Atmos. Chem. Phys. Discuss., 6, S4157–S4163, 2006
www.atmos-chem-phys-discuss.net/6/S4157/2006/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of linear ozone photochemistry parametrizations in a stratosphere-troposphere data assimilation system" *by* A. J. Geer et al.

A. J. Geer et al.

Received and published: 30 October 2006

Referee #1

General comments

1. Does the short time period affect our conclusions? Why was this particular period chosen?

This period was chosen so we could make use of software, independent observations, and initial conditions from the ASSET intercomparison of ozone analyses. Because we were running an entire NWP data assimilation system for each experiment, we were limited computationally to a period of about six weeks. As discussed elsewhere,

the DA methodology means that slow growing (e.g. monthly or seasonal) errors and behaviour cannot be tested. Such errors are largely irrelevant to DA systems, because assimilation of observations should correct such problems on the order of days. Thus in the mid and lower stratosphere we are more interested in testing the parametrizations under more rapid, i.e. synoptic, variability, such as during the breakdown of the polar vortex. That is why the October period was chosen. We will add this discussion to section 3.2.

As we already mention in the conclusion, it is likely that the parametrizations could generate different biases in seasons other than those tested here. However, the main issues that we discuss in the paper are unlikely to be restricted to a particular season: for example, discrepancies between the different temperature and ozone climatologies could cause problems at any time of year. As another example, the LINOZ (P-L)0 term (above 10hPa) and Cariolle v1.0 radiation (column ozone) coefficients are excessively large in all seasons, compared to the more up-to-date schemes. There would clearly be similar erroneous behaviour in other seasons. We will make this clearer in the conclusion.

2. What are the pros and cons of the DA methodology?

The reviewer is quite correct on the main limitation of the DA methodology: it does not detect slow-growing errors. Also, it involves very costly runs of a data assimilation system (though the cost could be substantially reduced if a CTM-based DA system had been used for these tests). In the conclusion we mention that free-model runs are still essential to test the full range of behaviours of the linearized photochemistry schemes (The "no ozone assimilation" methodology suggested by the reviewer is also useful, so we will now mention that in the conclusion.) However, it is good to evaluate the photochemistry paramterizations within the system in which they will be used, and DA systems have their own problems: As identified and described in the paper, if there is a discrepancy between the ozone climatologies in the linear ozone schemes and those in the analyses, an incorrect ozone forcing is generated. Such a problem would not

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show up in a free model run. We will add some of this discussion to the introduction.

Specific comments

3) "future developments should include ... diurnal cycle above 0.5hPa"

As suggested by the reviewer, we will delete this sentence from the abstract.

4) Explain the cold tracer

We will explain this in section 2.2

5) How "up to date" are the chemical models from which the parametrizations are derived, in terms of species concentrations, photochemical reaction rates and photolysis rates?

We will add some of the following information and extra discussion in section 2.3-2.5:

- Cariolle v2.1 and Chem2D make use of JPL (2002) recommendations, LINOZ uses JPL (1997), and Cariolle v1.0 uses early 1980s reaction rates and photolysis rates.

- Species concentrations: Cariolle v1.0 is based on early 1980s knowledge, but all other schemes are relatively up-to-date. For LINOZ, species concentrations are initialised as described in Avallone and Prather (JGR, 1997), based on early 1990s observations. For Cariolle v2.1, total chlorine is set to year 2000 amounts. For Chem2D, halogen compound amounts in the troposphere are kept fixed, and are set from WMO (2002). With this as a boundary condition, middle atmosphere chemistry was allowed to equilibrate over a 20 year "spin-up" period.

6) Assimilated MIPAS data

We will follow the reviewer's suggestion and move the description of MIPAS data to section 3.1, giving a few more details of how it is assimilated.

The suggested technical corrections will be made (the scale should be 10e-6kg/kg)

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Main comments

1) The paper needs to incorporate model free runs

Our approach already identifies (a) the most obvious differences between the parametrizations (applicable to free models as well as data assimilation systems), and (b) some of the pitfalls of their implentation in data assimilation systems. We acknowledge in the conclusion that an intercomparison based on model free runs would still be of great interest. Such studies would reveal more subtle differences between parametrizations (particularly in the lower stratosphere and in the polar night) than can be identified using the data assimilation approach. However, such long term runs are too expensive to do with the Met Office system, and would instead be best done with a CTM. These experiments would also depend very much on the quality of modelled transport. For example, Monz-Sanz et al (2006, GRL, accepted) show that the age of air calculated by a CTM can be greatly different depending on the meteorological analyses used. It would be difficult to decide whether ozone errors were coming from the linear ozone schemes, or from the transport. Given these complications, we believe that model free runs would require a lot of extra work and should best be done as an entirely separate study.

2) Only a short, six week period is investigated

See response to referee 1.

3) Conclusions are specific to the Met Office system and the MIPAS observations

True, the exact numbers in figure 5-7 are specific to our set-up and we will change the text to make sure that readers are not misled. However, the broad conclusions are relevant to all data assimilation systems. For example, the biases caused by LINOZ above 10hPa were also seen in analyses based on the KNMI CTM with SCIAMACHY total column observations assimilated (Geer et al. 2006, ACPD: The ASSET intercomparison of ozone analyses).

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4) The paper needs a comparison of the cold tracer to the heterogeneous depletion term in the v2.1 Cariolle scheme.

The ASSET intercomparison of ozone analyses (Geer et al. 2006) has already made qualitative conclusions on the different treatments of heterogeneous chemistry.

Specific comments

1) (GMAO reference) We will add a reference to Riishojgaard et al. (2000), which discusses the GMAO system, and identifies problems with the chemistry parametrization in the upper stratosphere.

2) (Sec 2.1 eq 2) We will note that level l is the level under consideration.

3) (p7433) We will improve the text: This analysis is very similar to that of McLinden et al. (2000), who used it to derive an analytic timestepping formulation for LINOZ. However, that analysis ignored the effects of ozone transport on the steady state ozone mixing ratio described by Eq. 5. Here, the steady state is that of a model including both chemistry and transport.

4) (Troposphere) We will state that the parametrizations are not appropriate for the troposphere in section 2 (for the reasons stated by the reviewer). However, it is still of interest to see how badly the troposphere is simulated, because most of the current generation of ozone analysis systems use them in the troposphere regardless.

5) (Cold tracer) Referee 1 also wanted more information on the cold tracer scheme, so we will expand section 2.2. We will reference Hadjinicolaou (1997) for the original concept. Further development at Cambridge produced the version we use, which is described by Eskes et al. (2003).

6) (LINOZ P-L term, p7441) We do not have an explanation for this.

7) (Fig 3) We will point out that the differences indicated by Fig. 3 are large, even for the latest Cariolle v2.1 and Chem2D schemes based on JPL (2002) chemistry. Hence,

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differences must be related back to the different models they are derived from, the way they are derived, or the transport in these models.

8) (Choice of method) See response to Reviewer 1, point 1

9) (Ozone sondes) No selection was applied except that we did not use the Indian sondes.

10) (Fig. 8) This is evidence that the chemistry scheme is dominating over assimilated MIPAS observations at these levels, making ozone amounts closer to HALOE than MIPAS.

11) (Troposphere) See 4 above.

Referee #3

General Comments

1) Upper stratospheric ozone deficit.

There are still uncertainties at these levels in the observations (e.g. between MIPAS and HALOE) but Nataranjan et al. (GRL, 2002) seem to suggest that any remaining chemical deficit is now within the range of observational uncertainty. However, because in the upper stratosphere, linearised photochemistry schemes act to relax ozone to a steady state close to the chosen ozone climatology (eq. 5 and following discussion), even if there were such a deficit reflected in the (P-L)0 term, it would be largely irrelevant. We will add this discussion to section 2.1.1.

2a) Use of a different climatology in LINOZ.

Yes, there is a certain linearisation state (i.e. chi_0,LINOZ) at which the LINOZ (P-L)0 ozone tendency coefficient was calculated. If a different climatology is used (i.e. chi_0,KNMI), the error in (P-L)0 will be approximately E = (d(P-L)/dChi|0)*(chi_0,LINOZ - chi_0,KNMI). If we were to correct for this error in Equation 5, it would become: chi_SS = chi_0,KNMI + [(P-L)0 - E + A]*tau. As in the answer to the previous comment,

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there would be little change to the steady state in the upper stratosphere, where tau is small. As we conclude in the paper, it is more important that the chosen climatology (i.e. chi_0,KNMI) should be accurate and consistent with the assimilated observations.

The use of Fortuin & Kelder climatology is also consistent with the use of LINOZ by KNMI (Eskes et al. 2003). We will explain this in section 2.4.

2b) There is an orders of magnitude difference between the LINOZ (P-L)0 term and other schemes. How come this results in only a 20-40% drop in ozone in the upper stratosphere?

Again this can be explained by equation 5 and following discussion. The relaxation to climatology dominates over the erroneous (P-L)0 at such levels. We will explain this better in the paper in relation to the discussion of upper stratospheric errors in LINOZ.

3) Short time period

See reply to Ref. 1, comment 1

Specific points

4) (Absolute values in Fig. 3.) We will make this clearer in the text

5) This is not relevant if absolute

6) We will include a similar figure showing the global mean tau values for each parametrization. However, it would be confusing to have these on the same figure.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 7427, 2006.