

Interactive
Comment

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

Anonymous Referee #1

Received and published: 6 August 2006

This paper is of possible interest to practitioners of data assimilation who are engaged in the construction of assimilation systems. It is appropriate for a project report or a technical memorandum. There is little new, and the scientific method is weak.

The paper presents an overview of results from many ozone data assimilation systems. Some use offline chemistry transport models (CTMs); some are online with numerical weather prediction (NWP) systems. No distinguishing differences are found between the two types of systems. Across the array of systems there are many treatments of the wind, background error specification, and statistical analysis computation. Again, no distinguishing characteristics are found. There is no sense of controlled experimentation that would be needed to identify the likely subtle differences between these schemes. It is like the authors are searching for some combination that might be dra-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

matically better than all the others. This reveals a lack of consideration of the results from the existing literature, including some by the present authors.

With regards to the evaluation technique used here, the notion of interpolating these results to a common grid then linking that grid to the observations stands in opposition to one of the strengths of assimilation, mapping to observation space. The common grid introduces an extra source of error. Mapping directly to observation locations is natural, and perhaps a minimal standard.

Major problems:

There is no consideration of the quality of the wind fields. Of particular concern is the fact that there is a lid at ~ 0.1 hPa in all of the dynamical models used to provide wind fields. There is extensive literature that shows this low lid profoundly influences stratospheric ozone in both assimilation and free running models. A significant discussion of wind errors over both the assimilation cycle and the long time average is required. (Perhaps temperature as well?)

The inattention to the wind errors reflects to all aspects of the paper. The background errors should reflect the wind errors as the lid is approached. Further in the background errors, the errors seem to be getting larger where the winds are getting better. This is counterintuitive. The specification of background errors seems ad hoc. Discussion is required.

The winds and background error issues carry forward to any attribution of cause and effect. For instance, the attribution of errors in the upper stratosphere to the linear chemistry, simply, does not follow. In general the linear chemistry should anchor the analysis at this altitude because of the fast time scales of relaxation. If it does not, then it suggests either errors in the implementation or, possibly, errors in the temperature field or errors in the combined representation of dynamics and chemistry. (Also, if there is an ozone thickness dependency, what is happening at the upper boundary?)

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Similarly, there is no reason to expect the troposphere to be credibly represented. There is no vertical column information to help partition the ozone burden (is there?). There is no representation of surface sources. Plus the chemical scheme's accuracy lessens in the troposphere. Convective transport is important, and (I think) ignored. Yet the discussion of the results is carried to the ground. Any success must be either luck or wired into the model. The variability in the troposphere shows there is no real skill - which is what would be expected. This should be stated; the discussion and figures add nothing.

Finally, while the authors reference many of the previous works in the field, they do not incorporate the conclusions of that work into their considerations. Most notably, many of the systems in the current paper use only MIPAS data, an excellent data set. They do not incorporate any information about total column. Previous studies show that some total column information is important to the quality of the assimilation. Without the total column information, the system is totally reliant on the model information below the profile, and this is simply not credibly represented in the troposphere. If there is the expectation that the information below the MIPAS profile is of geophysical interest, then this should be defended at length. The expectation of the ozone community (and I think the assimilation community as well) would be that the tropospheric information would come, primarily, from the residual of the total column and MIPAS partial column. Hence, credible tropospheric ozone (which this is not) without this source of information would be a major breakthrough, and would require extensive discussion and validation. The plots, however, show very large biases with respect to TOMS data, plus there is the unusual result where the product which uses the SBUV data has, perhaps, the largest variances relative to TOMS. The high variance, relative to TOMS, of the system using SBUV suggests a significant problem; SBUV and TOMS are very similar in their total column and other papers show quite good agreement when SBUV is assimilated.

In total, I am disappointed that there is little or no consideration of the extensive knowledge of ozone transport and chemistry. I am equally disappointed in the way that

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

assimilation systems seem to be cobbled together in a way to get a picture that has reasonable planetary wave structure as the primary metric of “success.”

Minor comments:

There are a number of minor comments throughout the paper. There are two I want to point out.

The description of the Jukes method states “This is equivalent to solving the Kalman Smoother with fully advected background error covariances.” Is this really true? I did not think anyone could really specify the background error covariances, and I know of no numerical scheme that propagates the variance and covariance with much robustness. And if it does, the results from the Jukes scheme suggest this offers no advantage. (Further, I don’t think you have referenced the Kalman Smoother. The Todling paper?)

Beginning of the Results Section and the Conclusions. It really does not serve anyone well to make the statement that throughout most of the stratosphere the assimilation systems compare well with independent observations. It is better to state what the comparison is. You state essentially that the comparison is good most of the time. But from a geophysical point of view, it is only good where the problem is easy, almost trivially easy. Every place that there is a challenge to the observations or the model, the systems diverge, and the comparisons suggest that the ozone from the assimilation is of interest primarily to those who study the assimilation of ozone, not those who study the transport and chemistry of ozone.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 4495, 2006.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper