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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Chemistry-climate model SOCOL: a validation of the present-day climatology" *by* T. Egorova et al.

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General comments

This work discusses the performance of a coupled chemistry climate model (CCM) and compares a simulation for current climate conditions with observational estimates. CCMs of the presented type are important tools to investigate the observed chemical and dynamical variability in the middle atmosphere, which is a relevant topic for ACP. The number of comparable model systems is still small due to the inherent difficulties.

Necessarily the focus of the manuscript is set on the simulated climatology and the differences to observations. The authors do not present scientifically new findings.

The introduction stresses the need to discuss the (statistical) significance of differences between simulated and observed quantities, which however is neglected in the evaluation in most parts. The paper could be improved significantly by using significance tests more thoroughly. Also it is necessary to discuss the identified differences between model and observations in a wider context than done now. The current manuscript neglects for example the effect of biases or simplifications in the chemical forcing, in the chemistry module or in the coupling of chemical, physical and dynamical processes. Feedbacks may be important to understand the biases.

Finally, in a number of places the authors discuss biases in the tropical middle atmosphere. This region cannot be discussed without considering the effects of the QBO. Currently the manuscript ignores the QBO completely.

The latter part of the manuscript on the AO seems a bit detached since this addresses mostly a dynamical phenomenon, for which chemical signals and feedbacks are weak. At the current stage of introducing a new CCM it would be more interesting to focus on the chemical processes, for example in the polar vortices, and to analyze the chemical properties of air masses, for example by tracer-tracer correlations.

Specific comments

Abstract

The authors highlights strongly the possibility to run the model on a PC. I think that this aspect does not deserve the weight given here. Other CCMs as well as the GCM used for the construction of SOCOL are running on PCs. This is possible due to the tremendous increase in computing power in the last couple of years. If this practical aspect were the most important one then the manuscript would not fit in the scope of ACP. I would strongly suggest to concentrate primarily on the scientific aspects.

Introduction

Models are simplifications not only by necessity, but also by purpose. CCMs as dis-

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cussed here include many simplifications and are far from including all known processes relevant in climate science (most CCMs neglect an ocean model component). This should be acknowledged in the manuscript which in the current form suggests that CCMs are including much more than they do.

Shindell et al. claim a delay that the maximum ozone destruction will be delayed due to the increased CO2 cooling, for the Antarctic as well as the Arctic.

The second paragraph (To increase model credibility ?) can be largely omitted without loss for the content of the paper. [The statements on the use of ensembles would have to be modified].

The third paragraph discusses the statistical issues of short observation time series and explains the importance of considering statistical significances. This should be replaced by a reference to a textbook to be more concise. The introduction of the term "hotspots" is not necessary, since it has no new meaning, and distracts the reader.

A disturbing problem is the fact that the presented work discusses significance values only for wind and temperature (Figures 3-5) so that it is not clear why the authors emphasize strongly the value of statistical significance tests. Further it should be pointed out that many important observations have been done with conventional observations (e.g. the ozone hole) or analyses (e.g. Berlin stratosphere analysis) in contrast to the assimilated products mentioned here. Though these modern analyses or re-analyses are very useful in general, they have there limitations as well (e.g. the residual circulation in the stratosphere in ERA40) and it is important to rely not entirely on them.

The paragraph related to the CCMVal project can be shortened strongly. Though CCMVal is a new project which will be important for the quantitative improvement of CCMs, the method of "validating" a GCM by studying the effects of particular processes is a standard approach and not new to the climate modelling community. It is sufficient to explain the aim of this particular project. The statement with reference to Baldwin (2000) is stronger than in the original work.

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5th paragraph: It is not clear to me why statistical significance tests are not done for the chemical species. Firstly, meteorological analyses and re-analyses are based mostly on the same raw data, so that the different products are not independent at all. Secondly, URAP has produced a valuable data set, but it is not the only one.

Section2

First paragraph: The current expansion or suggestive explanation of the MAECHAM4 acronym suggest a close relationship between the ECMWF and "Hamburg" models, which is misleading. The current IFS model of ECMWF actually no longer uses the dynamical core as still used in the ECHAM GCMs. Also the transport schemes and some parameterizations are different. For these reasons it is better to write for example:

MAECHAM4 is the middle atmosphere GCM developed at the MPI for Meteorology in Hamburg (Manzini et al., 1997; Charron and Manzini, 2002) based on the standard ECHAM4 GCM (Roeckner et al, 1996). The ECHAM GCMs evolve originally from the spectral weather prediction model of ECMWF (Simmons et al., Meteorol. Atmos. Phys., 40, 28-60, 1989).

At this place it should be mentioned that the MAECHAM4 GCM has been coupled previously to the CHEM module, validated in Steil et al. (J. Geophys. Res., 2003).

2.3: It is not completely clear if the transport scheme of MEZON replaces completely the transport scheme of MAECHAM4. How are the water vapour and cloud water transported in the coupled system? Is water vapour transported and processed identically in the whole model atmosphere? Is transport the only non-chemical process acting on the distribution of the chemical species other than water vapor? What about vertical "diffusion" or convective fluxes?

2.4: Already in a short while the technical specifications given here will be outdated. Therefore I think that these specifications should be part of an appendix.

A 40 year experiment is not equivalent to an ensemble of 40 integrations of 1 year

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each, if the initial conditions of the ensemble are independent of each other.

Is CO2 the only externally prescribed gas? The specifications of the lower boundary conditions should be explained in this manuscript because it is an important aspect of a chemistry-climate simulation.[The pdf-file of the referenced publication (Rozanov et al., JGR, 1999), as available on the JGR web site, is corrupted and hardly readable.]

Section 3

How are the data sets of Table 1 merged?

The ERA40 re-analysis of ECMWF extends to 0.1 hPa and covers the period 1960 to 2002, which could resolve to some degree the issue of averaging different time periods above and below 10 hPa. Why is ERA40 not used?

Section 4

"hotspots" is not really an adequate term for areas of significant differences.

4.1. is about monthly means, not annual means.

4.1.1: "? The model contains all main climatological features ...": The model does not include the QBO. This should be made clear from the very beginning since this is a major mode of internal variability, which has effects on the tracer distribution and chemistry. This cannot be neglected in the discussion of the model climatology in the tropical stratosphere and mesosphere.

4.1.2, second paragraph: a 40 year integration is not the same as an ensemble of 40 integrations of 1 years length. Figures 3 and 4 show differences of climatological monthly means. The tropical temperature bias (Figure 4) is not at the tropopause, but in the lower stratosphere. This cold bias and the warm bias above form a patterns that is typical for a missing QBO. The temperature bias in the upper tropical stratosphere may be the result of a biased semiannual oscillation.

4.1.3: The sensitivity of the temperature bias at the extratropical tropopause is dis-

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cussed in Roeckner et al., 2003 (MPI-Report 354) for the ECHAM5 GCM. The vertical resolution has no impact on the tropical temperature bias.

4.2: Statistical significance tests should be used also in the discussion of the chemicals.

4.2.1: Discussion of Figure 6: Methane seems to be low biased at most latitudes in January and July between about 2 and 20 hPa, and high biased above 20 hPa. What is the reason for that?

Discussion of Figure 7: The model shows a clear annual cycle in CH4 at the equator, which is not apparent in the observations. Here again the QBO may have an impact on the climatology or explain why the observations do not show annual variability.

Does the Brewer Dobson circulation determine the H2O mixing ratio at the tropical tropopause? Please add a reference. CH4 is low biased between 20 and 2 hPa. Is CH4 oxidation too efficient? The tropical tropopause is generally too cold (Figure 5), so that by simple arguments too low water vapour amounts would enter the tropical lower stratosphere, which would rather suggest a too strong oxidation, either because of a too high CH4 upward flux or a too efficient oxidation.

Discussion of Figure 9. The second sentence is not clear.

Figure 10. Steil et al. (2003) speculate that the missing QBO explains the exaggerated vertical ascent, which I can support from my own experience. The semiannual oscillation in UARS H2O has a significantly stronger first cycle, and hence a stronger difference between the first and second cycle in the model. The quantitative agreement is therefore rather limited.

Discussion of Figure 12: A too strong vortex, that is a dynamically undisturbed polar vortex would not necessarily combine with a strong descent in the polar vortex, would it? There seems to be a systematic negative bias in ozone mixing ratios in mid latitudes at the stratopause, in winter as well as in and summer, in January and July. What could

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be the reason, and what would be the radiative temperature effect of this bias?

Discussion of Figure 13. At the South Pole, the total ozone values drop steadily after March through the full winter, crossing 200 DU in July. What is the explanation? What is the role of heterogeneous chemistry?

4.3 Here a 25 year simulation is used. Is this a different simulation than that analyzed in the previous sections? Why is not the full 40 year experiment used in this section?

Second paragraph. The selection method for strong/weak vortex winters is not clear. Which are the precise criterions? Which months are considered? (The standard method is to compute the leading EOF and the related PC).

The use of significance tests is necessary for the interpretation of the figures 15 to 18.

Section 4.3 discusses the AO-related composite differences as it could be done equally well in a model without coupled chemistry (except for figure 18), for example with the atmospheric GCM of SOCOL, probably with nearly identical results. For that reason I would suggest to replace this section by an analysis relating more to the chemical processes or chemical properties of air masses, which is of much higher interest to the CCM community in the presentation of a new CCM.

Conclusions

The concluding remarks should take into account the possibility of shortcomings in the chemistry or in the occurrence of feedbacks leading to enhanced biases. The underestimation of stratopause temperature in summer hemisphere middle latitudes coincides for example with a negative bias in ozone that must contribute to too small heating rates.

Though the SW scheme does not consider the radiative properties of air in the spectral range shorter than 250nm, it should be noted that the solar constant describes the full irradiation. The flux in the range shorter than 250nm is mapped into the UV&visible band and will be absorbed (or scattered) as well. These processes, however, are rather

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occurring at lower levels than they should. Figure 19 therefore should be combined with a similar calculation that shows the effect of adding the radiative flux of the interval 120-250nm to the flux in the UV&visible band. The net effect therefore will be smaller than suggested by Figure 19.

Technical corrections, typos

2.1 and 2.2: A GCM is a model, not a module, the same linguistic problem occurs in "CTM module"

Figure 10: Positive contour intervals are 0.1 ppm, negative ones 0.05 ppm. This should be mentioned in the figure caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 509, 2005.