**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Implementation issues in chemistry and transport models" *by* S. E. Strahan and B. C. Polansky
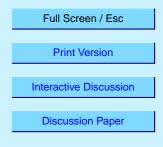
Anonymous Referee #2

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GENERAL:

I agree very much with the idea of the paper to use numerically relatively cheap models to perform extensive sensitivity studies to understand the dependence of models results on parameters that cannot be tested in large GCMs. The paper is focusing on model resolution and model domain. It is discussed under which circumstances resolution and the location of the model lid have an influence on the quality of the reproduction of transport barriers and, in turn, on the simulated chemical fields.

I have a few issues that should be addressed in a revised version of the paper. I believe that this is possible and that a revised version will be accepted for publication in ACP.

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* It is good that the model results are being compared to satellite data. However the resolution of such data is often lower than those of the employed models. This issue is completely neglected in the paper as it stands now. E.g., Khosrawi et al., ACP, 2005, have recently reported that different model setups were required in their model for a successful simulation of satellite and in-situ data.

* There are several publications that suggest that the vertical and horizontal resolution of a model should not be independently chosen. Observations suggest that the ratio between horizontal and vertical resol. should be about 250. This issue should not be neglected in the paper.

* There is a large number of Figures. While I agree that this is necessary in principle, I suggest that the authors try to reduce the number of figures somewhat.

SPECIFIC:

title: The title should be changed: Implementation of what? (see likewise Sec. 3 and 3.1)

section 4: There is a lot of discussion here of the differences between the two models – but which one is closer to reality.

P. 10227: There are many qualitative comparisons here. I suggest to be more quantitative, i.e. give a percentage rather than saying "faster". Similar on page 10229 and 10230.

section 5.4: Possibly, the small improvement of the high res. run is caused by the fact that the vertical resolution has not been increased at the same time. Could this be the case? (See General comment above). Further, how much is the conclusion that a 2 x 2.5 resolution is sufficient, dependent on the employed transport scheme. How would this number alter if a more diffusive is used? The dependence of the resolution on the employed transport scheme should furthermore be clearly stated in the abstract.

DETAILS:

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p. 10220, l. 7: at what altitude?

p. 10221, l. 15: what is the difference between "quasi-Lagrangian" and semi-Lagrangian here?

p. 10222, l. 1: which aspects?

p. 10222, l. 25: Give a reference stating which scheme is used.

p. 10224, l. 14: remove "that"

p. 10228, l. 24: insert "that"

There are a number of papers that are currently not cited but might be useful: The issue of the permeability of the vortex edge in models is treated in Sankey and Shepherd (JGR, 2003), Polar ozone simulations from a variety of models are reported in Austin et al. (ACP, 2003), v. Aalst et al (ACP, 2005) have reported simulation results from a GCM in a nudged (i.e. CTM like) mode. Finally, this paper clearly is relevant for the recent initiative (CCMval) to validate chemistry climate models and the links to this initiative (Eyring et al., BAMS, 2005) should perhaps be included.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 10217, 2005.

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