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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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

Anonymous Referee #2

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This article describes the outcome of the first comprehensive intercomparison of ozone analysis systems. Intercomparisons of models (Pawson et al. 2000, Austin et al. 2003) have been useful for determining regions where models can be improved, e.g. polar processes, tropical transport, mesosphere, etc. However, intercomparisons of assimilation systems are expected to be considerably more difficult. Assimilated products depend not only on the measurements (quantity, quality) and the model used, but also on the assimilation scheme employed and its inputs (observation and forecast error biases and covariances). Thus a strict difference between assimilated products and measurements involves errors from models, measurements and forecasts. Attributing differences in assimilation products from different systems to models or measurements or assimilation schemes can be difficult. Thus, the authors are to be commended for

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undertaking this first attempt at this daunting task. Invariably, with such a large collaborative effort, compromises must be made if results are to be obtained in a reasonable length of time. Thus, strict control on each system was relaxed. In the future, based on these results, a methodology for analysis intercomparisons may emerge. Thus, my comments focus on minor clarifications of the preliminary results and on extracting "lessons learned" for future similar exercises.

General comments

1. p. 4500, para. 3: All analyses are first interpolated to a common grid before comparing with measurements. As the authors indicated in numerous places (e.g. p. 4523, para. 3), the interpolation can include significant errors. This is particularly the case with vertical interpolation and when variables change drastically in value with height (as with many species). Therefore, it would be useful if the authors would discuss this point. For example, why was this route taken (presumably for expediency)? Can or should it be avoided in future exercises? Are some models disadvantaged through the choice of common grid? Why not compare against measurements in observation space? E.g. in equation (1), there is no need to introduce a common grid.

2. p. 4510, lines 22-25: "Here, instead of using the many different formats of O-F produced by individual systems, we simply compare the common-gridded analysis products to MIPAS..." This question is related to the one above: why aren't O-F's compared instead? This would avoid the interpolation error of going to the common grid and then back to the measurement space?

3. p. 4512, para. 1: Would it be possible in this or future intercomparison exercises to use exactly the same data by having all systems skip the quality control step? This would reduce one further difference between the systems. Of course, the effects of variational quality control cannot be avoided, but at least the systems could all start with the same observational data set.

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4. section 6: Many of the conclusions of the intercomparison exercise highlight model deficiencies (in the troposphere, mesosphere and winter polar regions). However, wouldn't a model-alone intercomparison (which could have been much simpler) have revealed the same problems? e.g. Austin et al. (2003, ACP) highlight model deficiencies in polar regions, and the lack of model chemistry in the troposphere and mesosphere for many systems make these results predictable. Therefore, what additional benefits are obtained through analysis intercomparisons? Also, what is the additional benefit over assimilation experiments done with a single system? A paragraph discussing the pros and cons of analysis intercomparisons, based on your experience, with recommendations for future such exercises, could be very useful.

Specific comments

1. Table 1: Because an assimilation system is limited in what it can represent by its model's resolution, it would be very useful if you would indicate approximate resolution (horizontal and vertical) in this table.

2. Abstract, last paragraph: "Using the analyses as a transfer standard..." This phrase is used many time throughout the article. Please define what this means upon the first usage in the text.

3. p. 4511, para. 1: Why are there fewer MIPAS profiles in the 0-10 degree latitude band for all months, in Fig. 4?

4. p. 4513, lines 10-12: "Most of these capture a small bulge in ozone but do not capture the full strength of what is likely a laminar intrusion of stratospheric air." Why do the analyses not capture these? The vertical resolution of most analyses is coarse compared to sondes so will not be capable of resolving structure finer

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than the grid. In addition, some systems (ECMWF, DARC) use vertical correlations which will further smooth vertical structure. However, what explains the lack of structure of BASCOE analyses which use 4D-Var and no vertical correlations?

5. p. 4515, lines 11-13: “All vertical interpolations were done linearly in ln(P)...” It should be noted that this type of interpolation can introduce a bias where the field being interpolated has extrema. This problem was mentioned for the case of DARC analyses, but is likely affecting all analyses.

6. p. 4516-7, section 4.1: Was the sensitivity to the vertical resolution of the common grid tested? This could be rather important.

7. p. 4519, para. 1: “These are likely explained by biases between the MIPAS temperatures...and the ECMWF temperatures...” Why not use MIPAS temperatures in the vertical transformation to pressure levels? If ECMWF temperatures are used, aren’t the results favorably biased toward ECMWF analyses? Would comparison in observation space avoid this problem?

8. p. 4519, para. 3: The number of observations in the southern hemisphere is very small so relative bias between the NH and SH may not be significant. However, results suggest a bias between SCIAMACHY profiles and column measurements, or a difference in the treatment of these two observation types by the data assimilation system, since the model and assimilation systems are presumably identical.

9. p. 4520, lines 15-16: “...the tropical stratosphere, analyses do little better, or even worse, than climatology.” What is the explanation for this? Where transport is important, standard deviations of analyses are better than climatology, but in the tropics where transport is not so important, do analysis errors make the standard deviations worse than climatology? Does this mean it is better to not

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assimilate data in this region? Or should model errors be reduced in this region, in assimilation schemes?

10. p. 4525, para. 4: In the discussion of Fig. 23, it is noted that the analyses do better when the tropospheric ozone is replaced by climatology. However, why do ECMWF, DARC and MOCAGE benefit the most by this improvement? Is it only an issue of poor tropospheric chemistry modelling, or does the data assimilation worsen results in the troposphere? This question arises because, perhaps co-incidentally, both ECMWF and DARC use vertical correlations which could erroneous move ozone from the stratosphere to the troposhere (although MOCAGE does not). Finally, why does the correlation worsen at 20 degrees latitude for ECMWF when the tropospheric ozone is improved?

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