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Interactive Comment

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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Our manuscript is the first in a series to accumulate into an ACP(D) special issue. It introduces our new model, presents first results, and provides the initial evaluation by comparing with observations. We emphasise that the model couples comprehensive representations of tropospheric and stratospheric processes, and the manuscript intends to highlight some of the outstanding model features. Obviously such a paper is larger than a regular scientific article. Nevertheless, the point is well taken that the manuscript should be shortened, in particular by reducing the number of figures. We will also more clearly formulate the role of the present manuscript in the special issue.

Specific comments:

1. One important aspect is that we introduce a new whole-atmosphere model including comprehensive treatment of chemistry in the troposphere and middle atmosphere. The model can be applied for highly detailed process studies as well as for climate studies with parameterised process descriptions. We are convinced that with our new approaches (modularity, weak nudging for efficient evaluation by direct comparison with observations) a major step in the overall model development towards comprehensive Earth system models is taken. The high complexity and comprehensiveness of the presented model system is a scientific achievement by itself, and for the sake of scientific reproducibility (one of the fundamental aspects of natural sciences) it needs to be documented in the scientific literature in its full complexity. To split the paper into 2 parts would be not useful in this respect, because the present form highlights the coupled nature of the lower-middle atmosphere chemistry-climate model.
   Since the present manuscript introduces this model, and it is already quite comprehensive, it cannot do more than touch upon the many features and present a selection of first results, which all clearly show that the state of the atmosphere from the surface to the mesosphere is overall very well represented by the model. In our evaluation analysis we have hereby largely used observational data in order to benchmark our model with 'reality' and less with previous models.
   More detailed analyses and discussions of pressing scientific questions are in progress for follow-up papers in the special issue.

2. The modularisation of the model is a comprehensive technical improvement,

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needed to improve the organisation of the complex numerical system. It makes the system more flexible for use in different applications, adaptable and portable to different architectures, and less sensitive to programming errors (e.g. the debugging is more straightforward). Modules can be easily exchanged and tested in inter-comparisons. Technical details are provided by Jöckel et al. (2005). We will discuss this more clearly in a revised manuscript.

Again, for scientific reproducibility, we see a need to describe the processes included in the model in sufficient detail. The way to organise such descriptions is to a large extent a matter of taste, and we have chosen the way that corresponds best to the organisation of our model, namely the description of the submodels. Nevertheless, we accept that the description of the modules, though necessary, might be distractive in the main body of the manuscript, and we will check if the description can go into an appendix without creating additional overhead in the main text.

3. Although the nature of our manuscript requires many plots, we will reduce their number. Indeed, not all ozone-sonde data which are available have been shown, but rather a representative subset including those where the agreement between model and observations is less favourable. By showing too few figures, we may create the impression that we have selected only those with 'best agreement'. We will consider to put some figures into the electronic supplement of the revised manuscript.

4. The paper does not really jump between topics, but discusses a number of model aspects in a well reasoned order. Obviously, the order can be changed, though this is very much a question of taste. The chosen order is: meteorology, ozone distributions and budgets, and subsequently the shorter lived tracers and chemistry. Stratospheric and tropospheric aspects need to be addressed in each of these topics.

   The discussion of the 2002 SH warming fits well within the discussion of the

middle atmosphere meteorology, since the satellite observations of the chosen species (in addition to the observed temperature) are a better suited (since more objective) quality control than a comparison of derived quantities that are not directly observed but are rather a combination of model results and observational data (e.g. ERA40 from ECMWF).
We intend to emphasise more strongly that our model can reproduce this type of sudden warming by reproducing the tropospheric meteorology based on the nudging technique (which leaves the stratosphere free) and by consistently computing stratospheric and tropospheric dynamics. Nevertheless, we will consider to move the statements on the short-lived tracers to the stratospheric chemistry section in a revised version, if this does not create additional overhead.

5. The simulation S1 was based on settings for nudging coefficients and gravity waves from the existing literature. The set of sensitivity runs in S2 was introduced to see the effects of the top height for nudging. We found that when we omit nudging in the lowermost stratosphere the gravity wave forcing needs to be reduced to reproduce the Brewer Dobson circulation at high latitudes. S2 with reduced nudging and gravity wave forcing is closer to observations e.g., for ozone distributions in the lower stratosphere and also total ozone. This is a new finding, which will need to be stated much clearer in a revised manuscript. S2 (from March 2003 onward) is also more self-consistent concerning the QBO. Nevertheless, as described in Giorgetta et al. (2006) it is possible that its period and phase can differ from observations after several years of integration. We will explain this more clearly in a revised version.
The S2 simulation therefore is a sensitivity simulation in which the nudging and gravity wave forcing coefficients have been reduced, as described on page 6968. We refer to this on several occasions to indicate where the changes have the most prominent effect. We will describe more clearly in the final section what we have learned from it. Indeed, the overall conclusion is that the S2 setting, in

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which the model has greater freedom to consistently compute the dynamics, produces the most realistic results. This is addressed for example in the section on total ozone, and will be addressed in the conclusions of a revised manuscript. A separate section for the comparison of S1 with S2 (which was originally planned) would require a number of repetitions (since more than a few sections are affected), and therefore would create a large overhead resulting in an even longer manuscript.

6. One of the major achievements with the new model system in the way it was applied for the evaluation simulations is that no longer arbitrary or artificial boundary conditions (for instance at the tropopause or between layers) are required. For instance, some models use different ozone distributions for chemistry in the lower stratosphere and for downward transport. In this case, often an ozone climatology at the tropopause is prescribed to realistically simulate stratosphere-to-troposphere transport. In many cases this covers-up the fact that models have difficulties simulating stratosphere-troposphere exchange.
   We do not prescribe such boundary conditions at the tropopause nor in the lower stratosphere or elsewhere, and consider this to be a major innovation, and we will highlight this aspect in the revised manuscript.

Minor comments:

1. Free running models have the disadvantage that one can only compare statistical quantities with observations, and therefore long time-series of observations and simulations are needed. If the tropospheric meteorology is nudged, it is possible to perform 'point-to-point' comparisons (i.e., at the same time and the same site) of simulated temperatures and chemical species with measurement campaigns or sparse satellite data, a technique mostly used in our analysis. This works for

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the troposphere but also for the whole stratosphere since by the dynamical model the waves from the troposphere are propagated correctly. With this we have done many comparisons with the complete time-series of ENVISAT/MIPAS-data (2002-2004) and UARS/HALOE-data from 1998 to 2005 (temperature, ozone, water vapour, HNO_3, NO_x, HCl, $ClNO_3$ are very close to observations). This will be further shown in the ACP special issue. We will improve this justification of the nudging technique in the introduction.

2. The emissions and boundary conditions have been discussed in separate papers by Kerkweg et al. (2006) describing the method, and by Ganzeveld et al. (2006) describing the inventories. Those papers are part of the special issue. Including these aspects into the present manuscript would further elongate it. Nevertheless, the (required) brief repetition of which tracer is handled how will be moved from the submodel section to the model-setup section in the revised manuscript.

3. We fully agree in this point.

4. This aspect of the discussion will be extended.

5. This indeed needs to be corrected.

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