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

Anonymous Referee #2

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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. Those key aspects are:

General Comments

1. General:

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

2. Chemistry:
   - There are no chemical budgets for stratospheric ozone; e.g., Crutzen and Brühl, 2001 (J. Phys. Chem. A).
   - Validations for nitrogen and chlorine species are missing.
   - There is nothing shown on PSCs.
   - The selection of seasons is inadequate for validation of processes in high latitudes, polar spring is missing.
3. Dynamics: There is no analysis on transport barriers by modern tools (e.g., probability density functions (PDFs, Sparling, 2000, JGR), or equivalent latitudes)

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.
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.
3. page 518, line 6ff: Some more details on tropospheric processes should be given (e.g. rainout, convective transport, dry deposition etc.)
4. page 518, line 9ff: The promised standard deviations are not discussed.

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5. section 3: The construction of the observational data set from different reanalysis data sets is misleading:

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

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

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

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

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 (+/-).

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 ($O_3 < 200$ DU in July!). Obviously this model does not need PSCs for an ozone hole.

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?

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Interactive Comment

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?

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