

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.

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We appreciate very much that referee #2 considers the model documentation and evaluation a scientific achievement being worth to be published. Furthermore, she/he also recognises that our results are original and that our study is reproducible. We do, however, not concur that our main sections are “practically independent and weakly connected parts”. Our intention is to present a new whole-atmosphere model which consistently simulates the state of the atmosphere from the surface up to the mesosphere.

We do not understand the difference between a “solid documentation” (which, accord-

ing of referee #2 we should provide) and a “technical documentation” (which, according to referee #2 we did provide). For reproducibility, the technical documentation is essential and needs to contain at least the most important approaches and applied techniques. We have provided this, and gone beyond it by providing several scientifically interesting first results from the model and the ability of current modelling to simulate the troposphere-stratosphere chemical system.

We agree that the aim of the manuscript needs to be more clearly stated.

We do not agree that our numeric experiments are not well thought through and do not support our major conclusions. The base simulation (S1) was carefully designed after nearly 20 sensitivity simulations to debug and determine the best setup to use. The S2 simulation is unusual in that it contains different segments, but each is a sensitivity run in itself from which we can learn. Nevertheless, we will reconsider the formulation of our conclusions in view of the above criticism.

Specific comments:

1. Introduction

paragraph 1: Indeed, the introduction needs to become clearer.

paragraph 2: This paragraph was intended to underline the progress towards a continuously growing degree of complexity which requires new techniques, to keep such models controllable. Increasing complexity, achieved by more and more explicit process descriptions which replace parameterisations, reduce the number of free ‘tuning’ parameters. The overall consistency of the model is thus increasing with complexity.

paragraph 3: To our knowledge, there is currently no other model of similar complexity available, which also applies a strict modularisation as MESSy. The MESSy approach differs fundamentally from the 'coupler' approaches. This has been discussed already in Jöckel et al. (2005) and should not be repeated here.

2. Model description and setup:

paragraph 1: Again, we have problems with the difference between a “technical description” and a “description appropriate for a scientific publication”. One of the central approaches of MESSy is to disentangle processes through independent submodels, which can easily be replaced and tested against alternative schemes. The process description is therefore equivalent to the submodel description. In the special issue we already published 'Technical Notes' on a subset of submodels. These manuscripts contain all relevant details required for reproducibility. Unfortunately, this method of peer-reviewed documentation is not yet well established in the scientific community, since these types of achievements and progress are not always acknowledged as “science”.

paragraph 2: (i) This information is indeed missing. We applied the advection scheme of Lin and Rood (1996) which is part of the ECHAM5 base model.

- (ii) We used a 3rd order Rosenbrock-solver with automatic time-stepping. The applied chemical mechanism is documented in the electronic supplement.
- (iii) The listed tracers are only advected as families, whereby all other tendencies (e.g., chemical tendencies) are calculated for the individual tracers. A Technical Note with the detailed description of the applied family concept is currently in preparation.
- (iv) The same set of tracers and reactions is used consistently from the surface to the mesosphere. The sentence “The set of ordinary differential equa-

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- tions (ODEs) describing the chemical mechanism has been integrated in the entire model domain without artificial or arbitrary boundary or interface conditions.” was meant to indicate this, however, it is required to clarify this.
- (v) The selected chemical mechanism is documented in the electronic supplement.
 - (vi) Stratospheric sulphur is prescribed (see description of HETCHEM).
 - (vii) Indeed there is no difference in the calculation between HETCHEM and PSC. The latter, however, calculates the rates only within the PSC region, and HETCHEM accounts for the rest. This adheres to one of the most important concepts of MESSy, namely, to untangle processes in independent submodels. With this approach, the PSC submodel can be improved or exchanged, without the need to take care of stratospheric processes outside the PSC region.
 - (viii) The calculations of those emissions are based on parameterisations which originally have been developed for a different model (cf. Kerkweg et al., 2006, Ganzeveld et al., 2006, and references therein). The additional scaling factor of 0.6 is required to adapt the parameterisation in order to achieve realistic amounts of isoprene and NO_x from soil.

We will improve the text for these points, thank you for pointing these out.

paragraph 3: The dynamical part of the free running base model (ECHAM5) has been extensively evaluated in a recent special issue of J. of Climate. We did not claim that our analysis proves that the combined model system ECHAM5/MESSy1 is stable and accurate in free running mode. This analysis has still to (and will) be performed, but at this stage it is beyond our scope. Before such studies are feasible, however, it is required to show that the relevant processes are simulated realistically. Application of the weak nudging technique (the model is still ‘almost’ free, especially in critical regions such as the boundary layer and the

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stratosphere) enables to compare results directly with satellite and in-situ observations, and therefore allows an efficient evaluation of processes. The motivation for the S2 simulation needs to be made clearer, indeed (see also our replies to referee #1 and #3, also for the discussion about performance).

3. Meteorology:

paragraph 1: We will consider this as an option to shorten the manuscript.

paragraph 2: This will increase the length of the manuscript even further. Moreover, we tried to evaluate our model results as much as possible with observations. A comparison of middle atmosphere zonal wind with other models (e.g. ECMWF) is less meaningful. Observed temperature and distributions of long-lived species (mostly controlled by dynamics) are more appropriate.

paragraph 3: We do not agree, since we compare both, seasonal averages with several thousand data points (Fig. 5), and snapshots for a very specific situation (vortex split). For both, the nudging technique is essential. Statistical comparison between climate simulations and long term observations is a different issue. We further emphasise that the model simulation covered 8 years in total, and that the statistics agree with HALOE observations.

paragraph 4: Unfortunately, in section 3.2.1 a sentence was messed up and will be corrected. The point on solar radiation pointed out by the referee was intended to be there. We have the comparisons with MIPAS data for both simulations (S1 and S2), but did not want to increase the number of figures even further.

paragraph 5: Tracers like N_2O are often used in the literature to identify transport barriers or the Brewer Dobson Circulation (e.g., Hall et al., 1999, Sparling, 2000).

Comparisons of zonal wind with ERA40 are presented in Giorgetta et al. (2006). Analysis of w^* from ERA40 is too noisy for a useful evaluation, we already looked for that.

paragraph 6: We did a comparison for the 8 year time series of HALOE CH_4 with the same result; more details will be given in a revised manuscript.

paragraph 7: The text will be improved.

paragraph 8: The vortex split occurred in 2 simulations (S1 and S2) with nudging. In a sensitivity experiment with only prescribed sea-surface temperature, it did not occur. We will expand the text.

paragraph 9: We suppose the referee means overestimated. In Fig. 8 we show the worst case, when you look at Fig. 11 the difference is much less. We will be more precise on this.

4. Global ozone distribution and budgets

paragraph 1: We suppose the referee means 10-15% (in S1). We will modify the text accordingly.

paragraph 2: This section was intended to emphasise the high degree of consistency, since not many models are able to provide a realistic ozone distribution / chemistry from the surface to the mesosphere. We will extend the text slightly and perhaps also add references to previous model documentation and evaluation papers that show similar figures to point out the features more carefully.

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5. Tropospheric tracers and chemistry

paragraph 1: Indeed, the cited publications only cover the stated periods, however, the corresponding databases contain also more recent data.

6. Stratospheric tracers and chemistry

paragraph 1: There is a clear need to better compare S1 and S2.

7. Summary and conclusions

This section has probably to be completely rewritten in a revised manuscript.

We agree with all suggested technical corrections.

References

- [] 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.

- [] L. N. Ganzeveld, J. van Aardenne, T. M. Butler, M. G. Lawrence, S. M. Metzger, P. Stier, P. Zimmermann, and J. Lelieveld. Technical note: Anthropogenic and natural offline emissions and the online emissions and dry deposition (EMDEP) submodel of the modular earth submodel system (MESSy). *Atmos. Chem. Phys. Discuss.*, 6:5457–5483, 2006. SRef-ID: 1680-7375/acpd/2006-6-5457.
- [] T. M. Hall, D. W. Waugh, K. A. Boering, and R. A. Plumb. Evaluation of transport in stratospheric models. *J. Geophys. Res.*, 104(D15):18815–18839, 1999.
- [] Patrick Jöckel, Rolf Sander, Astrid Kerkweg, Holger Tost, and Jos Lelieveld. Technical note: The Modular Earth Submodel System (MESSy) - a new approach towards Earth System Modeling. *Atmos. Chem. Phys.*, 5:433–444, 2005. SRef-ID: 1680-7324/acp/2005-5-433.
- [] A. Kerkweg, R. Sander, H. Tost, and P. Jöckel. Technical note: Implementation of prescribed (OFFLEM), calculated (ONLEM), and pseudo-emissions (TNUDGE) of chemical species in the modular earth submodel system (MESSy). *Atmos. Chem. Phys.*, 6:3603–3609, 2006. SRef-ID: 1680-7324/acp/2006-6-3603.
- [] S.-J. Lin and R. Rood. Multi-dimensional flux-form semi-lagrangian transport schemes. *Mon. Weather Rev.*, 124:2046–2070, 1996.
- [] L. C. Sparling. Statistical perspectives on stratospheric transport. *Rev. Geophys.*, 38:417–436, 2000.

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