the approach, that they are relatively small, and that they do not significantly affect the conclusions. Section 4.1 of the paper covers this, and we will add some tests of the sensitivity to the grid's vertical resolution to the revised paper. These show that the vertical interpolations we did had no significant effect on our conclusions.

3) "No consideration of the quality of the wind fields" - Section 5.7 on the tropical tropopause is one example where we do consider the quality of the wind fields. The introduction to the paper could certainly benefit from a parapgraph on the importance of the wind fields, and a mention of results such as those of Scheoberl et al. (2003, JGR)

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and Strahan and Polansky (2006, ACP). But it does not necessarily follow that wind errors that affect long-term model integrations will also affect ozone analyses, where regular insertion of observational data acts to correct, if not totally eliminate, such problems. Our own experience is that the wind fields are of high importance to ozone analyses in the lower and mid stratosphere, and of little relevance at higher levels, where the very fast chemistry simply takes over the system. It would be appreciated if the reviewer could state which papers that evaluate chemical data assimilation systems find otherwise. We will add this discussion to the introduction of the paper.

4) "Background errors should reflect wind errors as the lid is approached" .. "specification of background error seems ad hoc". The specification of background errors is known to be one of the most difficult parts of any data assimilation system; we agree with the reviewer that this is an area of concern. Section 2.9 is there in the paper to point this out and shows the varied and often ad-hoc approaches used in current chemical data assimilation systems, approaches that are shared by most of the references that we cite. The ECMWF system shows very small background error standard deviations near the model top. In contrast to most of the systems, ozone background errors in the ECMWF system are not ad-hoc but are produced using the same method (calculating the covariance of an ensemble of analyses) that works successfully for their operational weather forecasts. The reduction in such errors near the lid is almost certainly down to the strong control of the system by the linearised chemistry scheme. Biases (such as those we find in the ECMWF analyses at these levels and that are likely due to the linearised chemistry) cannot be represented by background error covariances, and neither is it appropriate to treat them as such. We will add a few sentences summarising this discussion, and comment on the poor quality of current background error formulations in the conclusion, but any more than that is beyond the scope of the paper.

5) "Attribution of errors in the upper stratosphere to the linear chemistry does not follow" - the reviewer is exactly right that the fast timescales of chemistry should anchor the

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analyses, and that the majority of the problems we find are errors in implementation of the linear chemistry schemes. This is shown in section 5.5 by a set of controlled experiments in the DARC/Met Office system. A second paper in now under review on ACPD (another Geer et al., 2006) examining these experiments, and others, in a lot more detail. It is very clear that in the systems with linearised chemistry, timescales of chemistry are so fast in the upper stratosphere that the system is almost totally controlled by the chemistry parametrization. Wind errors and observational increments are irrelevant in these areas. As seen in the new ACPD paper, temperatures (and their errors) do influence the linearised chemistry schemes. However, the biggest problems we find in the ASSET intercomparison are due simply to errors in implementation (the DARC/Met Office analyses) or to known deficiencies (the KNMI analyses using the LINOZ scheme). We will make these points clearer in the paper.

6) "there is no reason to expect the troposphere to be credibly represented" and "these systems do not incorporate information from total column observations" - We will make it clear in the introduction that, of course, we do not expect the troposphere to be well represented, and that total column observations are supposed to improve things. But it is still legitimate to investigate quite how badly the troposphere is represented. Also, some MIPAS observations go down to 400hPa, and at least in the UTLS, the detailed dynamics represented in a high-resolution NWP assimilation system might be able to represent the strat-trop exchanges. The results show that below 400hPa it would be much better to use climatology ozone but in the UTLS some of the assimilation systems do have some skill.

7) "the unusual result where the system that assimilates SBUV has the largest variances with respect to TOMS". This system is the operational ECMWF analyses. Dethof (2003a) compared in more detail the ECMWF system with and without MIPAS. The poor performance of the operational "limited GOME plus SBUV" version was ascribed to poor vertical attribution of the ozone increments. The lowest SBUV layer spans from 16hPa to the surface (ECMWF combine neighbouring Umkehr levels). Due

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to the background error specification, the increments were wrongly placed in the lower stratosphere. The difference between the ECMWF analyses with and without MIPAS is crucial in showing how important the MIPAS data is to these analysis systems. We will try to make these points clearer in the paper.

8) The Juckes method - Juckes' paper on this (2006, ACP) explains in more depth the equivalence to the Kalman Smoother. As the manuscript explains, the method does not advect background error covariances but performs an equivalent calculation: equivalent in the sense of solving the same optimisation problem and hence obtaining the same analysis. The direct inversion technique avoids the need to explicitly represent background error covariances. We share the reviewer's belief that this has not been achieved elsewhere: as explained in this manuscript and in Juckes (2006) it has only been implemented for a highly simplified model: isentropic advection with no vertical advection and no chemistry. The lack of a reference to the Kalman Smoother was a mistake and will be rectified.

9) "It does not serve anyone well to state that results are .. good most of the time" - The reviewer has a point: it is mainly in the areas where things "should" be easy that the errors are small. However, in most of the areas where systems show divergent results (excluding the troposphere) at least one system does agree well with the independent observations. Since most systems share the same good quality assimilated MIPAS observations, it is in general the better modelling of ozone chemistry and transport that distinguishes the better systems. This point does not yet come across well in the abstract or conclusions. We will alter them to state the "success" in these terms.

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