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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.

Anonymous Referee #2

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Ozone photo-chemistry parametrizations have been used in the past by a number of CTMs and GCMs. With the parametrization approach one avoids the extra burden of modelling o(50) tracers in the model. Especially in combination with an assimilation of satellite ozone data such an approach can be quite successful. Recently several new parametrizations have become available but the performance of these parametrizations has not been inter-compared in a systematic and detailed way. Given the popularity of the ozone parametrization approach such a comparison is very timely and would form a useful reference for the GCM/CTM modelling and assimilation communities. The paper provides a few clear conclusions regarding the behaviour and shortcomings of the individual parametrizations. Nevertheless, it fails in providing a more complete

overview.

I am not in favour of publication of the paper in it's present form, for the following main reasons:

1) For a good insight in the performance of the parametrizations it is essential that free model runs are presented together with the assimilation experiments. These runs can then be compared with HALOE and sondes. How much do the ozone distributions obtained from long runs differ from each other and from observations? These runs are completely missing, and the information that is obtained from the wok presented is thereby limited.

2) The authors present results for a short period of 6 weeks only, focussing on the period that the ozone hole is well established. The parametrizations are provided as a set of coefficients for each month separately. At least a one-year model simulation is needed to map the seasonal cycle obtained with the parametrizations! Arguably one may even claim that multi-year simulations are needed to account for inter-annual variability (e.g. statospheric warming events).

3) It is interesting to see how the chemistry parametrizations behave when used in an assimilation set-up, and this experiment is well described in the paper. However, it should be realised that the conclusions drawn are quite specific for the MIPAS assimilation with the Met Office model. In this case there is much vertical information available to constrain the lower and middle stratosphere, and the system is not very sensitive to the chemistry scheme chosen (related to the long chemistry relaxation time at this altitude range). I can imagine this would be very different if e.g. TOMS ozone columns or SBUV profiles are assimilated ! The fact that the results are assimilation set-up specific makes them less important. The results presented in fig 5-7 may be misleading for readers and a careful explanation of these results is important. Quoted numbers for the differences in ozone between HALOE/MIPAS/sonde and the MIPAS assimilation system with different parametrizations can not directly be interpreted as a bias of the chemistry scheme.

4) I would have liked to see an inter-comparison of the cold tracer vs. the Cariolle extra

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heterogeneous term.

The paper is well written and can become an interesting reference when the above points are addressed explicitly in a major revision. I realise that an assimilation run for one year or longer may be very time consuming. For discussing the impact of the chemistry schemes on the assimilation system I suggest that this is not really needed, and the main features are well captured by the 6-week assimilation experiment. However, simulations should be performed for at least a one year period. The authors mention the paper by Cariolle and Teyssedre (in preparation). I am not aware of the content of this paper: are free model runs with different schemes and comparisons with independent data sets covered in this paper?

Additional specific comments:

intro:
NASA DAO (GMAO) has many years of experience with ozone assimilation with/without chemistry parametrization schemes. A reference to this work would be appropriate.

sec 2.1:
- eq 2: What is level "l"? Does the integral include or exclude the level under consideration.
- p 7433: "Note that Eqs. (5) and (7) differ from the treatment given in McLinden et al. (2000), because here, the effects of transport are included". I did not understand this remark. Please explain.
- The chemistry schemes are typically not developed to describe the troposphere! A latitude-altitude parametrization is not suitable here, given the very distinct longitude-latitude distribution of precursor emissions. I suggest the discussion is restricted to stratosphere only (including tropopause region perhaps). The limitations of a parametrization of the form discussed applied to the troposphere should be clearly stated in sec 2.

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sec 2.2
- I believe the cold tracer approach was developed in Cambridge. A citation of the original work is more appropriate than the reference to Eskes and co-workers.

sec 2.6
- p 7441: Is there an explanation for the large (P-L) term in LINOZ?
- It struck me that the differences between the schemes is very large !? (fig 3) Is this related to inherent chemistry uncertainties, implementation aspects or perhaps even implementation errors in deriving the coefficients?

sec 3 (method)
- Why did the authors choose a full data assimilation system with MIPAS observations? Why do the authors focus on a month with a fully developed ozone hole?

sec 3.3.2
- Did the authors apply any selection of ozone sondes, possibly based on maximum altitude or total ozone ?

sec 4 (results)
- fig 8: The analysis minus HALOE seems to give better results that the analysis minus MIPAS for certain altitude ranges. This is a bit surprising, given that MIPAS is assimilated.

sec 4.3 (troposphere)
- Why is this section relevant? As discussed before a Cariolle-type parametrization is constructed specifically for the stratosphere. The troposphere is characterised by very inhomogeneous sources and sinks, and at least one should incorporate longitude as additional dimension controlling the rates and climatologies. Models used to derive coefficients are also not meant to describe tropospheric chemistry.

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