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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.

Anonymous Referee #2

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General comments

The manuscript describes a new atmospheric chemistry GCM ECHAM5/MESSy1, which includes detailed description of the chemical processes in the troposphere, stratosphere and mesosphere. Documentation and validation of such models are relevant to the scope of ACP. The model and results presented in the manuscript are original. The description of the conducted numerical experiments is clear and other scientists can reproduce them. The manuscript consists of the model description and five practically independent and weakly connected parts: analysis of the hydrological

cycle in the troposphere, comparison of the temperature and several chemical species with MIPAS data for 2002-2004 with emphasis on late September 2002, validation of the tropospheric chemistry, comparison of the QBO and total ozone with available observations. Overall, the present version of the manuscript does not look like solid documentation and validation paper, but rather as a combination of the technical documentation and some screen-shots illustrating the model performance. Moreover, the manuscript is too long, contains too many figures and, therefore, very difficult for readers. The scientific aim of the publication is not clearly formulated. The performed numerical experiments are not well thought and the results are not sufficient to support the interpretations and conclusions, therefore the paper can be recommended for publication only after major revisions.

Specific comments

1. Introduction

The introduction is rather vague. First of all, the authors should clearly define the main scientific goals of the paper. I would also suggest adding a brief review of the previous achievements in the field of CCM development and application. At the moment it reads like only two attempts to simulate ozone depletion and tropospheric chemistry were made. Such addition will help to understand the place of the described model among other CCMs.

I do not really understand the meaning of the second paragraph. Two problems mentioned in the beginning of the paragraph will remain, I think, for any advanced technology in use. Therefore, the third sentence is not a conclusion from the first two, but conveys completely independent idea.

I would suggest adding some historical overview about modular system/couplers and coding standards. It would be interesting whether the described modular system is the first in the world. If not, then what is the difference between the presented system and the others. How does it differ from the system couplers like PRISM and so on. It could

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be an important addition, because modular nature of the proposed system is one of the main focal points of the manuscript.

2. Model description and set-up

I think that the chosen form of the model description is more appropriate for the technical description than for scientific publication. The model description would be more compact and clear if the authors get rid of the module names and concentrate on the description of the processes and applied methods.

From the presented model description I could not find the answers for several questions, which are important for the analysis of the results. In particular, (i) what scheme was used for the advective transport of species? (ii) what chemical solver was used in KPP?; (iii) from the description of the TRACER module I understood that the model transports chemical families, but there is no explanation how these families are treated during the chemical step?;(iv) whether the standard set of 104 species was used from the ground to the mesopause or the number of considered species depends on the altitude? (v) is it true that for the stratosphere only the main chlorine and bromine reactions are considered?; (vi) how the sulfate stratospheric aerosol properties were prescribed or simulated? (vii) what is the difference between reaction rates calculated in HETCHEM and PSC modules and why is it necessary to calculate them twice?; (viii) how to justify artificial 40 percent reduction of the isoprene and soil NO emission, does this reduction help to get reasonable CO distribution?

I have some doubts about the choice of the performed experiments. First of all, the assimilation of the tropospheric meteorological fields cannot provide any information about how stable and accurate is the model in free running mode. It would be ok, if the future applications will exploit “assimilation” mode, but it was mentioned in the introduction that the model can be used for climate simulations. From this point of view the performed experiments have much less value. The set-up for the experiment S2 is also doubtful, because it is impossible to make any definite conclusions about the effects

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of the introduced changes from the series of very short runs with different parameters. The use of such short run gives an impression that the model is computationally very expensive and does not allow to perform necessary set of experiments. It would be useful if the authors mentioned what is wall clock time for 1 simulated year.

3. Meteorology

The illustration of the hydrological cycle can be easily eliminated to make the manuscript shorter and more chemistry oriented. If it is necessary the authors can just make a remark that higher resolution and nudging applied in E5/M1 does not change much the main features of the precipitation and WVC fields described in details by Hagemann et al. (2006). Anyway, the causes of the reported deficiencies (e.g. over Amazon area) are not clear and require additional studies.

Some illustration of the model dynamics (zonal wind for example) in the middle atmosphere would be appropriate taking into account that the section name is “meteorology”.

The authors do not discuss at all a very important problem of how robust the model results are. The comparison of the single experiment with observations cannot provide a solid basis for the assessment of the model performance. The both agreement and deviation of the model results from the observations should be evaluated to establish their statistical significance. It is very hard to make any conclusions just looking on Figure 5 where the results from short and different model experiments are presented.

The last two sentences in the section 3.2.1 are not supported by the presented results. What is the reason for the author’s suggestion that too cold tropical stratosphere and summer mesosphere (small part of which is only shown in Figure 5) is caused by nudging process? Did the authors consider the possibility that the radiation code of ECHAM-5 with only one interval in visible and UV spectral regions can lead to an underestimation of the solar heating rates in the upper stratosphere and mesosphere? How did the authors come to the conclusion that temperature is better for S2 simulation,

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if they do not directly compare the results from S1 and S2.

I do not think that the title of section 3.2.2 is appropriate because the B-D circulation and transport barriers are not really discussed. I guess, this title would make senses if the authors analyzed more appropriate quantities like TEM circulation, stream function and horizontal gradients of the species. In particular, it would be interesting to show residual vertical velocities in the lower stratosphere in comparison with observations, which would allow to judge whether the intensity of B-D circulation is reasonably good or not.

The question about the statistical significance of the model deviation from the observations remains for the results presented in section 3.2.2. Is the overestimation of the N2O mixing ratio in the lower stratosphere robust? It well could be that for some other year the model will underestimate N2O simply because of different state of the polar vortex. I think, that the assessment of the statistical significance or at least some discussion is absolutely necessary. It will be useful if the authors illustrate or describe also the reactive chlorines mixing ratio inside polar vortex. It is mentioned that they are underestimated, but their typical values were not shown.

Analyzing the N2O distribution the authors pointed out on some features related to the QBO. Would it be possible to explain what features exactly do they mean?

The section 3.2.4 describes interesting results, which suggest that the nudging in the troposphere provides necessary conditions to force major stratospheric warming in September 2002. However, some discussion of this process is necessary. First of all, it should be investigated using ensemble simulation how robust is this feature. It could be that its appearance is just a coincidence. It is also important to establish does this feature appear in the free running CCM. If this situation is unique, this opens a perspective to find the causes of this event in the tropospheric circulation.

Describing Figure 9 the authors mentioned slight underestimation of ozone depletion inside the vortex. However, Figure 8 shows that the total ozone inside the vortex is

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underestimated by about 100 DU, which cannot be qualified as small. In the last paragraph of section 3.2.4 the authors pointed out that the model reproduces the observed dehydration and denitrification inside the vortex showing HNO3 and NO2 fields. With my point of view some comments on this are necessary, because I do not readily see these in Figure 9. Probably, the H2O field could help.

4. Global ozone distribution and budgets

The model overestimates the magnitude of the total ozone during late winter and spring by about 10-15

The section 4.2 in the present form is unnecessary. I do not see how Figure 10 (without any comparison with observations) emphasizes consistent representation of ozone chemistry in the troposphere, stratosphere and mesosphere with no artificial boundaries. What do exactly the authors try to convey? Moreover, I cannot see in the Figure 10 all enlisted known features of the ozone distribution. All these features are somehow illustrated in the other sections, therefore the section 4.2 and Figure 10 can be eliminated.

5. Tropospheric tracers and chemistry

NOAA/CMDL data (Novelli et al., 1998) cover time period before 1998, while the simulated CO is appropriate for the year 2000. The same issue should be discussed concerning compilation of different campaigns data provided by Emmons et al. (2000). Therefore, I suggest adding some comments on this disagreement between the model results and the observation data used for the comparison. What is the role of artificial 40 percent reduction of the isoprene sources, does this reduction help to get reasonable CO distribution?

6. Stratospheric tracers and chemistry

The authors claimed that the results of S2 run is better than of S1 run, however, they did not compare them directly, therefore this conclusion is not supported by figures.

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7. Summary and conclusions

If the authors are willing to improve the manuscript, I suggest taking into account above-mentioned comments also in the section 7. Additionally, I would like to mention that the ozone in the mesosphere was not compared with observations at all.

Technical corrections

1. Page 6977, first paragraph: Figure 10 was wrongly introduced in the text. 2. It is necessary to decrease the number of figures in the section 5 and increase their size. In the present version it is really hard to see the lines even with magnifying glass.

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