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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.

**A. J. Geer et al.**

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We thank the reviewer for some very constructive suggestions. In a prior interactive comment, we responded to the general comments of reviewer 2: why did we follow particular strategies, and what were the lessons learnt? We hope to incorporate some of that response in the paper. This note describes how we will deal with individual comments.

General comments:

1) Discuss the use of a common grid:

-> We have tested the sensitivity of our statistics to vertical interpolation onto the common grid. The sensitivity is minor except around the tropopause, and particularly the tropical tropopause. The results of these tests will be included in section 4.1. In the conclusions we will add a brief discussion of the common grid and our approach in general, its advantages and disadvantages. We will recommend, as the reviewer suggests, that in future comparisons are made in observation space, and that interpolation be avoided if at all possible.

2) We calculated MIPAS - analysis statistics from analysis data on the common grid. Why not use the O-F statistics produced by each analysis system?

-> We will change the text at p4510, lines 22-25. The first reason was expediency - it was simply easier than dealing with the plethora of O-F data file formats generated by the individual systems. Second, different systems may well apply different quality control and observation operators, changing the sample of MIPAS observations, and this would affect the statistics. We eliminate this possibility by calculating MIPAS-analysis statistics centrally. Nevertheless, it would still have been more accurate to do the comparisons in observation space. We will mention this in the conclusion - see above. However, though it would be theoretically better to use MIPAS averaging kernels, these are not supplied with the operational MIPAS data. Averaging kernels vary from profile to profile, and it is not sufficient to use "representative" ones.

3) Should we skip the quality control step to standardise the comparison?

-> At p4512 we will mention this as a possibility. However, some QC is still required to remove bad data. Also, it is a moot point whether QC is a part of the assimilation system that we want to intercompare, or whether we want to standardise it.

4) What is the point of analysis intercomparisons when model-alone comparisons (e.g. Austin et al., 2003, ACP) are much simpler but provide overlapping conclusions?

-> We will mention this in the conclusion. A principle result of this intercomparison was to validate and help improve the ozone analyses provided by a number of data assimilation systems. This could only be done through analysis intercomparison. But as discussed in more detail in our other response to reviewer 2, it is clear that model-alone



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intercomparisons are highly useful and much simpler than analysis intercomparisons. Their results also help understand the differences between assimilation systems. However, a data assimilation system is designed to provide a better estimate of the atmospheric state than a model alone. Models should be tested to see if they work within data assimilation systems as well as in their own self-consistent atmospheric state. However, we also know that particularly for long-term processes, such as constituent transport in the Brewer Dobson circulation, DA systems can produce far worse estimates of the true atmospheric state than models on their own. These differences from models, both good and bad, mean it is worth comparing DA systems as a whole. The case of ozone data assimilation is somewhat special, in that no system in this intercomparison couples prognostic ozone to radiation and transport. Hence the NWP systems that assimilate ozone are in many ways little different from CTM-based assimilation systems, where the dynamical forcings are externally supplied. Many results from this intercomparison could thus have been found within the CTM-based DA framework, and indeed from experiments with CTM models alone. Nevertheless, in the future, a full coupling between ozone and dynamics in DA systems will bring further reasons to intercompare them.

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.

-> It would be nice to add a column on the model resolution to table 1 but there is not enough width in the online ACPD format. There's a little more width in the ACP format so if we can we will squeeze another column in. In any case, the model resolution is listed in the text.

Also, it's interesting to note that model horizontal resolution is probably not a limiting factor in the ozone analyses investigated here. Section 4.1 (Fig. 8) shows that ECMWF



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analyses can be reduced from 1.125 by 1.125 degree resolution to 3.75 by 2.5 (and indeed 5 by 3.75; not shown in the paper) without affecting standard deviations against ozone sondes. This suggests that the structures accurately represented by the analyses are of relatively large scale. Whether this is because ozone information is spread and smoothed to large scale through the horizontal background error correlations, or is just because small-scale ozone structure is not analysed or modelled realistically, is not clear.

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.

-> We will define this better on p4522 section 5.3, where the technique is described in detail. On p4511 l18, we will drop the mention of "transfer standard" and simply refer to section 5.3.

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

-> It is a feature of the MIPAS observations that the second profile after a northward equator crossing is normally missing. This is why Fig. 4 shows less data in the 0-10N latitude band. We will mention this.

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 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?

-> We will include the reviewer's explanation. We will see if we can understand the



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reasons why BASCOE produces no representation of this. Possible candidates are the chemistry scheme or the relatively coarse (5 by 3.75 degree) horizontal resolution.

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.

-> We meant the DARC results to be an example of the effect of reduced vertical resolution on the calculation of total column, and we expect this would affect all analyses. We will re-word this paragraph to make it clear. We will also include the reviewer's comments on the problems with the vertical interpolation method.

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

-> The sensitivity of comparing MIPAS to analyses on common grid or original vertical levels has now been tested. ECMWF analyses were chosen as they have the highest vertical resolution. Compared to using full resolution analyses, the common grid increases MIPAS biases versus ECMWF by up to 7% at the tropical tropopause (100hPa) and lowermost stratosphere (68hPa) and midlatitude tropopause (100hPa) but is elsewhere negligible (less than 2%), and does not affect standard deviations at all. We will add the new figure here, and gather together some of the comments on strategies for MIPAS vertical interpolation that are currently scattered around the text.

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?

-> To interpolate to the common grid we need global gridded temperatures and hence



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cannot use MIPAS. However, a comparison in observation space would have allowed the use of MIPAS temperatures, and this would definitely be more consistent. We will mention this in the text. I don't see how the use of ECMWF temperatures would cause a favorable bias to ECMWF analyses in comparison to others, but clearly, as already mentioned in the text, the isentropic analyses are disadvantaged by this additional source of error.

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.

-> On re-examination the comparisons to MIPAS and HALOE (mentioned but not shown in the paper) actually do reveal a similar bias in the SCIAMACHY profile analyses in both SH and NH in the region approximately 22-100hPa. However, as in the sonde comparison figure included in the paper, the bias does not stand out so clearly from the other analyses as it does in the NH. However, this supports the reviewer's criticism. We will modify this paragraph.

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

-> In areas where synoptic (i.e. transport driven) variability is very low, the analysis systems seem to introduce excessive, unrealistic structure or noise into the ozone fields. This could either come from erroneous transport or observational noise, or information spreading from other increments, but it is not clear which. Standard deviations and biases are still very low compared to other regions, suggesting the analyses are relatively good from the point of view of, say, heating rate calculations. However, users of the data should be aware that structure shown in the ozone fields in these areas is likely spurious. We will make this discussion clearer and make a summary at the end of the paragraph.

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 coincidentally, both ECMWF and DARC use vertical correlations which could erroneous move ozone from the stratosphere to the troposphere (although MOCAGE does not). Finally, why does the correlation worsen at 20 degrees latitude for ECMWF when the tropospheric ozone is improved?

-> We will mention the fact that vertical correlations, as well as tropospheric chemistry modelling, could be relevant here. This is an interesting point, but to investigate further would require experiments to vary the background errors in the DARC or ECMWF system. Due to a lack of observational data the ECMWF operational analyses had very poor stratospheric ozone distributions for much of the intercomparison period; they are not even shown in the vertically resolved comparisons in the paper. However, the strange decrease in correlation in Fig. 23 at 20N is probably symptomatic of that.

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