Atmos. Chem. Phys. Discuss., 6, S3027–S3032, 2006
www.atmos-chem-phys-discuss.net/6/S3027/2006/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The atmospheric chemistry general circulation model ECHAM5/MESSy1: consistent simulation of ozone from the surface to the mesosphere" *by* P. Jöckel et al.

P. Jöckel et al.

Received and published: 12 September 2006

We are thankful for the helpful suggestions by referee #3.

General comments:

1. "The weakness of the approach ...": We do not fully agree. Two points have to be stressed. First, the applied nudging is weak so that the self-consistent

model physics is not severely disturbed. And second, the base model (the GCM ECHAM5) has been thoroughly evaluated. The results are available in a recent issue of J. of Climate (see references of the manuscript) and clearly show that present-day climate is well reproduced, including the development of a QBO at a sufficiently high vertical resolution. Thus, the nudging cannot be regarded as ’constraining’ temperature and transport, but rather as a method for an efficient evaluation.

Indeed, our model ECHAM5/MESSy1 was not primarily developed for long term climate runs but as a reference model for key processes in the upper troposphere / lower stratosphere which are most critical for chemistry climate feedbacks. Nevertheless, it is also possible to integrate it for several decades, also without nudging of the tropospheric meteorology. An additional evaluation of the quality of such simulations, however, is beyond the scope of this manuscript. The present simulation will be used as a benchmark for simulations with reduced model complexity. This will be clarified in a revised version.

2. “Moreover, the vertical resolution ...”: This is not correct. During the final development phase (between summer 2005 and spring 2006), we have integrated the model for nearly 5 decades (summed up between all the final development, debugging and testing simulations). Thus, climate simulations with this model configuration are possible, not to mention that further optimisations are in progress, which will speed up the model considerably without changing its complexity. Additionally, the model has been run on machines which are either not longer in the Top 500 list or at its very end. But fundamentally there are 3 scientific arguments which made us decide in favour of the high vertical resolution setup: First, we are very much aware that the representation of advective transport is very sensitive to vertical resolution (Jöckel et al., 2001), therefore we have chosen a high resolution to minimise numerical effects. Second, a prognostic model should represent the key dynamical features of the atmosphere, such as for instance the QBO for

ACPD
6, S3027–S3032, 2006

Interactive Comment

the middle atmosphere. A significant reduction in vertical resolution would not allow us to simulate the QBO as already shown by Giorgetta et al. (2006). And third, the high vertical resolution in the UTLS region leads to a milestone in the quality of the results obtained there. The UTLS is a region which is critical for coupling the lower and middle atmosphere and is sensitive to climate change.

Finally, in the applied setup we used the highest degree of complexity which was available at that time. Our intention is to provide a benchmark for more efficient simulations in which some of the CPU demanding routines may be switched off or reduced (e.g., the comprehensiveness of the chemistry). This possibility is one of the advantages we achieved with the strict modularisation of MESSy. In addition, the model results are available through the internet, so that subsequent model changes can be documented in terms of technical changes and scientific improvements.

3. “Where is the new scientific result?”: We are surprised that referee #3 (apparently a modeller, but perhaps not a model developer) does not consider the successful development of a new model to be a scientific achievement. As such, it is very well worth being documented and published in a peer-reviewed journal and not only in the grey literature. This is very similar to the development of new instrumentation and measurement techniques, which are clearly accepted as needing to be documented in peer-reviewed literature.

   It is one of the fundamental principles in natural sciences that a new development (a theory, an experiment, a numerical model) must first prove being able to reproduce (or falsify) present knowledge, before being applied to extended, prognostic studies. Unfortunately, this fundamental principle (in the same way as another fundamental principle like reproducibility; see our reply to referee #1) may be losing ground in atmospheric chemistry and climate modelling.

   We would like to ask what would be better suited for (absolutely required) model evaluation than in-situ and/or satellite data. To degrade this as ’descriptive’, ’with

ACPD

6, S3027–S3032, 2006

Interactive Comment

essentially no analysis' is not acceptable. In our analysis we show that our model essentially reproduces the state of the atmosphere from the surface to the mesosphere in a highly self-consistent way, and reaches a quality that has - to our knowledge - not been achieved so far.

Nevertheless, we agree that "the paper needs to be much clearer about what its purpose is, and what it does and does not do in this respect given the chosen methodology". We will discuss this as part of the model philosophy more clearly in the first two sections of the revised manuscript.

We further appreciate the two exemplary suggestions on how to proceed, however, detailed analyses of the suggested kind (e.g., a detailed comparison of the nudged with the free running model system) are beyond our present scope, and are in progress for subsequent papers. For the same reason, we are reluctant to add figures, also in view of the critical comments by referee #1.

Indeed, follow-up papers will address the tropospheric influence on the stratosphere, using e.g. the 2002 vortex split as one example. An additional manuscript, which is close to submission, will address the temperature distribution in the tropical lower stratosphere, in connection with the water vapour distribution. This will be another example, showing that the nudged tropospheric and free running stratospheric model excellently reproduces dynamical and radiation processes. A further example of a paper in preparation is a comparison of ECHAM5/MESSy1 results with ECMWF analyses, in which wave fluxes and polar processes will be analysed.

In summary, we very much agree with the referee comments, though we will follow them up in subsequent papers. We agree though that it should be stressed that the present manuscript is the first in a series of publications in a special issue of ACP(D).

ACPD

6, S3027–S3032, 2006

Interactive Comment

Specific comments:

p. 6958, line 6: We agree that the given grid-point resolution refers to the so called 'quadratic Gaussian grid', which is required for aliasing-free transformations of quadratic terms between grid-point and spectral space (and not for accuracy as referee #3 states). Furthermore, we agree that "physics is performed on the quadratic grid", but we do not agree that this "is a waste of time as the information is lost ...". This is not the case for all quantities which are never transformed to spectral representation, such as for instance the distribution of chemical species. For an in depth discussion of the "information content" of spectral models, we refer to Laprise (1992).

p. 6959, line 4: Either statement taken absolutely is wrong. We will improve the explanation of the stratospheric circulation in the introduction.

p. 6959, line 6: A GCM (General Circulation Model) including chemistry is not necessarily a CCM (Chemistry Climate Model). Since we did not apply our model in 'climate mode', as the referee in her/his general comments analysed, we refer to our model as an "Atmospheric Chemistry General Circulation Model" (AC-GCM), to indicate the difference. Nevertheless, as stated above, the model can also be applied in a CCM mode.

p. 6968, lines 10-19: We replied on that topic to referee #1 (see point 5 in our reply). Certainly, we will improve the motivation for the S2 simulation in a revised manuscript.

p. 6973, line 22: The referee asks for a reference to a manuscript which, at the time of our submission, was not publicly available (including potential referees of our manuscript). As soon as it is available, we will include an additional reference in a revised manuscript, even though it is not much different in the points for which we cited Austin et al. (2003).

Figure 5: Here, indeed an explanation is required.

p. 6974,lines 13-15: Instead of the (weak) nudging, also a model deficiency could be responsible. Nevertheless, we will reformulate this sentence.

p. 6991, lines 6-7: The Arctic and Antarctic transport and temperature issue will be discussed in greater depth.

References

[] J. Austin, D. Shindell, S. R. Beagley, C. Brühl, M. Dameris, E. Manzini, T. Nagashima, P. Newman, S. Pawson, G. Pitari, E. Rozanov, C. Schnadt, and T. G. Shepherd. Uncertainties and assessments of chemistry-climate models of the stratosphere. *Atmos. Chem. Phys.*, 3:1–27, 2003.

[] M. A. Giorgetta, E. Manzini, E. Roeckner, M. Esch, and L. Bengtsson. Climatology and forcing of the quasi-biennial oscillation in the MAECHAM5 model. *J. Climate*, 19:3882–3901, 2006.

[] Patrick Jöckel, Rolf von Kuhlmann, Mark G. Lawrence, Benedikt Steil, Carl A. M. Brenninkmeijer, Paul J. Crutzen, Phil J. Rasch, and Brian Eaton. On a fundamental problem in implementing flux-form advection schemes for tracer transport in 3-dimensional general circulation and chemistry transport models. *Q. J. R. Meteorol. Soc.*, 127:1035–1052, 2001.

[] R. Laprise. The resolution of global spectral models. *Bul. Am. Met. Soc.*, 73:1453–1454, 1992.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6957, 2006.

ACPD

6, S3027–S3032, 2006

Interactive Comment