Atmos. Chem. Phys. Discuss., 6, S3188–S3191, 2006
www.atmos-chem-phys-discuss.net/6/S3188/2006/

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.

Anonymous Referee #1

Received and published: 19 September 2006

Linearized ozone photochemistry parameterizations have been used successfully in a number of different contexts related to stratospheric modelling. If one is well aware of the inherent limitations of a linearized chemistry scheme then such a scheme offers the advantage of being computationally cheap and conceptionally less complex compared to a fully coupled chemistry scheme. One example where a linearized ozone chemistry makes sense is the assimilation of ozone observations into a meteorological data assimilation system. A number of different linearized ozone chemistry schemes have been proposed over recent years and their usefulness has been demonstrated. What was missing, however, was an evaluation of the different schemes and a thorough test of their performance. Geer and coworkers now present such an evaluation of a set

of different schemes in a data assimilation framework. Their paper is in general well written and will be a useful reference for anyone working with linearized ozone parameterizations. It is already in a relatively good shape and I recommend publication in ACP after some (mostly minor) changes and corrections.

General comments

1. The model experiments presented here were run over a period of only six weeks between 23 September and 5 November 2003. This seems to be quite short, given the long photochemical lifetime of ozone in the lower stratosphere. Moreover, the results thus only apply to a very specific season (actually much less than a season) where the Antarctic ozone hole has already developed and at the transition from autumn to winter in the northern hemisphere. I would have thought that one should perform such a comparison over at least one year to capture processes such as the photochemical decrease of ozone in mid to high latitudes between spring and autumn and to see whether there is any difference in the performance in different seasons. While it is probably not feasible to extent the paper in this aspect, the authors should at least include some discussion how the short analysis period potentially influences their conclusions.

2. One point I still haven't fully understood is how the assimilation of MIPAS data helps in the evaluation of the different linearized ozone chemistry schemes. As stated on p. 7450, in the mid and lower stratosphere the assimilation of ozone effectively masks biases in the photochemistry schemes. Would it have been possible (or maybe even better) to evaluate the linearized ozone schemes in a system without ozone assimilation? Over a period of six weeks the ozone field should not run away too much from the initialization. So I believe it would be helpful if the benefits as well as the limitations of the chosen methodology were explained clearly with a bit more detail in the introduction. (Currently, the introduction just states that a free running model could evolve to a state different from reality, but it does not discuss more precisely the benefits and limitations of the ozone assimilation.)

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Specific comments

Abstract, p. 7428, l. 24: "Future developments should include ... a parameterization of the diurnal cycle of ozone above 0.5 hPa." I could not find much discussion in the rest of the paper on the effect of the diurnal cycle that would justify to include this statement in the abstract. In fact the comparisons with independent data were not shown above 0.5 hPa. So I suggest to remove this statement from the abstract or to include something in the paper that substantiates this statement.

Section 2.2: Please explain briefly the cold tracer. In particular as I expect this will take only one or two sentences anyway.

Sections 2.3 to 2.5: Ozone photochemistry of course depends on the atmospheric loading of ozone depleting substances such as chlorine and bromine but also odd nitrogen and water vapour. I.e. a linearized photochemistry scheme that is based on the pre-ozone hole atmospheric state (e.g., 1970s halogen loading) will probably give very different results than a scheme that is based on the 1990s atmospheric state. Very little is said about the assumptions being used for the different linearized ozone schemes. Giving this information would help to qualify any differences. On a similar issue: The older schemes are based on old and now outdated photochemical reaction rate constants and photolysis cross sections. One would hope that more recent schemes based on more recent photochemical data will show a better agreement with observations. However, this is not necessarily true. I feel that it would be good to include some statements in the discussion of how "up to date" the individual schemes are, to better judge if the better schemes perform right for the right reason.

Section 3.1: Please give some details on the MIPAS data being assimilated. I assume this is largely identical to the discussion of MIPAS data as used for the validation described in 3.3.1? However, I could not find a clear statement over what altitude range MIPAS is assimilated, how many profiles (per day?) are assimilated and things like that. Suggestion: Move part of section 3.3.1 to 3.1 or 3.2 and then when discussing

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MIPAS data as a validation data set just refer to the sections above.

Technical corrections

p.7434, l.1: Don't introduce "Practical considerations" as a sub-subsection if there is no other sub-subsection to subsection 2.1.

p.7452, l.19: "influence modelled temperatures" -> "influence on modelled temperatures"

Figure 9: The colour bar gives ozone between 6 and 16 kg/kg. I guess this should be mg/kg?

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