Atmos. Chem. Phys. Discuss., 7, S824–S825, 2007
www.atmos-chem-phys-discuss.net/7/S824/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A revised linear ozone photochemistry parameterization for use in transport and general circulation models: multi-annual simulations" *by* D. Cariolle and H. Teyssèdre

Anonymous Referee #3

Received and published: 28 March 2007

An interesting paper that summarises the start-of-the-art of parameterised ozone schemes for stratospheric applications. The first author was a leading force in establishing those schemes and updates his classic scheme in this paper. The paper is well written and should be published with only some small changes necessary.

p1657, l27: I am slightly confused, Oikonomou and O'Neill (2006) validate ERA-40 as-is (and not the IFS), whereas Dethof and Holm (2004) are really looking into the details of ozone data assimilation in ERA-40. In addition, there must be a multitude of other models using variations of CD86 ...

p1660, l28: "lower" should read "smaller"

p1661, l12-13: The statement about "global models" (meaning CCM, I guess) being similar should be substantiated or dropped.

p1664, l28: This means MOBIDIC with full chemistry runs once to produce Clx fields and afterwards the same version of MOBIDIC runs but the ozone tendencies from the full chemistry are adjusted using equation 2 (or is the adjustment done "on the fly")? I think a little more explanation is needed.

p1668, l12: I think this statement is hard to substantiate, given that MOCAGE runs on a reduced T42 grid, which is quite crude compared to the IFS grid. A small comment with respect to differences in horizontal resolutions should be included.

Overall I find section 3.1 a little "jumpy" changing between NH and SH and back to NH again. It obviously establishes the NH ozone overestimate in spring, which is afterwards explained in section 3.2. This brings me to the only small concern I have in terms of conclusions. Using v2a the ozone overestimate is established. The authors rightly cite the literature highlighting too fast overturning in ECMWF data products. I assume the authors are using the analysis and not the 6 hourly forecast which may alleviate the problem? In addition I wonder what the role of the chemistry parameterisation is? In figure 18 the authors show a 20DU difference between v2b and v2a. So the effect of the too fast overturning might not be as strong as implied by section 3.2 (obviously I don't know how figure 7 would look for v2b).

The final conclusion is very optimistic and I guess the jury is still out there to decide if the simple nature of such parameterisations allows transient climate runs or should be used for sensitivity studies only. Maybe the authors could add a small comment.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1655, 2007.

ACPD

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