Atmos. Chem. Phys. Discuss., 6, S3398–S3400, 2006
www.atmos-chem-phys-discuss.net/6/S3398/2006/

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 #3

Received and published: 28 September 2006

Geer et al. have presented a comparison of several different linear, chemical stratospheric ozone schemes which are based on pre-calculated coefficients. These schemes are generally able to reproduce the main features of the stratospheric ozone field and how it responds to changes in local ozone, temperature, and overhead ozone column. The authors have utilized these schemes in a data assimilation model in which they assimilate MIPAS ozone data over a six period. The resulting ozone generally compares favourably with sonde and HALOE profiles suggesting these schemes are suitable for this purpose.

This paper is well written and the content is sufficiently new and relevant to merit pub-

lication pending a response to the issues raised below.

General issues:

1. Upper stratospheric ozone deficit. I thought that chemical transport models still were unable to correctly simulate upper stratospheric ozone - the so called ozone deficit problem - where models are 10-15% too low at 40 km (see e.g., Natarajan et al., GRL, v29, p. 56-1, 2002). There is not evidence of this is your comparisons (the Linoz model notwithstanding). Should this problem not also arise in these linearized schemes which are based on the same rate data as the full CTMs. Please explain.

2. So why was a different ozone climatology used when using the LINOZ scheme? Does the Linoz scheme not include its own self-consistent ozone climatology? Surely this will introduce an artificial bias since each scheme is linearized about a particular point, and switching climatologies intriducses a shift in the tendency of A(chi_o,Linoz - chi_o,KNMI), where A is the partial derivative. Similar argurments if the temperature climatology is changed. One would assume that this bias will be most noticeable in the upper stratosphere where chemical time scales are short compared to dynamical ones. Could this be a cause for some of the differences? Also, are the orders of magnitude differences in the Linoz (P-L)_o (Fig 3a) compatible with only the 20-40% low bias in the ozone (fig 5). According to equations (5)-(7) should the ozone in the upper stratosphere not be hugely different as well, even with the assimilation helping it towards MIPAS? Other free-running models like the original Linoz paper do not see biases that are worse. Please explain.

3. The experiments were performed for 6 weeks. Is this all that has been done; or is this the only period being reported on? Would other periods give similar results, given the unique nature of this period? Was this suppose to be a more stringent test than, for example, summer.

Specific points:

ACPD
6, S3398–S3400, 2006

EGU

Section 2.6, line 26: you plotted absolute values in Figure 3?

Figure 3: perhaps denote + and - portions with different line styles

Figure 3: You should all another panel in Figure 3 showing the corresponding tau (chemical relaxation times) values

---

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