**Atmospheric
Chemistry
and Physics
Discussions**

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

T. Egorova et al.

Received and published: 22 April 2005

Answer to Anonymous Referee #2

"The authors claim to present a process oriented validation for a new chemistry-climate model. However, the authors fail to achieve that aim, because crucial processes and aspects of a thorough validation needed to judge the quality of a stratospheric chemistry climate model are missing."

We did not claim to present process oriented validation. We claim to "..present the description and validation of the present day climatologyĚ" and "Ěan example of process-oriented validationĚ" and achieved these aims. We suggest reviever#2 to reconsider this statement.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

EGU

"General Comments" "1. General:

Ţ There is no validation of variability of dynamical and chemical quantities (e.g., standard deviations, time series). The comparison of mean states only is not sufficient."

It is not helpful to analyze time series from the steady-state run simply because we do not have appropriate observations. The aim of this particular paper is to compare model climatology.

"Ţ Inclusion of tracer-tracer correlations is state of the art for validation papers."

This kind of validation is a part of process-oriented validation and worth a separate study. Many published papers devoted to CCM validation do not consider tracer-to-tracer correlations (the list is attached).

Al-Saadi et al., J. Geoph. Res., 109, D17301, 2004 Austin et al., 2003 (full reference is available from the paper) Hein et al., Ann. Geophys., 19, 435-457, 2001 Rozanov et al., 2001 (full reference is available from the paper) Takigawa et al., 1999(full reference is available from the paper)

"2. Chemistry:

Ţ There are no chemical budgets for stratospheric ozone; e.g., Crutzen and Brühl, 2001 (J. Phys. Chem. A)."

We do not see how to validate chemical budgets for stratospheric ozone, because of absence of direct measurements.

"Ţ Validations for nitrogen and chlorine species are missing."

We have added comparison of HCl with URAP data. URAP does not provide NOx climatology.

"Ţ There is nothing shown on PSCs."

There is no reference climatology for PSCs.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

EGU

"Ţ The selection of seasons is inadequate for validation of processes in high latitudes, polar spring is missing."

All seasons are represented in Figure 5, 7, 9, 14.

"3. Dynamics: There is no analysis on transport barriers by modern tools (e.g., probability density functions (PDFs, Sparling, 2000, JGR), or equivalent latitudes)"

This kind of validation is a part of process-oriented validation and worth a separate study. Many published papers devoted to CCM validation do not consider tracer-to-tracer correlations (the list of papers is the same as above).

"Specific Comments"

"1. page 515, line 7ff: Important source gases for chlorine and bromine are missing without explanation. The set of species differs also from Rozanov, 1999."

We have added an explanation. Why the set of species should be identical to Rozanov et al., 1999?

"2. page 516, lines 8/9: Are photolysis rates calculated without clouds (Rozanov 1999)? This is inadequate for the lower stratosphere and not state of the art."

Fixed clouds have been included for the calculation of look-up-tables. We cannot afford state-of-the-art Monte-Carlo approach to take into account complicated cloud structures.

"3. page 518, line 6ff: Some more details on tropospheric processes should be given (e.g. rainout, convective transport, dry deposition etc.)"

We have added some details on tropospheric processes.

"4. page 518, line 9ff: The promised standard deviations are not discussed."

We have changed this sentence.

"5. section 3: The construction of the observational data set from different reanalysis

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

data sets is misleading:

Ţ Those data sets are not statistically independent, because they cover approximately the same time period.

Ţ The choice of the years from the reanalysis data sets does not match the time slice experiment which should be representative for the 90's. The period from 1979-1989 should be excluded because of trends. Or the comparison should be done with a transient experiment."

We have excluded the period from 1979-1990. We do not apply Student t-test using just a criteria based on the standard deviations.

"6. page 519, 4.1 heading: Does not fit to content, there is no annual mean just monthly means."

We have changed the heading.

"7. pages 519, 520, and Fig. 1, Fig. 3: arbitrary selection of the wind observations to beautify the results."

Can you justify this statement? Why to beautify if all months are shown in Figure 5 anyway.

"8. page 523, line 3, Fig. 6: Text and figure differ; the differences in the figure are larger."

We have checked the text.

"9. page 523, section 4.2.2, Fig. 8: There is a systematic positive bias between model and HALOE water vapor, which cannot be put into perspective to the HALOE accuracy. The HALOE error is a statistical error (+/-)."

We think, that if , for example, the declared accuracy is ś10% , then 5% deviation of the model from the data cannot be considered as significant.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

EGU

"10. page 526, line 1ff, Fig. 13: "The position and magnitude of the ozone hole is very well reproduced ...". This is not true and in contradiction to Fig. 13. By far the most of the ozone decrease appears to be related to the downward transport of the wrong ozone of the upper and middle stratosphere during polar night (Fig. 12). Heterogeneous ozone destruction during polar night is not possible (O3 $<$ 200 DU in July!). Obviously this model does not need PSCs for an ozone hole."

This statement is not true. It is clear from the Figure 14 (now Figure 15) that the position and magnitude of the ozone hole in October is well reproduced by the model. Without heterogeneous chemistry on PSC's it would not be possible. It is confirmed by the results of our short-term model runs without heterogeneous chemistry and transient run for 1975, when the ozone hole did not appear (not shown in the paper) .

"11. page 527, line 2ff: Why are only 25 years used for this analysis and not the complete 40-year simulation? What is the role of the sea surface temperature?"

We have changed the figures using 40 years of simulation.

"12. Fig. 5,7,9,10: The figures do not show two consecutive years as suggested in caption of figure 10, but the same year (average ?) twice. Just copying the mean seasonal cycle does not give additional information and is misleading especially in the case of the tape recorder. Are these figures based on monthly means?"

These figures are based on monthly means. We have change the caption for Figure 10. In Figure 5,7,8,10 we presented 2 years to show the transition months more clearly. We do not see why it could be misleading.

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

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

EGU