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Atmospheric Chemistry and Physics Discussions

Interactive comment on "CHEM2D-OPP: A new linearized gas-phase ozone photochemistry parameterization for high-altitude NWP and climate models" *by* J. P. McCormack et al.

Anonymous Referee #2

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General Comments

This paper provides a very good description of the performance of the CHEM2D-OPP linearized ozone photochemistry scheme. I particularly liked the way in which the theoretical basis of the linearization approach was discussed (Section 2.1), something that has been lacking in previous linearized ozone photochemistry papers. The diagnosis in Section 4.1 of problems in predicted ozone at high latitudes, which showed that these were related to differences between climatological and local temperature, was also well thought out and presented. Overall, it is a good study that merits publication.

I think this paper can become a benchmark to which future linearized ozone photo-

chemistry parametrization papers will refer to. In recognition of this, it would therefore be fitting to extend the scope of the discussion (chiefly in the Conclusions section, but also in the Introduction) to consider what role linearized ozone photochemistry schemes will fill in NWP/DA systems in the future. Cariolle and Deque's now famous paper appeared 20 years ago and computer power has increased significantly in that time. Is it reasonable to assume that the way forward is to provide ever more sophisticated versions of linearized schemes (as suggested by the authors in the Conclusions), or will the linearized approach be soon superseded by more sophisticated, but still computationally fast, approaches (eg Taylor and Bourqui, 2005 QJRMS) that have already been implemented in climate GCMs? I think discussing these points would be a valuable additional contribution to the paper.

Furthermore, the linearized scheme is really only suitable for the stratosphere. The authors mention in the Conclusions section ways in which performance in the mesosphere can be improved, but what about the troposphere? Increasing demand for air quality forecasts means that the accurate representation of tropospheric ozone is very important. It would be good if the authors added that to the discussion, too.

Specific comments:

p 6643 l27 - p6644 l1: More discussion of the "cold start" is needed. Is there an issue with "spin-up" or "spin-down" of ozone and other fields, as evidenced by time series of these fields through the duration of the 6 day hindcast? If so, this should be mentioned. And why are only 135 and 138 hour hindcasts shown in Figs 12-15? Is is because the the hindcasts hadn't spun up before that time?

p 6644, l17-20: The text says that initial ozone fields revert to zonally and diurnally averaged monthly mean values above 0.4 hPa, but it's not clear if this is also done at those levels during the hindcast with the tendencies from the linearized scheme. If this is the case, it would be good to clarify it in the text. Also, in McCormack et al (2004) it says "NOGAPS-ALPHA applies the photochemistry scheme up 1 hPa, then smoothly

ACPD
6, S2450–S2452, 2006

EGU

relaxes to 2-D climatological ozone mixing ratios in the mesosphere". If something radically different from this is done here, it would be good to explain why.

p 6649-6652: I like the way that the analysis of the results has focused on individual EOSMLS profiles (Figs 12-15). However, it would be good to get a grasp of how well the CHEM2D-OPP scheme is performing at other latitudes, or on other dates, in order to get a grasp of how representative the results presented are of the scheme's performance as a whole. A few sentences summarising this should be added. Reference to other papers or reports is sufficient; further analysis of the existing results is not necessarily required.

p 6658 l 2-6: Figures 20 and 21 show excessive ozone depletion in the southern polar lower stratosphere in July and insufficient depletion in October. Are there issues with model transport or the absence of a parametrization of heterogeneous ozone loss? This should be discussed.

Minor comments:

p6646, footnote: I see that the Geer et al paper has now been accepted by ACPD - ACPD, Vol 6, 7427-7469

p6668, l 17: "Thpaut" should be "Thepaut"

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6627, 2006.