Atmos. Chem. Phys. Discuss., 8, S6806–S6809, 2008
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Parameterization of middle atmospheric water vapor photochemistry for high-altitude NWP and data assimilation" *by* J. P. McCormack et al.

Anonymous Referee #2

Received and published: 8 September 2008

This paper describes a linearised parametrization of water vapour photochemistry in the stratosphere and mesosphere that follows similar principles to those used in ozone chemistry parametrizations (e.g. Cariolle and Deque, JGR, 1986). The new parametrization has a latitude and seasonal dependence that should make it an improvement over schemes with fixed coefficients such as the one used in the ECMWF system. As such, this is a useful paper and deserves to be published in ACP, although a number of points need to be addressed:

Main points

1) The equivalence between $(k_1 + k_2)^{-1}$ (the timescale for the ECMWF scheme) and

$\tau*$ (that of CHEM2D-H2O) is alluded to in two places (p. 14005 and p. 14009) but it is never properly explained how this arises. I would like to see a much fuller discussion of this, as I think it would help explain better some points that confused me and would likely confuse others. As far as I can see, the authors have to start by assuming that the rate of change of H2O in the CHEM2D model must be described by:

$$\frac{d[H2O]}{dt} = \frac{-sum(H2Olossrates)}{[H2O]_0} * [H2O] + 2 * \frac{sum(CH4lossrates)}{[CH4]_0} * \frac{(Q-[H20])}{2} \quad (1)$$

Here, Q is as given on l.16, p. 14002, $[H2O]_0$ and $[CH4]_0$ are the CHEM2D values. So that as in the paper,

$$\tau_{H2O} = \frac{[H2O]_0}{(sum(H2Olossrates))}$$

and similarly for τ_{CH4}. If (1) is true, please can that equation be given and explained explicitly in the paper. If it is not, please explain how $\tau*$ is arrived at. In writing (1) it is clear that there is an assumption that all CH4 loss instantly produces 2 H2O atoms, and that there are no other H2O production processes. However, I don't believe those assumptions hold in the meosphere. Are these assumptions being made in the mesosphere? If so, the authors need to quantify the errors in doing this. While this is mentioned on line 7 p14008, this sentence does not give enough justification in my view.

2) Both the ECMWF scheme and CHEM2D-H2O have a tunable parameter that can be adjusted to produce a better mean H2O state. In CHEM2D-H2O, this is r_0, and in ECMWF it is r_Q. In comparisons with the ECMWF scheme, CHEM2D-H2O gets the benefit of a tuned r_0 in EXP2, but no attempt is made to tune r_Q. Thus it is possible the ECMWF scheme is being disadvantaged. This should be mentioned in the conclusions. However, since r_0 is a function of time, latitude, and height, but r_Q is applied universally, this ability to tune the scheme more precisely may well be an advantage of CHEM2D-H2O. Again this should be mentioned.

Minor points

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1) The introduction needs to contain an overview of the history of linearised ozone parametrizations, as these provide the starting point for the current work . However, I would like to a little more detail (perhaps 4-8 lines) and a few more references on this subject than found in the limited discussion there is currently, and which only occurs later (l.24-25, p14008).

2) l.17, p. 14002 - please say whether this Q = 2[CH4] + [H2O] assumption is made, implicitly or explcitly, in CHEM2D-H2O in the mesosphere as well (see main point 1).

3) Figure 4 and discussion. Please explain why r_Q in CHEM2D is lower in the lower stratopshere and peaks towards the upper stratosphere.

4) Figure 5 and discussion. Why is τ_{CH4} omitted from the graph above 10^{-1} hPa? Since it is part of the CHEM2D-H2O parametrization at all levels of the atmosphere, it should be shown at all levels.

5) p 14008, l.3-5 - "giving a horizontal grid spacing" - I don't think there is a direct link between the spectral formulation and the Gaussian grid, which could each be chosen independently, so I would not imply that with the word "giving".

6) p 14008, l.7 - please better justify this assumption in the mesosphere (see main point 1)

7) p 14009 l.1 "Applying the expansion".. instead, for clarity I would suggest writing "Expanding the ECMWF relation eq. 2 as a Taylor series, it is straightforward..."

8) p 14009 l.8 "the CHEM2D-H2O analog..." - please expand and explain this better (see main point 1)

9) p14010 l.23 - the climatology based on NOGAPS-ALPHA. It needs to be made very clear that NOGAPS-ALPHA in this context is (presumably) something separate from the NOGAPS-ALPHA forecast model simulations EXP 1, 2, 3 (p.14011 l.8). On first reading you get the feeling there is some kind of chicken and egg scenario going on.

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Interactive Comment

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10) p14011 l.11 and the title of Fig. 8a seem to imply that the MLS/HALOE climatology IS r_0, when really r_0 can be either climatology or NOGAPS-ALPHA analyses. Please reword and change the figure title.

11) p14013 l.21 "into" needs to be capitalised.

12) p14015 l.14-29 aand Figure 12. Why is EXP1 used, rather than EXP2? Surely EXP2 would be expected to produce better results.

13) p14016 l.6 "..the parametrization is valid up to the lower thermospheric altitudes.." Surely this relies on the asumption mentioned in main point 1?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 13999, 2008.